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OTHER DEPARTMENTS

Number 66 of a series of photographs of past presidents of the Association

George J. Stigler

The American Economic Review

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THE ECONOMIST AND THE STATE*

By GEORGE J. STIGLER

In 1776 our venerable master offered clear and emphatic advice to his countrymen on the proper way to achieve economic prosperity. This advice was of course directed also to his countrymen in the American colonies, although at that very moment we were busily establishing what would now be called a major tax loophole. The main burden of Smith's advice, as you know, was that the conduct of economic affairs is best left to private citizens—that the state will be doing remarkably well if it succeeds in its unavoidable tasks of winning wars, preserving justice, and maintaining the various highways of commerce.

That was almost two centuries ago, and few modern economists would assign anything like so austere a role to the economic responsibilities of the state. The fact that most modern economists are as confident in prescribing a large economic role to the state as Smith was in denying such a role is not necessarily surprising: professional opinions sometimes change after 188 years, and economic and political institutions are of course even less durable.

But, surprising or not, the shifts in the predominant views of a profession on public policy pose a question which I wish to discuss. That question is: on what basis have economists felt themselves equipped to give useful advice on the proper functions of the state? By what methods did Smith and his disciples show the incapacity of the state in economic affairs? By what methods did later economists who favored state control of railroads, stock exchanges, wage rates and prices, farm output, and a thousand other things, prove that these were better directed or operated by the state? How does an economist acquire as much confidence in the wisdom of a policy of free trade or fiscal stabilization as he has in the law of diminishing returns or the profit-maximizing propensities of entrepreneurs?

The thought behind these questions is simple. Economists generally share the ruling values of their societies, but their professional competence does not consist in translating popular wishes into an awe-inspir-

* Presidential address delivered at the Seventy-Seventh Annual Meeting of the American Economic Association, Chicago, December 29, 1964.

ing professional language. Their competence consists in understanding how an economic system works under alternative institutional frameworks. If they have anything of their own to contribute to the popular discussion of economic policy, it is some special understanding of the relationship between policies and results of policies.

The basic role of the scientist in public policy, therefore, is that of establishing the costs and benefits of alternative institutional arrangements. Smith had no professional right to advise England on the Navigations Acts unless he had evidence of their effects and the probable effects of their repeal. A modern economist has no professional right to advise the federal government to regulate or deregulate the railroads unless he has evidence of the effects of these policies.

This position, you must notice, is not quite the familiar one that an economist's value judgments have no scientific status—indeed I shall neither dispute nor praise value judgments. The position is rather that if a subject is capable of study, a scholar ought to study it before he advises legislators. Suppose you deplore disease or, conversely, that you greatly admire the much-persecuted germ. My assertion is that however you stand, you should not support proposals to compel or to forbid people to go to a doctor until you find out whether their attendance on a doctor will increase or decrease the incidence of disease. If this particular example strikes you as absurdly pedantic, I offer two responses. First, will your answer be the same whatever the state of medical science in a country? Second, we shall come to harder problems.

My task, then, is to ask in as hardheaded a way as possible what precisely was the evidence economists provided for their policy recommendations, evidence that successfully linked their proposals with the goals they were seeking to achieve. I begin with Adam.

I

Smith bases his proposals for economic policy upon two main positions. Neither basis is presented in a formal and systematic fashion, and there are serious problems in determining exactly why he wishes most economic life to be free of state regulation.

Smith's first basis for his economic policies was his belief in the efficiency of the system of natural liberty. There can be little doubt that this tough-minded Scotsman, this close friend of that cool and clear thinker, David Hume, had a deep attachment to the natural law of the late enlightenment. But Smith did not propose natural liberty as a lay religion of political life. Instead he argued, as a matter of demonstrable economic analysis, that the individual in seeking his own betterment will put his resources where they yield the most to him, and that as a rule the resources then yield the most to society. Where the individual

does not know, or does not have the power to advance, his own interests, Smith feels remarkably free to have the state intervene.

Thus Smith says that to restrain people from entering voluntary transactions "is a manifest violation of that natural liberty which it is the proper business of law, not to infringe but to support"; yet he continues [11, p. 308]:

> But those exertions of the natural liberty of a few individuals, which might endanger the security of the whole society, are, and ought to be, restrained by the laws of all governments; of the most free, as well as of the most despotical. The obligation of building party walls, in order to prevent the communication of fire, is a violation of natural liberty, exactly of the same kind with the regulations of the banking trade which are here proposed.

Natural liberty seems to have been little more than a working rule, and Smith proposes numerous departures from natural liberty because the participants are incompetent or fail to consider external effects of their behavior.[1] He is quite willing to outlaw payment of wages in kind, which he believes will defraud the worker, and to put a limit on interest rates, because high interest rates encourage lenders to entrust their funds to improvident projectors, and to have a complicated tax system to change the uses of land.

The second foundation of Smith's strong preference for private economic activity was that he deeply distrusted the state. This distrust, I must emphasize, was primarily a distrust of the motives rather than of the competence of the state. Smith makes very little of inept governmental conduct—indeed he clearly believes that as far as efficiency is concerned, the joint stock companies, and even more the universities, are worse offenders than the state. His real complaint against the state is that it is the creature of organized, articulate, self-serving groups—above all, the merchants and the manufacturers. The legislature is directed less often by an extended view of the common good than by "the clamorous importunity of partial interests" [11, p. 438].

Purely as a matter of professional appraisal, I would say that Smith displayed superb craftsmanship in supporting his first argument—that free individuals would use resources efficiently—but was excessively dogmatic in asserting his second argument, which accepted the competence but rejected the disinterest of the governmental machine. He gives no persuasive evidence that the state achieves the goals of its policies, and in particular he asserts rather than proves that the mercantile system had a large effect upon the allocation of British resources. Nor does he demonstrate that the state is normally the captive of "partial interests."

Smith's intellectual heirs did little to strengthen his case for *laissez*

[1] See the essay by Viner, "Adam Smith and Laissez Faire," in *Adam Smith 1776-1926* (University of Chicago, 1928).

faire, except by that most irresistible of all the weapons of scholarship, infinite repetition. Yet they could have done so, and in two directions.

Where Smith finds the competitive market incapable of performing a task, they might have corrected him, for he was sometimes wrong. To a degree this was done: Smith's belief that the market set too low a value on investment in agriculture, and too high a value on foreign investment, was properly criticized by McCulloch [6, pp. 144 ff.], and the aberration on usury was of course promptly challenged by Bentham. But for each of Smith's errors that was corrected, several new ones were introduced. J. S. Mill, for example, gravely argued that the competitive market was incapable of providing a reduction in the hours of work even if all the workers wished it—a mistake I am not inclined to excuse simply because so many later economists repeated it.

What I consider to be a more important weakness in Smith's position, however—his undocumented assumption that the state was efficient in achieving mistaken ends[2]—was not only accepted, but emphatically reaffirmed by his followers. James Mill's identification of the evils of government with the undemocratic control of its instruments was an extreme example, but an instructive and influential one. The holder of the power of government would always use it to further his own ends—so argued Mill with an oppressive show of logical rigor. It followed that only a democratically controlled state would seek the good of the entire public:

> The Community cannot have an interest opposite to its interest. To affirm this would be a contradiction in terms. The Community within itself, and with respect to itself, can have no sinister interest . . . The Community may act wrong from mistake. To suppose that it could from design would be to suppose that human beings can wish their own misery.[3]

Hence a democracy, unlike a monarchy or an aristocracy, would do no unwise thing except in ignorance. And this exception for ignorance was not a serious one:

> There can be no doubt that the middle rank, which gives to science, to art, and to legislation itself, their most distinguished ornaments, and is the chief source of all that has exalted and refined human nature, is that portion of the Community of which, if the basis of Representation were ever so far extended, the opinion would ultimately decide. Of the people beneath them, a vast majority would be sure to be guided by their advice and example.[4]

[2] McCulloch, a somewhat underrated man, again challenged Smith here; see "Navigation Laws," *Edinburgh Review*, May 1823.

[3] *The Article on Government* (reprinted from the Supplement to the *Encyclopædia Britannica* [London 1829]), p. 7.

[4] *Ibid.*, p. 32.

Education of the masses, and their instinctive reverence for the wisdom of their middle class leaders, those ornaments of society, would thus insure that the democratic state would seldom stray far from the public good. The argument meant that at the time the essay was written the American government was a reliable instrument of public welfare and 50 years later England's government would become so.[5]

It would be possible to document at length this proposition that the classical economists objected chiefly to *unwise* governmental intervention in economic life, but I shall give only two instructive examples.

The first example is provided by that fine Irish economist, Mountifort Longfield. Apropos of certain dubious programs to assist the laborer he wrote, "here Political Economy is merely a defensive science, which attempts to prevent the injudicious interference of speculative legislation" [3, p. 18]. This sounds suitably conservative, but let us continue. Years later, as a witness before a Royal Commission on Railways, he complained that his timid fellow directors of the Great Southern and Western Railway underestimated the long-run elasticity of demand for rail service. To produce the necessary courage he proposed that the government appoint a director with unlimited power to vary the rates of each railroad, with the government taking half of any resulting profits and compensating all of any resulting losses.[6] Longfield wanted not *laissez faire* but half fare.

The second example is the major controversy provoked by the campaigns for the ten-hour day for women in factories, which reached success in 1847. This was one of the first of the modern English interventions in the contracts of competent adults, and it invited excommunication by the economic divines. This Factory Act was in fact opposed with vigor by two important economists, Torrens and Senior, but explicitly *not* as a violation of natural right. Torrens prefaced his criticism with a passage that reads better than it reasons:

> The principle of non-interference can be applicable to those circumstances only, in which interference would be productive of mischief; in

[5] Mill's essay elicited a brilliant attack by Macaulay, who turned Mill's argument that every man seeks only his own interests against the plea for universal suffrage:

> That the property of the rich minority can be made subservient to the pleasures of the poor majority will scarcely be denied. But Mr. Mill proposes to give the poor majority power over the rich minority. Is it possible to doubt to what, on his own principles, such an arrangement must lead?

The argument is carried to an interesting prediction: "As for America, we appeal to the twentieth century," "Mill's Essay on Government," in *Critical, Historical and Miscellaneous Essays* (New York 1873), II, pp. 36-37, 40.

[6] Royal Commission on Railways, *Evidence and Papers Relating to Railways in Ireland* (1866), pp. 126-30, 359-60.

> all those cases in which the interference of the central authority in the transactions between man and man is capable of effecting good or averting evil, *laissez faire* is a criminal abandonment of the functions for the performance of which a central authority is established and maintained.[7]

Hence Torrens, and equally Senior,[8] criticized the ten-hour bill because it would lower weekly wages, increase production costs, and reduce employment by impairing the competitive position of the British textile industry abroad.

Both Senior and Torrens died in 1864, so they had adequate time, one would think, to have tested their predictions of the effects of the ten-hour law. It is wholly characteristic of the insulation of discussions of policy from empirical evidence that no such study was undertaken by them, or by anyone else.

James Mill's oldest son, surprisingly enough, put up a stronger case against state control of economic life than his much more conservative father had. John Stuart did not follow his father in accepting the invariable wisdom of the democratic state, possibly because he was writing well after the Reform Act.[9] He rested the case much more on the defense of individual liberty, and fully three of the five reasons he gave for favoring *laissez faire* as a practical maxim were variations on the importance of the dignity, independence, self-reliance, and development of the individual [7, Bk. V, Ch. 11].

Although I reckon myself among the most fervent admirers of individualism, even for other people, I must concede that the younger Mill's position was ambiguous. He does not tell us how to determine whether a given public policy frees or inhibits individuals. Suppose I contemplate a program of public housing. If I bribe or force people into such housing, of course I have reduced their area of choice and responsibility. But I have also, I presumably hope, given a generation of children a chance to grow up in quarters that are not grossly unsanitary and inadequate for physical and moral health. Mill does not tell us whether this policy fosters or inhibits individualism—although I strongly suspect that he would have favored public housing, as he did free public education and limitation of hours of work for young people. If an economist is to be a moral philosopher, however—and I have no doubt that we would do this well too—he should develop his philosophy

[7] *A Letter to Lord Ashley* (London 1844), pp. 64-65.

[8] *Letters on the Factory Act* (London 1844).

[9] He did make some reference to the incompetence of state action: ". . . the great majority of things are worse done by the intervention of government, than the individuals most interested in the matter would do them, or cause them to be done, if left to themselves" [7, II, p. 511]. This argument does not play a major role in shaping his attitude, however.

to a level where its implications for policy become a matter of logic rather than a vehicle for expressing personal tastes.[10]

Let us leap on to Marshall who brought up the rear of this tradition as of so many others in English economics. He conceded an expanding potential role to the state, in the control of monopoly, in the housing of the poor, and in the treatment of poverty generally. Yet he persevered in his preference for private enterprise wherever possible. The preference rested heavily on the belief that bureaucratic management would be burdensome and inefficient.[11] Marshall at this point wrote the boldest sentence of his life:

> If Governmental control had supplanted that of private enterprise a hundred years ago [1807], there is good reason to suppose that our methods of manufacture now would be about as effective as they were fifty years ago, instead of being perhaps four or even six times as efficient as they were then.[12]

Yet the "good reason" was never presented, although it was more important to demonstrate this proposition if true than to answer any other question to which Marshall devoted a chapter or a book or even his life. Marshall's other reason for his distrust of government was the fear that Parliament would become the creature of special interests, and in particular of the Trade Unions[13]—an unknowing but not unknowledgeable reversion to Adam Smith!

So much for a century of *laissez faire*. The main school of economic individualism had not produced even a respectable modicum of evidence that the state was incompetent to deal with detailed economic problems of any or all sorts. There was precious little evidence, indeed, that the state was unwise in its economic activities, unless one was pre-

[10] Mill's famous essay, *On Liberty*, does little to reduce our uncertainty. It is here that he asserts:

> Despotism is a legitimate mode of government in dealing with barbarians, provided the end be their improvement, and the means justified by actually effecting the ends.
>
> The laws which, in many countries on the Continent, forbid marriage unless the parties can show that they have the means of supporting a family, do not exceed the legitimate powers of the State. . . .
>
> As the principle of the individual liberty is not involved in the doctrine of Free Trade . . . (*The English Philosophers from Bacon to Mill* [Modern Library 1939], pp. 956, 1035, 1024).

It is not easy to avoid the conclusion that for Mill "liberty" was conveniently well correlated with the forms of behavior of which he personally approved.

[11] *Memorials of Alfred Marshall* (1925), pp. 274-76, 339 ff.; *Industry and Trade* (1919), pp. 666-72.

[12] *Memorials*, p. 338.

[13] *Official Papers by Alfred Marshall* (1926), pp. 395-96.

pared to accept as evidence selected corollaries of a general theory. The doctrine of nonintervention was powerful only so long and so far as men wished to obey.

II

There was no day on which economists ceased to commend reductions in the government's role in economic life and began to propose its expansion. The limitation of hours of work for children was supported well before the attack on the corn laws reached its climax. The statutes liberalizing dealings in property in the 1830's followed at a distance the regulation of passenger ships to protect emigrants.

How else could it be? The distinction between ancient police functions admitted by all and new regulatory functions proposed by some was most elusive. The same economist could and did repel the state with one hand and beckon it with the other.[14]

The expansion of public control over economic life which took place in the mid-nineteenth century in England, and a trifle later in the United States, was usually of this sort: a traditional state function was expanded or a new function was adopted which had close analogies to traditional functions. Economic effects were usually incidental to protective effects: the inspection of factories and mines, the sanitation laws for cities, the embryonic educational system, and most of the controls over railroads were of this sort [9] [4].

One thing did not change at all, however, from the heyday of *laissez faire:* no economist deemed it necessary to document his belief that the state could effectively discharge the new duties he proposed to give to it. The previous assertions of governmental incompetence were met only by counter assertion; the previous hopes of wiser uses of governmental powers by a democracy were deemed too prophetic to deserve the discourtesy of historical test. I shall illustrate this persistent neglect of empirical evidence with the writings of two economists who have almost nothing in common except great ability.

The first is Jevons. Governmental operation of an industry was appropriate, Jevons believed, if four conditions were fulfilled: (1) The work must be of an invariable and routine-like nature, so as to be performed according to fixed rules. (2) It must be performed under the public eye, or for the service of individuals, who will immediately detect and expose any failure or laxity. (3) There must be very little capital expenditure, so that each year's revenue and expense account

[14] Thus McCulloch said of the post office: "It does not seem, though the contrary has been sometimes contended, that the Postoffice could be so well conducted by anyone else as by government: the latter alone can enforce perfect regularity in all its subordinate departments . . ." (*Dictionary of Commerce* (1854 ed.), article on "Postage").

shall represent, with approximate accuracy, the real commercial success of the undertaking. (4) The operations must be of such a kind that their union under one all-extensive Government monopoly will lead to great advantage and economy [1, pp. 355, 279, 338]. On what is this garbled description of a municipal water system based?—mature introspection, of course.

Jevons is equally devoted to the a priori method when he discusses public regulation. The "Principles of Industrial Legislation" are illustrated first with the problem posed by a dangerous machine. Neither worker nor employer, Jevons says, generally displays due concern for the dangers that lurk in the unfenced machine.

> But there remains one other mode of solving the question which is as simple as it is effective. The law may command that dangerous machinery shall be fenced, and the executive government may appoint inspectors to go round and prosecute such owners as disobey the law [2, p. 4].

Several aspects of Jevons' position are instructive. There is no showing of evidence on the failure of employers and employees to curb dangerous machinery. There is no showing of evidence that direct controls are simple and effective. Direct controls surely were not effective in factories too small to catch the inspector's eyes, and it is a completely open question whether they were effective elsewhere. And finally, Jevons does not conceive of the possible role of the price system in supplementing, if not replacing, direct inspection by a law making employers responsible for accidents.[15]

But let us recall who Jevons was; he was the economist whose supreme genius lay in his demand for empirical determination of theoretical relationships and his immense resourcefulness in making such determinations. This powerful instinct for empirical evidence spilled over into a proposal that wherever possible new policies should first be tried out at the local governmental level: "we cannot," he said, "really plan out social reforms upon theoretical grounds."[16] But, possible or not, he really so planned out his reforms.

We may learn how a theorist coped with the problem by turning to my second economist, Pigou. In *Wealth and Welfare* [8] he recited four reasons for distrusting the ability of legislatures to control mo-

[15] It should be a source of morbid instruction to us, that immediately after laying down this dogmatic rule on how to treat with dangerous machinery, Jevons denounces those who view the economist as a "presumptuous theorist, who is continually laying down hard-and-fast rules for the conduct of other people" [2, p. 8].

[16] "Experimental Legislation and the Drink Traffic," *The Contemporary Review*, 37, 1880, 192 (reprinted in *Methods of Social Reform*, p. 275). He did not see the potentialities of empirical study in the absence of formal experiment, however, and denied the feasibility of a statistical approach ("Experimental Legislation," pp. 184-85).

nopolies. They were shallow reasons, but what is instructive is that all of them "can be, in great measure, obviated by the recently developed invention of 'Commissioners,' that is to say, bodies of men appointed by governmental authorities for the express purpose of industrial operation or control." Hence the government is now capable of "beneficial intervention in industries, under conditions which would not have justified such intervention in earlier times" [8, p. 250].

If time were not the most precious thing that one professor can give to another, I would follow in detail Pigou's travels from this inauspicious beginning. We would be instructed by the evidence which he found sufficient to a series of propositions on the state's competence:

> . . . laws directly aimed at "maintaining competition" are practically certain to fail of their purpose [8, p. 253].
>
> . . . *in respect of industries, where the quality of the output is of supreme* importance and would, in private hands, be in danger of neglect, public operation is desirable [8, p. 288].
>
> . . . the relative inefficiency of public operation, as compared with private operation, is very large in highly speculative undertakings, and dwindles to nothing in respect of those where the speculative element is practically non-existent.[17]

The evidence, you will hardly need be reminded, consisted of a few quotations from books on municipal trading.

Pigou's views of the competence of the state were, like his predecessors' views, a tolerably random selection of the immediately previous views, warmed by hope. He felt that reliance upon such loose general reflections was unavoidable. On the question of whether public or private operation of an industry would be more efficient in production, we are told "at the outset, it must be made clear that attempts to conduct such a comparison by reference to statistics are fore-doomed to failure" [8, p. 274]. How is it made clear? Very simply: by pointing out that it is unlikely that a public and a private enterprise operate under identical conditions of production. This test of the feasibility of statistical research would rule out all such research, and of course Pigou throughout his life accepted this implication.

Let me say that Pigou did not differ from his less illustrious colleagues in the superficiality of his judgments on the economic competence of the state—here he was at least as shrewd and circumspect as they. He differed only in writing more pages of economic analysis of fully professional quality than any other economist of the twentieth century.

Rather than sample other economists, I shall characterize more gen-

[17] The maturing fruit in a later edition; *The Economics of Welfare* (4th ed., 1932), p. 399.

'ally their role in the period of growing state control over economic fe. The traditional and inevitable economic functions of the state ıch as taxation and the control of the monetary system are not condered in the following remarks. These functions pose no question of ıe desirability of state action and very different questions of the econnist's role in policy. On the basis of a highly incomplete canvass of ıe literature, I propose three generalizations.

First, there was a large and growing range of policy issues which :onomists essentially ignored. If we examine the English legislation overning shop closing hours, or pure food and drug inspection, or muicipal utilities, or railway and truck and ocean transportation, or the gal status of labor unions, or a host of other questions, we shall find ıat as a rule economists did not write on the issue, or appear before ıe Royal Commissions, or otherwise participate in the policy formulaon. Before 1914 the detachment from contemporary policy was Olymian, thereafter it was mortal but awesome. American economists, peraps reflecting their Germanic training, were more interested in policy,) one can cite examples like John R. Commons on regulation of public tilities and on workmen's compensation laws, J. B. Clark and a host of :hers on the trust problem, and so on. Even here, however, many imortant economic policies were (and are still) ignored, among them ure food laws, wage legislation, fair employment practices acts, the oning of land uses, and controls over the capital markets.

Second, even when economists took an active and direct interest in policy issue, they did not make systematic empirical studies to esıblish the extent and nature of a problem or the probable efficiency of lternative methods of solving the problem.

It is difficult to support allegations about the absence of a given type f scientific work; often the allegation illuminates only the reading abits of its author. I am reasonably confident, however, that the folowing subjects were not investigated with even modest thoroughness: 1) the effects of regulation on the level and structure of prices or rates f public utilities; (2) the extent to which safety in production processes and purity in products are achieved by a competitive market and y a regulatory body; (3) the cost to the community of preventing ilures of financial institutions by the route of suppressing competion compared with the costs by the route of insurance; (4) the effects f price support systems for distressed industries upon the distribution f income, as compared with alternative policies; and (5) the effects f policies designed to preserve competition. This list is short, but I ıbmit that the examples are important enough to give credence to my eneralization on the paucity of systematic empirical work on the techiques of economic policy. From 1776 to 1964 the chief instrument

of empirical demonstration on the economic competence of the state has been the telling anecdote.

Third, the economist's influence upon the formulation of economic policy has usually been small. It has been small because he lacked special professional knowledge of the comparative competence of the state and of private enterprise. The economist could and did use his economic theory, and it cannot be denied that the economist's economic theory is better than everyone's else economic theory. But for reasons to which I shall immediately turn, economic theory has not been an adequate platform. Lacking real expertise, and lacking also evangelical ardor, the economist has had little influence upon the evolution of economic policy.

III

If economists have lacked a firm empirical basis for their policy views, one might expect that guidance could be derived from their theoretical systems. In fact, to the degree that a theoretical system has been submitted to a variety of empirical tests, it is a source of more reliable knowledge than an empirical uniformity in solitary confinement. The theory allows tests of the relationship incorporated in the theory that are outside the view of the discoverer of the theory, so these tests are more challenging.

The economists' policy views have in fact been much influenced by their theories. The vast preference for free international trade is surely based in good part upon the acceptance of the classical theory of comparative costs. The general presumption against direct regulation of prices by the state is surely attributable in good part to the belief in the optimum properties of a competitive price system. The growth of support among economists for public regulation of economic activities is at least partly due to the development of the theory of disharmonies between private and social costs, and partly also to the increasingly more rigorous standards of optimum economic performance.

If it would be wrong to deny a substantial influence of economic theory on economists' policy views, it would be wronger still to suggest that the policies follow closely and unambiguously from the general theory. Our first example of free trade will suffice to illustrate the looseness of the connection. Smith supported free trade because he believed that tariffs simply diverted resources from more productive to less productive fields, and the absence of an explanation for the rates of exchange between foreign and domestic commodities did not bother him. A century later Sidgwick argued that on theoretical grounds tariffs were often beneficial to a nation, but that "from the difficulty of securing in any actual government sufficient wisdom, strength, and singleness of aim to introduce protection only so far as it is advanta-

geous to the community" the statesman should avoid protective duties [10, pp. 485-86]. To the extent that theory was guiding Sidgwick, surely it was a theory of government rather than of economics.

There is one primary reason why the theory is not, as a rule, coercive with respect to the policies that a believer in the theory must accept: a theory can usually be made to support diverse policy positions. Theories present general relationships, and which part of a theory is decisive in a particular context is a matter of empirical evidence. Consider the wages-fund doctrine, if I may be permitted to refer to it without its almost inseparable prefix, notorious. This theory asserted that there was a relatively fixed amount to be paid in wages in the short run, and that if one group got higher wages, other groups would get lower wages or be unemployed. It followed that if a particular group of workers formed a union and managed to raise their wages, other workers would bear the burden, and numerous disciples of the wages-fund doctrine accepted this policy view.[18] But John Stuart Mill could argue, quite in the opposite direction, that since most workers would be at a subsistence level, at most the successful union would inflict only short-run harm on other workers, whereas its higher income could be permanent.[19] And obviously it is a quantitative question whether the short-run costs or the permanent benefits were larger.

What is true of the wages-fund theory is true of other theories: an empirical question always insists upon intruding between the formal doctrine and its concrete application. The truly remarkable fact is not that economists accepting the same theory sometimes differ on policy, but that they differ so seldom. The wide consensus at any time comes, I suspect, from a tacit acceptance of the same implicit empirical assumptions by most economists. All classical economists accepted as a fact the belief that wage earners would not save, although they had no evidence on the matter. All modern economists believe they will never encounter Edgeworth's taxation paradox, with no more evidence. All economists at all times accept the universality of negatively sloping demand curves, and they do so without any serious search for contrary empirical evidence.

These empirical consensuses have no doubt usually been correct—one can know a thing without a sophisticated study. Truth was born before modern statistics. Yet generations of economists also believed that over long periods diminishing returns would inevitably triumph over technological advance in agriculture, a view that agricultural history of the last 100 years has coolly ignored.

A second and lesser source of the loose connection between theory

[18] For example, J. E. Cairnes, *Some Leading Principles of Political Economy* (London 1873), pp. 258-60.

[19] *Principles of Political Economy*, Ashley ed. (London 1929), p. 402.

and policy has been the difficulty of translating theory into policy because of practical politics or administration. The economist refrains from drawing a policy conclusion because its implementation would pose large social or administrative costs. Mill dismissed an income tax because of the inquisitorial burdens it would put on taxpayers; one would have thought that he would remember that an earlier inquisition had been welcomed to Spain. For at least 100 years economists have recommended that a nation proceed to free trade gradually over a five-year period to ease the transition, and the period is usually lengthened if protectionism is on the ascendant. I have often wondered why we deem it necessary to tell a confirmed drunkard not to reduce his drinking too rapidly.

A third, and fortunately a moderately rare, reason for separating theory from policy is flagrant inconsistency, usually stemming from that great source of inconsistency in intelligent men, a warm heart. Marshall proved—rather unconvincingly, I must say—that the doctrine of consumer surplus instructed us to tax necessaries rather than luxuries [5, p. 467 n.]. The idea was disposed of in a footnote because it disregarded ability to pay. The economic arguments against minimum wage legislation have usually been refuted by reference to the need of poorer people for larger incomes.

The essential ambiguity of general theoretical systems with respect to public policy, however, has been the real basis of our troubles. So long as a competent economist can bend the existing theory to either side of most viable controversies without violating the rules of professional work, the voice of the economist must be a whisper in the legislative halls.

IV

The economic role of the state has managed to hold the attention of scholars for over two centuries without arousing their curiosity. This judgment that the perennial debate has refused to leave the terrain of abstract discourse is true, I believe, of the continental literature as well as the English and American literature. Economists have refused either to leave the problem alone or to work on it.

Why have not the effects of the regulatory bodies on prices and rates been ascertained, even at the cost of a 1 per cent reduction in the literature on how to value assets for rate purposes? Why have not the effects of welfare activities on the distribution of income been determined for an important range of such activities, even at the cost of a 1 per cent reduction in denunciations of the invasion of personal liberty? Why has not the degree of success of governments in bringing private and social costs together been estimated, even at the cost of a 1 per

cent reduction in the literature on consumer surplus? Why have we been content to leave the problem of policy unstudied?

This variously phrased question can be considered to be a request for either a formal theory of state action or a set of empirical studies of the comparative advantages of public and private control.

Consider first the control over economic life as a formal theoretical problem. Why do we not have a theory to guide us in ascertaining the areas of comparative advantage of uncontrolled private enterprise, competitive private enterprise, public regulation, public operation, and the other forms of economic organization? This theory would predict the manner in which the state would conduct various economic activities, such as protecting consumers from monopoly or fraud, assisting distressed industries and areas, or stimulating inventions. The theory might yield rules such as that a competitive system is superior for introducing new products, or public enterprise is superior where there are many parties to a single transaction. That we have not done so is attributable, I conjecture, to two difficulties.

The first difficulty is that the issue of public control had a constantly changing focus: it was the relations of labor and employers one year the compensation to tenants for improvements on farms and the control of railroad rates the year thereafter. At any one time few areas of economic life were seriously in dispute: most economic activities were uncontroversially private or public. That a single theory should be contrived to guide society in dealing with these various and changing problems was perhaps too great an abstraction to encourage serious efforts.

Moreover, and this is the second difficulty, the standard apparatus of the economist is not clearly appropriate. Ordinary maximizing behavior, with the ordinary rewards and obstacles of economic analysis does not seem directly applicable to the problem. The bounds of state competence, and the areas of its superiority over variously controlled private action, are difficult to bring within a coherent theoretical system.

In short, the theory of public policy may be a difficult theory to devise, although until we have tried to devise it even this opinion is uncertain.

A usable theory of social control of economic life was not essential however, to professional study of policy: could not the economist make empirical studies of the effects of various ways of dealing with specific problems? The state regulates machinery in factories: does this reduce accidents appreciably? The state regulated the carriage of emigrants from England and Ireland to the new world—what did the regulation achieve? A thousand prices had been regulated—were they lower o

stickier than unregulated prices? The empirical answers would obviously have contributed both to public policy and to the development of a general theory of public and private economy.

Here we must pause, not without embarrassment, to notice that we could ask for empirical studies in areas traditional to economics as well as in the netherland of half economics, half political science. We need not be surprised, I suppose, that we know little of the effects of state regulation, when we also know very little about how oligopolists behave. Marshall's theory that the differences between short- and long-run prices and profits are regulated by the differences between short- and long-run reactions of supply will be 75 years old next year. Despite its immense influence, this theory has yet to receive a full empirical test. If such basic components of modern economic theory have escaped tests for quantitative significance, it is hardly surprising that our antitrust laws, our motor carrier regulation, and our control of insurance company investments have also escaped such tests.

Still, there has been a difference. Empirical tests of economic theories have been made for generations, and with greater frequency and diligence than we encounter in the area of social experiments. Already in 1863 Jevons had ascertained the serious fall in the value of gold consequent upon the Californian and Australian gold discoveries—it was 26 per cent over the 13-year period, 1849-62. No such diligence or ingenuity can be found in the study of state controls at that time. A half century later Henry Moore was calculating statistical demand curves; again the study of the effects of public policies was lagging.

The age of quantification is now full upon us. We are now armed with a bulging arsenal of techniques of quantitative analysis, and of a power—as compared to untrained common sense—comparable to the displacement of archers by cannon. But this is much less a cause than a consequence of a more basic development: the desire to measure economic phenomena is now in the ascendant. It is becoming the basic article of work as well as of faith of the modern economist that at a minimum one must establish orders of magnitude, and preferably one should ascertain the actual shapes of economic functions with tolerable accuracy.

The growth of empirical estimation of economic relationships, please notice, did not come as a response to the assault on formal theory by the German Historical School, nor was it a reply to the denunciations of theory by the American Institutionalists. It has been a slow development, contributed to by an earlier development in some natural sciences but mostly by the demonstrated successes of the pioneers of the quantitative method—the Jevons, the Mitchells, the Moores, the Fishers.

It is a scientific revolution of the very first magnitude—indeed I consider the so-called theoretical revolutions of a Ricardo, a Jevons, or a Keynes to have been minor revisions compared to the vast implications of the growing insistence upon quantification. I am convinced that economics is finally at the threshold of its golden age—nay, we already have one foot through the door.

The revolution in our thinking has begun to reach public policy, and soon it will make irresistible demands upon us. It will become inconceivable that the margin requirements on securities markets will be altered once a year without knowing whether they have even a modest effect. It will become impossible for an import-quota system to evade the calculus of gains and costs. It will become an occasion for humorous nostalgia when arguments for private and public performance of a given economic activity are conducted by reference to the phrase, external economies, or by recourse to a theorem on perfect competition.

This is prophecy, not preaching. You have listened to sage advice on what to study and how to study it for well over a century. If you had heeded this advice, you would have accomplished almost nothing, but you would have worked on an immense range of subjects and with a stunning array of approaches. Fortunately you have learned that although such advice is almost inevitable on such occasions as the retirement of an officer of a professional society, it is worth heeding only when it is backed by successful examples. I have no reason to believe that you left your tough-mindedness at home tonight, and I shall respect it. I assert, not that we should make the studies I wish for, but that no one can delay their coming.

I would gloat for one final moment over the pleasant prospects of our discipline. That we are good theorists is not open to dispute: for 200 years our analytical system has been growing in precision, clarity, and generality, although not always in lucidity. The historical evidence that we are becoming good empirical workers is less extensive, but the last half century of economics certifies the immense increase in the power, the care, and the courage of our quantitative researches. Our expanding theoretical and empirical studies will inevitably and irresistibly enter into the subject of public policy, and we shall develop a body of knowledge essential to intelligent policy formulation. And then, quite frankly, I hope that we become the ornaments of democratic society whose opinions on economic policy shall prevail.

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WAGES AND EMPLOYMENT IN A LABOR-SURPLUS ECONOMY

By LLOYD G. REYNOLDS*

A decade ago Arthur Lewis published the first of two pioneer articles on economic growth under conditions of labor surplus [5] [6]. While this problem has since been explored by other writers, particularly Gustav Ranis and John C. H. Fei [8] [9], it is appropriate to refer to a family of "Lewis-type models" of economic development. These models depict the absorption of an initial labor surplus by transference of labor from the traditional to the modern sector of the economy. Thus far, however, there has been little effort to compare the development experience of specific countries with the predictions derived from Lewis-type models. The present essay is intended as a step in this direction, using data for the Puerto Rican economy from 1945 to date.

The salient features of the Lewis model are illustrated in Figure 1. The economy is divided into subsistence and capitalist sectors. The capitalist sector is "that part of the economy which uses reproducible capital and pays capitalists for the use thereof" [5, p. 148]. The subsistence sector includes everything else. While the subsistence sector is often identified empirically with traditional agriculture, Lewis notes that surplus labor may be found equally well in petty trade, domestic service, and other service occupations.

The subsistence sector contains surplus labor in the sense of workers whose marginal productivity is negligible, zero, or even negative.[1] These workers nevertheless receive an income, *OS* in Figure 1, which

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[1] The existence of zero marginal productivity as a common phenomenon has been disputed by some writers, including Theodore W. Schultz [10]. If I understand Schultz aright, he is arguing that, through generations of experimentation, traditional agriculture gets organized in a way which makes optimum use of whatever labor supply is available, and which leaves each member of the labor force with a positive (though low) marginal product. If labor is withdrawn from agriculture *with no change in techniques*, output will fall. This may well be correct. But it does not contradict the possibility that, if techniques known and used in progressive agricultural economies can be transferred to a backward economy, labor can then be transferred out of agriculture with no loss of output. Withdrawal of labor may itself stimulate improvement of techniques. Moreover, nothing in the Schultz argument denies the possibility that the marginal productivity of labor, though positive, may be below the worker's income.
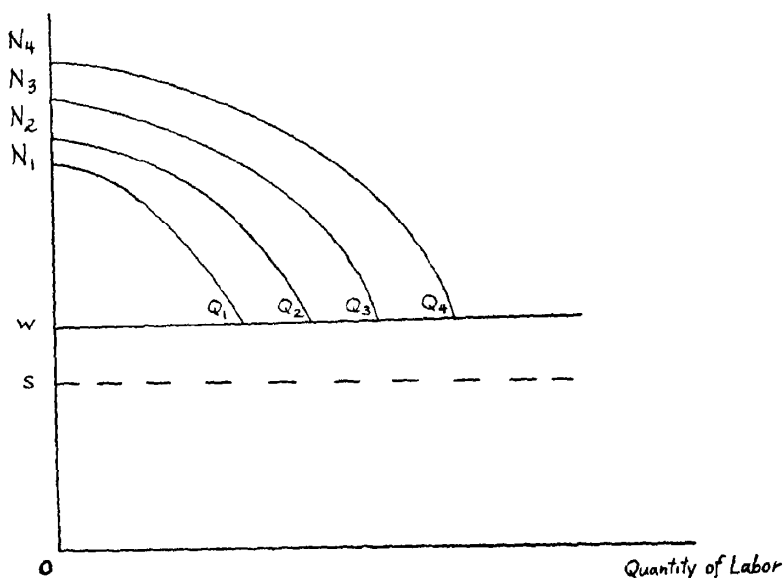
FIGURE 1

enables them to live after a fashion. This situation can arise *inter alia* because the income of a family group is shared among family members regardless of their individual contribution.

The wage rate in the capitalist sector, OW, must be somewhat above average income in the subsistence sector. Lewis suggests that the margin is usually 30 per cent or more, though its exact size does not matter for his argument. The reasons for this wage gap include higher living costs in the towns, the psychological cost to workers of transferring from the leisurely pace of traditional activities to the tighter discipline of industry, and perhaps also higher conventional standards of living in urban areas.

Employment in the capitalist sector is determined on ordinary maximizing principles. If at a certain time the marginal productivity schedule of labor in the capitalist sector is N_1Q_1, then WQ_1 workers will be hired. Capitalist profit is N_1Q_1W, and Lewis assumes that this will be reinvested. This raises the marginal productivity schedule to N_2Q_2, so that WQ_2 workers are now hired, and so on. Because of the reserve of surplus labor in the subsistence sector, labor supply to the capitalist sector is infinitely elastic at the constant real wage OW. The wage OW, incidentally, is measured in *industrial goods*, while OS is

measured in agricultural goods. This is explicit in the Ranis-Fei argument, but implicit also in Lewis.

When does the era of unlimited labor come to an end? It may never end because labor supply is being enlarged continuously through natural increase. Unless the labor demand curve moves to the right faster than the labor supply rises, surplus labor will increase over time. But suppose labor transference proceeds fast enough to cut into the labor surplus. Labor becomes scarce, and the supply curve of labor to industry turns upward, when disguised unemployment has been eliminated in the subsistence sector, i.e., when enough labor has been transferred to the modern sector so that the marginal productivity of those remaining in subsistence activity rises to the level of *OS*. Beyond this point, which Ranis and Fei term the *commercialization point,* the subsistence sector must pay workers the value of their (rising) marginal product and must compete with the industrial sector for scarce labor. The subsistence sector has vanished by becoming "modern," and both sectors now operate on commercial principles.

But the industrial wage rate may be forced up even before the commercialization point has been reached. Suppose, for example, that while labor productivity is rising in industry it remains unchanged in agriculture. When enough workers have been moved out of agriculture so that the marginal productivity of those remaining is no longer zero, further withdrawals of labor will reduce food output. Even without a drop in food output, expansion of incomes in the capitalist sector will raise the demand for food and, in a closed economy, turn the internal terms of trade in favor of agriculture. The industrial workers, whose wage is measured in industrial goods, will have to receive more of those goods to enable them to consume as much food as before.

The industrial wage level may also rise prematurely, that is, before the labor surplus is exhausted, for noneconomic reasons—a rise in conventional standards of life, voluntary increases granted by the capitalists on moral grounds, trade union pressure, or government regulation. This last possibility, which Lewis notes only in passing, has turned out to be quite important in Puerto Rico.

I. *Aspects of the Puerto Rican Case*

Space does not permit an over-all review of the recent development of the Puerto Rican economy, but a few preliminary points must be made. Additional background material will be found in studies by Harvey Perloff [7], Werner Baer [1] [2], and others.

The growth rate of total and per capita output in Puerto Rico since 1940 has been one of the highest in the world. Real GNP per capita

rose at an average rate of 4.1 per cent a year during the 'forties and 5.2 per cent a year during the 'fifties. In 1954 dollars, per capita GNP rose from $269 in 1940 to $673 in 1961, placing Puerto Rico almost above the range of "underdeveloped" countries. GNP per employed worker, in 1954 dollars, rose from $932 in 1940 to $2,802 in 1961.

While the industrialization program has attracted widest public attention, economic progress has been general. Food production has risen at a rate which has permitted Puerto Rico to maintain about the same degree of self-sufficiency despite much higher income levels. This degree is quite low, however, only about 40 per cent of Puerto Rican food consumption coming from domestic sources. Different sectors of the economy have expanded at different rates, and the industrial composition of output has changed materially. Manufacturing rose from 12 per cent of total output in 1940 to 23 per cent in 1962, while agriculture dropped from 32 per cent to 13 per cent. There were substantial increases in the contribution of the construction industry and the Commonwealth government to national product.

Manufacturing development has been stimulated by legislation granting manufacturers of products not produced in Puerto Rico in 1947 full exemption from both income and property taxes for periods ranging from 10 to 17 years, depending on the part of the island in which the plant is located; by a wage level which in the late 'forties was only about one quarter of that in mainland manufacturing plants; and by an energetic and capable Economic Development Administration which provides a variety of market research, plant-construction, labor-training, financing, and technical advisory services. Since 1947 about thirteen hundred E.D.A.-sponsored manufacturing plants have been established in the island, the great majority being branch plants of mainland companies. The failure rate among these new establishments has been about one-third, but 910 of them were still in operation at the end of 1963, with a total employment of about seventy thousand workers, or one-tenth of the island's labor force. Manufacturing development has been primarily in light industry. Clothing, textiles, and food products provide about half of manufacturing employment. But in recent years there has been a considerable development of oil refining, chemicals, paper, metal products, and other heavier types of industry.

The peculiarities of the Puerto Rican case are readily apparent. It is a small economy, with close ties to the United States, hence is a very open economy. Exports and imports run higher than 50 per cent of domestic output. Capital, labor, and commodities move freely between Puerto Rico and the mainland United States. Manufacturing investment has been financed mainly from the mainland rather than from

domestic sources. Puerto Rico has autonomy in tax matters, but only limited autonomy in wage determination, and little control over product prices, which are dominated by price movements on the mainland.

In other respects, however, Puerto Rico appears as a labor-surplus economy in the early stages of industrialization, with the employment and wage problems common to such economies. From a research standpoint, the economic statistics available for Puerto Rico, which largely follow U.S. concepts and procedures, are of unusually high quality. See in particular the sources cited in [11] [12] [13] [14] from which most of the data in this and subsequent sections were derived. We proceed, then, to examine the behavior of wages, productivity, profits, employment, and unemployment in Puerto Rico since World War II, and to compare this behavior with the predictions from Lewis-type models.[2]

II. *Employment and Unemployment*

Puerto Rico is one of the most densely populated areas on earth, with about 730 people per square mile in 1963. While birth rates have shown some tendency to fall since 1950, the rate of natural increase remains close to 2½ per cent a year. Total population is about 2½ million, the annual increment to population above 60,000. A major objective of the development program has been to provide jobs at a rate exceeding the rate of labor force growth, and thus to cut into the island's labor surplus.

Heavy emigration to the U.S. mainland, stimulated by good employment opportunities and cheap air travel, provided a respite from population pressure during the 'fifties. Net emigration from the island averaged 43,000 a year, or about three-quarters of the natural population increase, during the decade. Moreover, since migration was concentrated among young adults, the Puerto Rican labor force actually declined between 1950 and 1960. With a shrinking labor force and a booming island economy, one might have expected rapid strides toward elimination of unemployment.

[2] The substantive sections of this paper draw heavily on the findings of the Manpower Resources Project, which the writer directed at the Social Science Research Center of the University of Puerto Rico. Many of the points made here will be documented more completely in a forthcoming volume by the writer and Professor Peter Gregory, of the University of Minnesota, who served as assistant director of the project. I would like to acknowledge Professor Gregory's contribution to our joint effort, both in terms of data accumulation and techniques of analysis. In particular, he developed the measures of elasticity of demand for manufacturing labor in Puerto Rico described in a later section. He should not be held accountable, however, for any defects in this interpretation of our findings. Professor Luz Torruellas, director of the department of economics at the University of Puerto Rico, who also served as assistant director of the project, was most helpful throughout, and her contribution is gratefully acknowledged.

TABLE 1—EMPLOYMENT AND UNEMPLOYMENT IN PUERTO RICO, BY SEX, FISCAL YEARS 1951 AND 1963

Employment Status	Men				Women			
	1951		1963		1951		1963	
	No. (000)	Per cent	No. (000)	Per cent	No. (000)	Per cent	No. (000)	Per cent
Labor force	508	100.0	516	100.0	205	100.0	179	100.0
Employed	431	84.8	443	85.8	173	84.4	163	91.1
35 hours or more	255	50.2	300	58.1	71	34.6	91	50.8
less than 35 hours	162	31.9	117	22.7	93	45.4	55	30.7
employed but not working	14	2.8	26	5.0	9	4.4	17	9.5
Unemployed	78	15.3	73	14.1	32	15.6	16	8.9

Source: Puerto Rico Department of Labor, Bureau of Labor Statistics; 1963 figures are estimates based on extrapolation of data for the first three quarters; data *include* home needleworkers.

When one looks at the data, however, one gets an unpleasant surprise. Total employment *fell* between 1950 and 1960. The extent of the decline in labor force and employment depends on whether one includes home needleworkers in the totals. If one excludes home needleworkers as being very part-time employees,[3] the labor force dropped by 35,000 and employment declined by 12,000 between 1950 and 1960. It follows that there was only modest progress in reducing unemployment.[4]

This impression is confirmed by Table 1. The percentage of full-time unemployed male workers was almost as high in 1963 as it had been in 1951. The unemployment percentage for women, however, fell from 15.6 to 8.9 over this period. This reflects the fact that about 60 per cent of the jobs in the new E.D.A.-sponsored factories have been filled by women workers. Note also that the proportion of employed people working less than 35 hours a week dropped considerably for both sexes. In the case of men, this probably represents mainly a transference of underemployed workers out of agriculture. In the case of women, the

[3] The number of women doing needlework in their homes was 51,000 in 1950, only 10,000 in 1960, which is why their inclusion or exclusion makes a substantial difference in the behavior of employment. Net annual output per worker, however, is estimated at only $143 for 1955 and $159 in 1960, which is suggestive of very part-time supplementary employment. On this ground it seems reasonable to set these workers aside as a separate category.

[4] Since 1960, labor force and employment in Puerto Rico have begun to rise; and this has been accompanied by a sharp decline in net emigration to the mainland. So while part of the recent increase in employment is no doubt genuine, part may represent a "backing up" of surplus labor and an increase in underemployment.

figures reflect a decline of 40,000 in the number of home needleworkers, most of whom seem to have dropped out of the labor force instead of seeking other employment.

Why was there not more striking progress on the employment front? There are two lines of explanation that may be widely applicable to newly industrializing countries. First, economic development brings a shrinkage of employment in some sectors, so that total employment can rise only if this shrinkage is more than offset by expansion in other sectors. Second, manufacturing industries embodying modern production methods create relatively little employment; and part even of this employment involves drawing new people into the labor force rather than absorbing previously unemployed workers.

The most dramatic employment shrinkage in Puerto Rico has been in agriculture. In the single decade 1950-60, agricultural employment fell from 214,000 to 124,000. There was a drop of about 40,000 in sugar growing, 10,000 in tobacco cultivation, and 40,000 in food production for local consumption. This last decline is especially interesting, since the farm value of domestic food production on the island rose from about $70 million in 1949-50 to $130 million in 1960-61, indicating a substantial gain in real output. The implication is that the 1950 farm labor force was seriously underemployed.

This impression is confirmed by special studies of the Puerto Rico Department of Labor. Between 1955 and 1960, for example, the agricultural labor force dropped by 45,000. But the number of *fully employed agricultural workers* declined by only 12,000. The rest of the shrinkage came from a drop of 12,000 in the full-time unemployed, 8,000 in wage workers averaging less than 35 hours a week, and 12,000 in subsistence farmers.[5]

The decline of 41,000 in the number of home needleworkers between 1950 and 1960 has already been mentioned and in part discounted as representing a much smaller decline in equivalent full-time employment. It nevertheless represents a substantial decline in economic activity, due mainly to the fact that rising legal minimum wages have made it increasingly difficult for Puerto Rican producers to compete with products from the Philippines, Hong Kong, Japan, and other areas. Finally, economic development brought the usual rapid decline in the number of domestic servants, which fell from 31,000 in 1950 to 17,000 in 1960.

The declines in these sectors during the 'fifties were about balanced by expansion in others. Between 1950 and 1962 manufacturing added

[5] Puerto Rico Department of Labor, Bureau of Labor Statistics, *Full Employment and Underemployment in Puerto Rico*, Special Reports Nos. 22, 27, 31, and 34.

36,000 workers, construction 24,000, government 12,000, and other service industries about 20,000. Together with smaller expansions in utilities, trade, and finance, about 115,000 jobs were added to the economy over these twelve years. Note that manufacturing provided less than one-third of the new jobs. It is significant also that, while manufacturing output tripled in real terms between 1950 and 1962, factory employment rose only 65 per cent. The source of these productivity gains will be explored in a later section.

Today, about one-eighth of the Puerto Rican labor force is wholly unemployed. Another one-quarter work less than 35 hours a week.[6] There are doubtless others who could be withdrawn from agriculture, trade, and service with little loss of output. The unlimited supply of labor to industry remains a reality.

III. *Wage Determination and Wage Behavior*

Despite this abundance of labor, Puerto Rican wages have risen at a startling rate. Between 1950 and 1963, the average hourly earnings of production workers in E.D.A.-sponsored manufacturing plants almost tripled (Table 2). The gap between Puerto Rican and mainland wages narrowed considerably. Because of substantial differences in the composition of manufacturing in the two areas, the gap should really be measured on an industry by industry basis. Table 3 indicates that, for selected industries which are important both in Puerto Rico and on the mainland, Puerto Rican wages rose from between 25 and 35 per cent of mainland levels in 1952 to between 50 and 70 per cent in 1962.

Even this comparison is not conclusive for appraising locational advantage. The Puerto Rican wage level in each industry should properly be compared with low-wage areas on the mainland which might be considered as alternative locations. One might also compare wages in Puerto Rican plants with those of mainland plants operated by the same company. One such study, which covered 50 companies in the spring of 1958, found that wages in the Puerto Rican plant ranged from 35 per cent to 94 per cent of those in the mainland plant, with a median of 59 per cent [3]. Considering the continued improvement of Puerto Rico's relative position since 1958 shown by Table 3, a comparable survey today would probably show a median in the neighborhood of 70 per cent.

[6] Only part of this group, of course, can be considered underemployed. Women workers in particular often prefer a part-time schedule. Special analyses by the Puerto Rico Department of Labor suggest that about 70 per cent of the women working a short week, and 35 per cent of the men, consider themselves fully employed and do not want longer hours of work.

TABLE 2—AVERAGE HOURLY EARNINGS OF PRODUCTION WORKERS IN E.D.A.-SPONSORED MANUFACTURING PLANTS AND IN U. S. MANUFACTURING, 1950–63

Year	E.D.A. Plants Puerto Rico	All Manufacturing United States	Ratio of E.D.A. Plants to U. S. Manufacturing
1950	$0.412	$1.501	.274
1951	.448	1.615	.277
1952	.453	1.705	.266
1953	.475	1.79	.265
1954	.505	1.81	.279
1955	.607	1.91	.318
1956	.720	2.02	.356
1957	.830	2.09	.397
1958	.884	2.14	.413
1959	.935	2.21	.423
1960	.983	2.30	.427
1961	1.031	2.34	.427
1962	1.091	2.39	.457
1963	1.159	2.47	.470

Sources: The Puerto Rican data for E.D.A.-sponsored plants are reported in E.D.A., *Annual Statistical Report,* 1960–62, pp. 27–31. The U.S. data derive from the U. S. Bureau of Labor Statistics. Data are for October of each year.

TABLE 3—AVERAGE HOURLY EARNINGS IN SELECTED INDUSTRIES: PUERTO RICO AS PER CENT OF U.S., 1952 AND 1962

Industry	1952	1962
Cigars	.30	.67
Broad woven fabrics	.38	.56
Knitting	.41	.70
Floor coverings	.26	.53
Men's and boys' furnishings	.32	.61
Women's and misses' outerwear	.26	.52
Women's and children's undergarments	.32	.70
Girls' and children's outerwear	.28	.59
Paperboard containers and boxes*	.36	.63
Drugs	.22**	.49
Leather footwear	.30	.52
Fabricated metal products	.31	.51
Machinery—nonelectrical	.39	.54
Household appliances	—	.56
Toys and sporting goods	.26	.51
Costume jewelry, buttons, and notions	.33	.50

* The data for Puerto Rico are for the broader industrial group, paper and allied products; however, the bulk of the activity is to be found within the paperboard and box division.

** The ratio is for 1950.

Sources: The Puerto Rican data were derived from the annual *Census of Manufacturing Industries of Puerto Rico,* published by the Puerto Rico Department of Labor, Bureau of Labor Statistics. The wage data apply to the week ending nearest the first week of October for each year. The wage data for the United States were taken from the *Monthly Labor Review* and apply to the month of October for each year.

The large wage increases since 1950 have not been confined to manufacturing, but have been general throughout the economy. Manual workers in construction, public utilities, and the service industries have received increases of roughly the same percentage size as in manufacturing. There has been a serious lag, however, in agricultural earnings. In 1952 the average wage earner in agriculture earned about half as much as the average factory worker. By 1962 he earned less than one-third as much. The urban wage level has pulled away increasingly from the agricultural base.

It is hard to find an economic rationale for the rapid rise of the urban wage level. Some pressure on food supplies is suggested by the fact that consumer prices of domestically produced foodstuffs rose more than 50 per cent between 1947 and 1961. But 60 per cent of the food consumed on the island is still imported, and import prices have risen only slightly. Prices of nonfood items, and the over-all consumer price index, have risen at about the same rate as on the mainland. Thus there has been a rapid rise in real wage levels, and it is this which has to be explained.

Trade union organization in Puerto Rico is relatively weak. Pressure for higher wages has come mainly from the U.S. and Commonwealth governments, operating through legal minimum wages. Industries involved in external commerce are covered by special provisions of the Fair Labor Standards Act. There is also a Puerto Rico minimum wage law, passed originally in 1941 and revised and strengthened in 1956. This act can be applied to external commerce, but in practice the Commonwealth government has ceded jurisdiction in this area to industry committees appointed under the FLSA. The main impact of the Puerto Rican law is on intra-insular activities, where minima have been established for most of the major industries, including agriculture.

We are accustomed to regard minimum wage regulation in the mainland United States as rather unimportant. The reason is that the legal minimum is a flat rate, infrequently revised, and set well below the prevailing wage level of most industries. The control structure in Puerto Rico is quite different. Under both the federal and island legislation, minimum wages are set separately for each industry on the recommendation of tripartite industry committees. The minima are geared to the estimated wage-paying ability of each industry, and there is at any time a wide range between the highest and lowest industry rates. Most workers in each industry earn very close to the minimum rate; and as the minimum is raised, which happens every year or two, the industry wage level is forced up by a proportionate amount. There is convincing evidence that minimum wage regulation, rather than labor-supply conditions, is mainly responsible for the advance of real wages over the past 15 years.

The rate at which the legal minima have been raised, as well as the dispersion of industry minima at a particular time, is illustrated by Table 4. In 1949 the median minimum wage was about 30 cents an hour, and there were still many rates below 25 cents. By 1963 the median minimum wage was $1.00 an hour, and there were scarcely any rates below 70 cents. The impact of successive revisions stands out clearly from Puerto Rican wage statistics. When an industry's minimum is raised, average hourly earnings rise by about the same amount, though usually with some lag because the rates of upper occupational groups may not be revised immediately. After the increase, as before, the bulk of the labor force is found earning very close to the legal minimum.

TABLE 4—FREQUENCY DISTRIBUTION OF INDUSTRY WAGE MINIMA UNDER THE FLSA, 1949 AND 1963

Minimum Rates	1949	1963*
Under .25	37	0
.25–.299	20	1
.30–.349	17	1
.35–.399	16	0
.40–.449	35	0
.45–.499		2
.50–.599		2
.60–.699		2
.70–.799		27
.80–.899		20
.90–.999		19
1.00–1.149		38
1.15–1.249		36

* Data as of June 1963; a further automatic increase of 10 per cent in all rates was scheduled for November 1963.

Source: U. S. Department of Labor, Wage and Hour and Public Contracts Division.

The leaders of the Commonwealth government would doubtless have acted to raise the manufacturing wage level in any event. However one may view the classical model of capital accumulation in principle, the spectacle of very large profits and stagnating wages can scarcely be viewed with equanimity by a democratically elected government. Whether Commonwealth leaders, given completely free choice, would have moved so rapidly to reduce the island's locational advantage is uncertain, for they have been under strong pressure from the mainland. Each time Congress has revised the Fair Labor Standards Act, many mainland manufacturers and union leaders have urged that Puerto Rico be blanketed under the federal minimum to eliminate "unfair" competition. Political and business leaders on the island have avoided this only by accepting substantial wage increases under the

industry committee system.[7] Each industry committee contains union, industry, and public representatives from the mainland as well as from Puerto Rico. The Puerto Rico industry representatives, who alone have a strong incentive to resist wage increases, find themselves outnumbered; and the wage decision which emerges may well differ from that which would have been reached by a committee of island residents only.

We shall argue in a moment that rapid wage increases have operated to retard the expansion of factory employment in Puerto Rico. They have doubtless had advantages in other directions, and opinions will differ on where the balance of advantage lies. But even if one concludes that a more moderate rise of wages would have better served the interest of Puerto Rico, this is not necessarily a criticism of the economic judgment of Commonwealth officials. Domestic and external political pressures have operated to restrict their freedom of maneuver.

IV. *Wages, Productivity, and Employment*

The rapid rise of wages may have retarded the expansion of employment in two ways. It may have deterred some mainland companies from establishing branch plants in Puerto Rico, and it may have induced plants that were established to use more labor-saving methods of production. What evidence is there on the strength of these effects?

Puerto Rico has certain cost disadvantages which must be offset to make location there attractive. Transportation costs are often higher, especially where components are shipped from the mainland to Puerto Rico for processing or assembly and the finished product is re-exported to the mainland. Uncertainty of shipping is an additional cost, for the docks are well organized and dock strikes are not infrequent. Executives brought from the mainland to manage plants in Puerto Rico are usually paid a substantial premium over their mainland salaries to cover the cost of living in their accustomed fashion by buying imported U.S. goods, the cost of sending children to mainland schools and colleges, and vacation and other travel for themselves and their families. Companies seem also to expect a substantially higher profit margin on their Puerto Rico operations to offset additional risks and uncertainties.[8]

[7] And even so, they have not always averted specific Congressional action. In the most recent FLSA revision, Congress provided that the mainland minimum of $1.00 was to be raised in two steps, to $1.15 in 1961 and to $1.25 in November 1963. In the case of Puerto Rico, all minimum rates were to be increased in two steps, simultaneously with the mainland increases and by the same relative amounts. Thus a 15 per cent increase was provided for the first step and a further 10 per cent increase for the second. Still higher minima, of course, could be set by industry committee action.

[8] For evidence on this point see the study by Mrs. Griffith [3], based on interviews with 50 companies which have located in Puerto Rico and 50 companies which considered locating there but decided against it. It is possible, of course, that companies which require a high profit rate to locate in Puerto Rico initially may be willing to stay there

Labor efficiency appears to be an unfavorable consideration *ex ante*, although it is on the average a neutral factor *ex post*. Prominent in the thinking of companies which have decided against locating in Puerto Rico is an expectation that labor productivity will be sufficiently lower that, even with lower wage rates, there will be no saving in unit labor costs relative to the mainland. These expectations are in fact unduly pessimistic. The evidence suggests that well-managed plants designed to mainland standards can reach mainland productivity levels after a reasonable breaking-in period. For locational decisions, however, it is expectations which matter rather than facts; and productivity expectations have usually been unfavorable.

The main favorable factors are tax exemption, which is temporary in nature, and a lower wage level. Many companies reason that the wage level should yield savings in labor cost sufficient to offset the cost disadvantages noted above, leaving them with the tax advantage as a net gain. Thus as the wage level in Puerto Rico approaches that of competing areas on the mainland, one may expect a rise in the proportion of companies deciding against a Puerto Rican location. Eventually one should reach an equilibrium leaving no net inducement for plant migration to Puerto Rico. But at this equilibrium, will Puerto Rican wages be 5 per cent below competitive areas on the mainland, or 10 per cent below, or 20 per cent below? This is hard to estimate, and the answer will differ from one industry to another.

There has been no absolute retardation in the movement of industry into Puerto Rico. On the contrary, the number of new E.D.A.-sponsored plants established in the island has risen from 283 during the four-year period 1952-55 to 388 during 1956-59 and 511 during 1960-63. It seems likely that the inflow of plants would have been even larger had wage increases been less rapid; but it is hard to test this hypothesis.

We can speak with more confidence about the reactions of plants already established in Puerto Rico. They responded with productivity-raising improvements which were sufficient to offset most of the higher wage costs. But the offset was not complete. Average annual profits for all E.D.A.-sponsored manufacturing plants, calculated as a percentage of owners' equity, varied in the range of 35 to 40 per cent up to 1956. As the pace of wage increases accelerated in the mid-'fifties, the annual profit rate declined until by the early 'sixties it was in the range of 25-30 per cent.[9] In some labor-intensive industries, such as the important

for a lower rate. Despite large wage increases and eventual expiration of tax exemption, relatively few plants have been shut down. Of 95 E.D.A.-sponsored plants whose tax exemption had expired by the end of 1962, only 21 had been closed.

[9] Calculations based on data from Economic Development Administration, *Annual Statistical Report of EDA Manufacturing Plants*, successive editions through 1962.

foundation-garment industry, pretax profits in Puerto Rico have falle to about the mainland level, leaving only the tax advantage to th Puerto Rican producers.

How were productivity increases accomplished? The aggregate statis tics for the manufacturing sector are revealing. The period of ac celerated wage increase seems to have brought little change in capital output ratios.[10] But over the years 1954-61 both capital per worke and output per worker approximately doubled. There was a sharp re duction in the use of labor relative to *both* capital and output. Manage ment found ways of dispensing with labor and of getting greater outpu from those who remained.

Introduction of labor-saving machinery in response to wage increase does not seem to have been of major importance. It is true that in som of the older native firms, wage pressure has forced modernization o the entire plant, with a consequent increase in the amount of capita employed. But in mainland branch plants, the capital equipment wa usually already of recent vintage. With a few notable exceptions, main land firms establishing branch plants in Puerto Rico did not adopt pro duction techniques different from those employed in mainland plants Where exceptions were made, they were generally in the direction o more labor-intensive methods of materials handling. Thus the possi bility of factor substitution was limited, and seems inadequate to ex plain more than a small part of the increase in capital-labor ratios.

What mainly happened was that personnel and production manage ment were much improved over the years. Field investigation of 8 recently established manufacturing plants in Puerto Rico, carried ou in the mid-'fifties as part of the Manpower Resources Project, revealed remarkable instances of inefficiency. Among the managers of mainland branch plants whom we interviewed, almost half had never before oc cupied a management position, and some had no industrial experience of any sort. First-line production supervisors had been chosen largely from the Puerto Rican population, often on the ground of fluency in English, which is scarcely a sufficient qualification. Workers were often carelessly selected, training methods were inadequate, standards of expected output were low, waste of materials and spoiled work were excessive, labor turnover and absenteeism were high. Even at this time one encountered some well-managed establishments which were approaching mainland productivity levels. But in others the feeling seemed to be that, with wage rates so low, one could scarcely avoid showing a profit.

Efficiency would doubtless have risen over the years through normal

[10] For E.D.A.-sponsored manufacturing plants of assets of less than $1 million, capital employed per dollar of sales receipts was $0.90 in 1954 and $0.87 in 1960. For plants with assets above $1 million, the corresponding figures are $1.27 in 1954 and $1.23 in 1960.

learning. But the rising wage level was a powerful stimulus to learning. Each time a minimum wage increase was impending, most managements reviewed their personnel policies and production standards to see what might be done to offset the higher wage; and usually something could be done. Managers and supervisors were replaced, job layout was improved, work crews were trimmed down to minimum size, waste of materials and products was reduced through better training and supervision, standards of expected output were raised, costs of turnover and absenteeism were lowered. There is apparently considerable interdependence between how much management expects workers to produce, how much they were willing to produce, and the level of their earnings. Workers on incentive systems, who would have resisted a simple increase in output standards, as a "speed-up," often accepted such an increase cheerfully when it was offset by a proportionate rise in their minimum wage.

Thus labor requirements per unit of product were reduced, not so much through larger capital inputs as through larger (or higher-quality) *inputs of management effort.* Anyone familiar with industry realizes that this is possible in some measure. The Puerto Rican experience dramatizes the magnitude of the productivity gains which can be achieved in this way during the early stages of industrialization.

It is not clear how one should rationalize this process in terms of production theory. If one defines the production function as embodying *median current practice,* one would have to say that the production possibilities frontier moved outward as the wage level rose. If on the other hand one defines the production function in terms of *best available techniques* (say, in this case, the performance of a superior mainland plant in the same industry), one would have to say that most Puerto Rican plants started off well within their production frontier and moved toward it as a result of wage pressure. (Some plants, of course, failed to adapt rapidly enough and passed out of existence.) Alternatively, one could define management as a separate input; but the difficulty of quantifying this input would be a serious bar to statistical analysis.

It would be interesting to know, not just that wage increases had a negative effect on employment, but the approximate size of this effect. So an attempt was made to estimate the elasticity of demand for labor in Puerto Rican manufacturing.[11] The procedure involved a basic assumption that, within each of the subperiods for which the elasticity was estimated (1949-54 and 1954-58), the production function of the Puerto Rican manufacturing sector was homogeneous of degree one.

[11] The method used was devised by my colleague, Peter Gregory, who also supervised the statistical calculations. Both the method and the results are described in greater detail in the forthcoming volume.

This abstracts from economies of scale as well as from technical prog ress. Following from this assumption is the proposition that, given con stant relative factor prices, the increase in output during a perioc should have been accompanied by a similar increase in employment Failure of employment to expand by the same amount as output coulc then be attributed to a change in the relative price of labor.

The procedure followed involved regression analysis using Censu: of Manufactures data on employment, production-worker wage pay ments, and value added for the years 1949, 1954, and 1958. A simpl linear regression equation was used, in which employment foregon (the difference between the rate of change in output and the rate o change in employment) was held to be a function of the rate of chang in the wage. The regression was fitted by least squares. Separate cal culations were made for two periods, 1949-54 and 1954-58. These pe riods were presumed to be long enough to permit adjustment of pro duction operations to any change in the price of labor. Cross-sectio data were used, covering 37 industries for the earlier period and 5(industries for the latter. The finest available classification was used so that most industries, particularly for the latter period, were definec at the 4-digit level. The value-added data for 1949-54 were deflated b the appropriate wholesale price indexes; those for 1954-58 were no deflated in view of the slight variation of price over this period.

Solution of the regression equations yielded an elasticity estimate o −1.137 for the period 1949-54, and −0.939 for 1954-58, neither o these being significantly different from unit elasticity by the *t*-test. So a change in the wage could be expected to be associated with an approxi mately equal proportionate change of employment in the reverse direc tion.[12] The regression equations can also be used to estimate th amount of employment foregone as a result of wage increases. Thi procedure yields an estimated loss of about 9,000 jobs between 194 and 1954, and of 29,000 jobs between 1954 and 1958, in the manu facturing sector alone. This amounts to more than 5 per cent of th island labor force. Since the wage advance was general, there was pre sumably a sacrifice of employment in other sectors as well.

These findings must be interpreted with caution. The procedur tends in some ways to overestimate, and in other ways to underesti mate, the employment effect of wage changes; and one cannot be cer tain which bias predominates in the results.

On the side of underestimation, we have taken no account of th

[12] This conclusion, of course, does not mean that the manufacturing wage bill in Puerto Rico remained constant from 1949-58. This would have happened only if the demand curve for manufacturing labor had itself remained constant. But in fact the demand curve has been shifting upward rapidly as a result of new investment.

fact that actual and anticipated wage increases may have deterred plants from locating in Puerto Rico. Our estimate of employment in the terminal year of each period at the base-year wage was based on *actual output* of goods rather than what output might have been in the absence of a rise in wages. Locational effects are thus excluded. We have already given reasons for thinking that these effects may have been substantial.

On the other hand, two factors may have led us to overestimate the association of wage and employment changes. We assumed a homogeneous production function in order to estimate potential employment at the base-year wage level. If the actual function yields increasing returns to scale, then we will have overestimated the amount of employment that should have been associated with the output of year 1, and hence the employment forgone as a result of the rise in the wage. Moreover, the assumption of a stable production function within each time period ignores the likelihood that even without wage shocks management would have achieved economies in the use of labor through normal learning.

It must be remembered also that the elasticity estimates are averages for all manufacturing. Elasticities in different Puerto Rican industries can be expected to vary widely because of differences in product market conditions and production functions. That these elasticities do vary widely has been recognized implicitly by Congressional reluctance to apply an across-the-board general wage minimum to Puerto Rico, and by the actions of the industry committees which have tried to weigh the probable effects of varying wage increases in different industries.

V. *Unlimited Labor, Wages, and Employment*

Unlimited supply of labor has been and is a reality in Puerto Rico. Employers in the modern sector have never had serious difficulty in recruiting labor, and it has not taken long to transform raw recruits into competent industrial workers. This labor has come from the sources which Lewis enumerated: agriculture, trade, domestic service, other service industries, and new entrants to the labor force.

Employment has not expanded, however, along the constant real wage line in Figure 1. The wage level has been raised repeatedly, and this has stimulated management responses which have restricted the rise of employment.

The actual course of events may be interpreted with the aid of Figure 2. The industrial wage level at time t_0 is *OW*, the schedule of marginal labor productivity is *MPL*, and employment is *OE*. Suppose that by time t_1 there has been new industrial investment which, by itself,
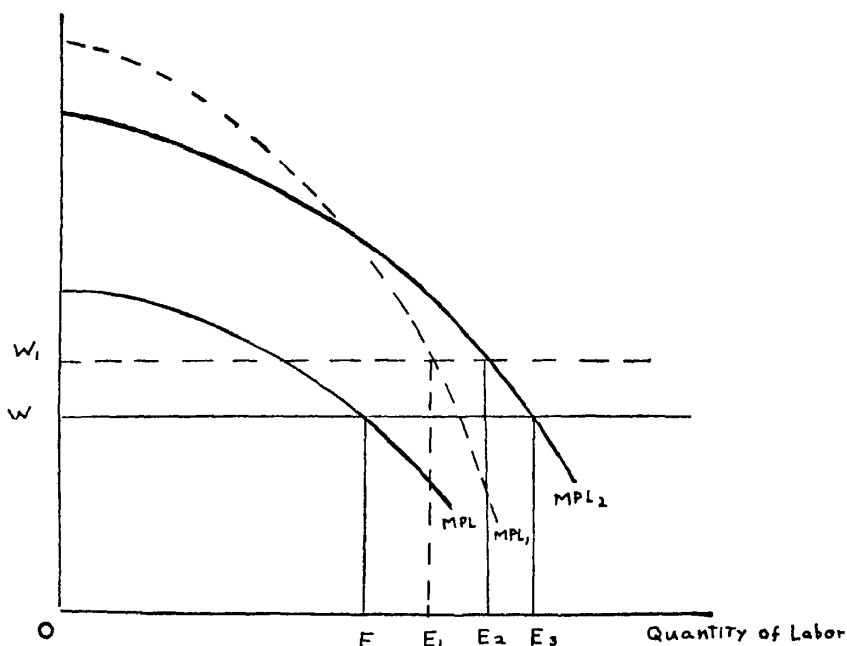
FIGURE 2

would shift the productivity schedule proportionately upward to MPL_2 and, with no change in wages, would result in employment of OE_3. Meanwhile, however, government has raised the wage level to OW_1. Labor supply is unlimited, as before, but it is unlimited at a higher real wage level. (An interesting feature of the Puerto Rican case is that raising money wages does raise real wages as well, since product prices are largely determined in the mainland market. This would not necessarily be true in a more closed economy.)

The wage increase stimulates management to make labor-saving innovations so that, with the investment of t_1, the labor productivity schedule is tilted to the position MPL_1.[13] The potential employment OE_3 is reduced on two counts. The wage increase alone would reduce it from OE_3 to OE_2; the labor-saving innovations cut it further to OE_1. Thus the substantial investment between t_0 and t_1 leads to only a slight increase in employment.

The upward shift of wages is repeated in the next time period, and similar management adjustments follow (Figure 3). Thus the wage-

[13] The shift from MPL_2 to MPL_1 would in fact be a *very* labor-saving innovation in the Hicksian sense, i.e., an innovation leading to reduced employment at the same wage level [4, Ch. 2] [9, Ch. 3].

employment locus, instead of moving horizontally to the right along path I moves upward to the northeast along path II. If government wage policy is aggressive, and if management is very successful in saving labor, path II may be quite steep—large wage increases, small employment increases. Path II looks like a conventional upward-sloping
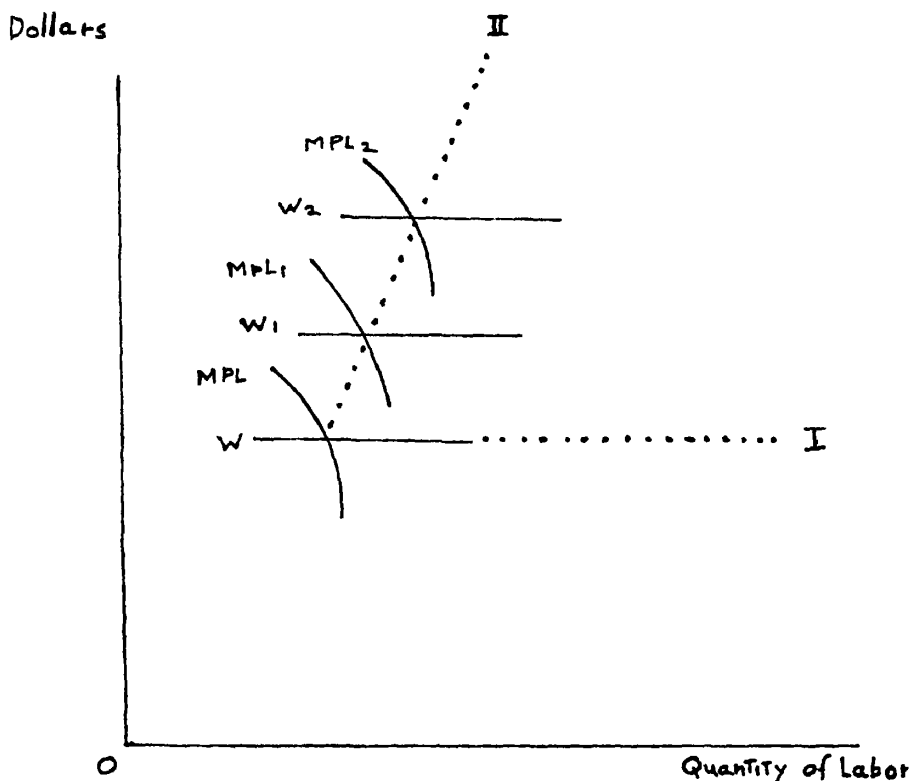
FIGURE 3

labor-supply curve; but it is actually the locus of demand-supply intersections in successive time periods, during each of which labor supply remains unlimited.

Is this behavior a peculiarity of the Puerto Rican case? Or are "premature" increases in the industrial wage level a general characteristic of today's industrializing countries? It would be useful to have evidence on this from other parts of the world.

The Puerto Rican experience is suggestive also as regards development policy. The objective of a development program is usually stated as a certain behavior of real per capita income. If employment objectives are included, this is usually as an afterthought. The plan document may assert optimistically that the number of new jobs created

will exceed projected additions to the labor force over the plan period. But the statistical foundation of these projections is typically weak, and there is little analysis of the kinds of action which would be needed to make them come true.

In a country which starts with a labor surplus, and which has a high population growth rate, employment objectives are important. The economy must transfer labor to the modern sector fast enough to cut into the labor surplus. Puerto Rican experience underscores the degree of effort required. The conjuncture of circumstances in Puerto Rico since World War II has been unusually favorable, yet progress in reducing the labor surplus has been slow. In countries less favorably situated, such as India and Pakistan, the tide of surplus labor is rising and will continue to rise over the foreseeable future.

Development of modern factory-style manufacturing makes only a limited contribution to employment. The Puerto Rican industrial development program has been unusually vigorous and successful; but from 1952-62 the average annual increase of employment in E.D.A.-sponsored plants was about 5,000 a year. With present labor force participation rates, and in the absence of net emigration to the mainland, annual additions to the Puerto Rican labor force would be of the order of 40,000. One hears reports also from other countries which, after a decade or more of accelerated industrialization, are surprised to find how little employment has been created. Manufacturing organized on Western lines, and particularly heavy manufacturing which is fashionable in the larger developing countries, is not very labor-using.

The moral is partly that there should be more energetic pursuit of employment opportunities outside the manufacturing sector: in agriculture, where new techniques which are land-saving rather than labor-saving may be able to absorb surplus labor without any physical transference; in labor-intensive public works programs organized along Nurkse lines; and in other directions which require mainly education and organization rather than capital investment. Within manufacturing, there should be imaginative exploration of small-scale, more decentralized, more labor-using forms of organization such as have persisted in the Japanese economy to the present day and have contributed materially to its vigorous growth. It can be shown that, up to a certain point, techniques which are more labor-using will also be output-increasing [9, Ch. 3]. Development policies which are not oriented toward using the abundant labor supply will also fail to maximize national output.

The problem of what pattern of incentives might be used to persuade privately owned manufacturing concerns to develop in this di-

rection is too large to be explored here. It seems clear, however, that rapid increases in the real wage level would not form part of the prescription. A country which considers employment expansion important should ponder the wisdom of raising the price of labor. Modern Western reasoning about wages, in which labor is taken as a scarce factor, may be quite misleading. Poor labor-surplus countries are still living in a classical world. Perhaps they should follow the classical route toward fuller employment.

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INTERNATIONAL SHORT-TERM CAPITAL MOVEMENTS

By Jerome L. Stein*

A major problem confronting international economics is exactly how sensitive are short-term capital movements to interest rate differentials? If there is a speculative flow of short-term capital, what interest rate differential is required to offset it?[1] If no change in exchange rates is expected, what will be the flow of short-term capital if U.S. short-term interest rates are changed relative to foreign short-term rates? A subsidiary question is whether the interest rate differential determines a stock or a flow. For example, if the interest rate differential determines a stock—e.g., short-term dollar liabilities to private foreigners—then a constant or declining basic deficit requires a growing interest differential. On the other hand, if the interest differential determines a flow, a constant basic deficit is consistent with a constant interest differential.

There are reasons to believe that the valuable pioneer studies by Bell [2] and Kenen [3] can be improved. First, these studies did not obtain consistent estimates of the sensitivity of capital flows to interest rate differentials. The reason for the inconsistency is that the independent variable used to obtain *significant* results, the covered differential, is affected by the disturbance term. Or, to put it differently, the "independent" variable used is an endogenous rather than an exogenous variable.

Second, they did not resolve the issue of whether the interest rate differential determines a stock or a flow. Bell used the stock theory, whereas Kenen used the flow theory.

Third, they did not successfully evaluate or measure the importance of speculative movements of short-term capital. Hence, there was a confounding of speculative and interest-rate-induced movements of short-term capital.

Fourth, the substitution between deposits and money market assets was clearly perceived by Kenen, but the effect of the interest rate differential on the *total* capital flow was not estimated. Instead, the sensi-

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[1] Assume that the speculative flow is of sufficiently short duration so that the domestic economy is not perceptibly affected in terms of income and employment by the change in short-term rates.

tivity of particular capital flows to the interest rate differential was examined. Bell's strictures against aggregation are unjustified because of the substitution effects. An aggregative approach is employed here.

My conclusions are based upon monthly data for U.S. and private foreign short-term capital reported by banks. The activities of nonfinancial corporations, not reported by banks, have been excluded.[2]

1. If there were no interest differential or speculative expectations, there would tend to be an annual outflow of $159 million of U.S. short-term capital and an annual inflow of $850 million of private foreign short-term capital. The net inflow is $691 million per annum.

2. If there were no speculative flows of short-term capital and the U.S. Treasury bill rate were one percentage point below the U.K. Treasury bill rate, there would be an annual outflow of $436 million of U.S. capital and an annual inflow of $665 million of foreign private capital. The net inflow would be $229 million per year. The U.S. capital outflow would rise by $277 million per year, and the private foreign capital inflow would decrease by $185 million per year. The total inflow of private capital would decline by $462 million per year. (Of course only the change in the U.S. capital flow would affect the officially measured deficit.)

3. If there is speculative pressure against the dollar such that the forward rate on the dollar is one percentage point less than predicted (from a regression of the forward premium on the interest differential), there will be an outflow of $783 million of U.S. short-term capital per annum and an outflow of $990 million of private foreign short-term capital per annum. The net outflow would be $1,773 million per year. The net effect is a $2.5 billion decrease in the net capital inflow per year.

There are three parts to this paper. Part I evaluates the studies of Bell and Kenen and shows how they can be improved. Part II uses models of the short-run foreign exchange market to derive two different short-term capital movements functions. Part III presents the statistical analysis and shows the empirical superiority of a flow theory over a stock theory in explaining capital movements, i.e., flows of short-term capital.

I. *Recent Empirical Studies*

A. *The Design of Recent Empirical Studies*

Philip Bell and Peter Kenen examined many components of the U.S. and private foreign short-term capital accounts. Bell used stocks, and

[2] They were excluded because they are not reported monthly. Hence my estimates of the total interest sensitivity, etc., are on the low side. Results are given in annual rates for convenience of presentation. No claim is made that speculative situations last that long.

Kenen used flows as the dependent variable. Their object was to ascertain the sensitivity of the dependent variable to changes in the interest rate differential between countries.

Their general conclusion is that if any interest rate differential is statistically significant, it is the covered and not the uncovered differential. Kenen, for example, used a multiple regression analysis in explaining variations in the flows of capital. Since the covered and the uncovered yields were positively correlated, they were not used as independent variables in the same regression equation. Many equations were calculated twice: once with the uncovered and once with the covered yield. In his study, if the Treasury bill rate differential is statistically significant at the 5 per cent level in a multiple regression, there is seldom any other statistically significant variable at the 5 per cent level.[3] When there is another significant variable, it is usually the Euro-dollar deposit rate (London) less U.S. Treasury bill rate, which is positively correlated with the U.K. minus U.S. Treasury bill differential. Kenen shows that better results were obtained using the covered rather than the uncovered U.K. minus U.S. Treasury bill differential.

Unlike Kenen who regressed capital flows upon various measures of the interest rate differential, Bell used the stock of claims against foreigners as his dependent variable. His experiments were designed to answer the question: Is the stock of U.S. short-term claims against foreigners positively related to the interest rate differential between countries? He did not find the uncovered interest rate differential to be a significant explanatory variable. Experiments were performed where the covered interest rate differential is an independent variable, and various categories of U.S. short-term claims against foreigners are the dependent variables. Most of the time, the regression coefficient of the covered interest rate differential had the wrong sign. A rise in the covered yield in favor of the foreign country was associated with a decline in the total U.S. short-term claims held against foreigners. In only one case did the regression coefficient have the correct sign and a sufficiently high level of statistical significance to lead us to reject the null hypothesis at the 5 per cent level. Bell concludes that there is little evidence that interest considerations are a primary motivating force in short-term capital movements [2, p. 441; Table 9, p. 440]. Moreover, the volume of exports to an area was a significant variable in explaining capital movements only in the case of U.S. exports to Canada.

B. *A Critique of Existing Studies*

1. *Bias and inconsistency in estimation.* Any regression equation used by Bell or Kenen which was successful in explaining variations in the

[3] See [3, Appendix I, Annex 2, pp. 5–12].

capital account contained the covered rather than the uncovered interest rate differential. It is unfortunately true that such a regression produces misleading estimates of the sensitivity of short-term capital movements to short-term interest rate differentials.[4]

A concrete example is instructive. A precipitous rise occurred in the U.K. short-term investment in the United States during the period August through November of 1961. During this period the Treasury bill differential moved strongly in favor of the United Kingdom. However, the net covered yield moved in favor of the United States during June and July because the forward premium on the dollar rose. In June the forward premium on the dollar was 2.7 per cent; in July it was 3.9 per cent; in August it was 4.08 per cent; and it was 4.06 per cent in September.

The massive capital inflow into the United States was not caused by the relative rise in the U.K. Treasury bill rate. Rather, there was a disturbance which led many to believe that sterling would be devalued. The rise in the forward premium on the dollar was induced by the disturbance. As a result of the capital flow, the U.K. authorities raised interest rates to stem the speculative tide. If this interpretation is correct, and it is one that is widely held, then there are equations which determine the covered interest differential.

Regression equation (1) is an example of a predicting equation in the existing studies of capital movements. Variable y is the capital variable. If it is a stock, a rise in y is an inflow, and a fall in y is an outflow. If it is a flow, a positive y is an inflow, and a negative y is an outflow. Let x_1 be the covered interest differential, (+) = favor of dollar, (−) = favor of pound. Variable u is a disturbance term.

$$y = \beta x_1 + u. \tag{1}$$

The investigator wants to estimate the value of parameter β, the sensitivity of the capital account variable to changes in the net covered yield.

Equation (2) is an identity defining the net covered yield as the difference between the uncovered interest rate differential x_3 [(+) = favor of U.S., (−) = favor of U.K.] and the forward rate x_2 [(+) = premium on forward pound, (−) = discount on forward pound].

$$x_1 \equiv x_3 - x_2. \tag{2}$$

The forward rate x_2 is intimately related to the uncovered interest rate differential x_3. According to the interest parity theory, x_2 and x_3 tend to move closely together in normal times. However, when changes

[4] The inconsistency of the estimates applies equally to the prediction of the stock as it does to the flow. For this reason, the distinction between the stock and the flow will be dropped until later.

in the exchange rate are anticipated, the excess demand for or supply of forward exchange may drive the forward rate away from the interest rate differential. When u, in (1), is positive, there is a speculative demand for dollars which will also reduce the premium (or increase the discount) on the forward pound. Similarly, when u is negative, there is a speculative demand for pounds which will also raise the premium (or reduce the discount) on the forward pound. Equation (3) summarizes these influences and claims that x_2 is positively related to x_3 and negatively related to u.

$$x_2 = c_2x_3 - c_1u. \tag{3}$$

Substitute (3) into (2) and obtain equation (4), which relates x_1, the net covered yield, to x_3 and u.

$$x_1 = (1 - c_2)x_3 + c_1u. \tag{4}$$

An independent variable in equation (1), x_1, is correlated with the disturbance term. Hence, the least-squares estimate of β is inconsistent and biased.

The nature of this bias will be shown graphically for two points and proved mathematically for many points.

Figure 1 presents equation (1) for two values of u: u and u', where u' exceeds u.
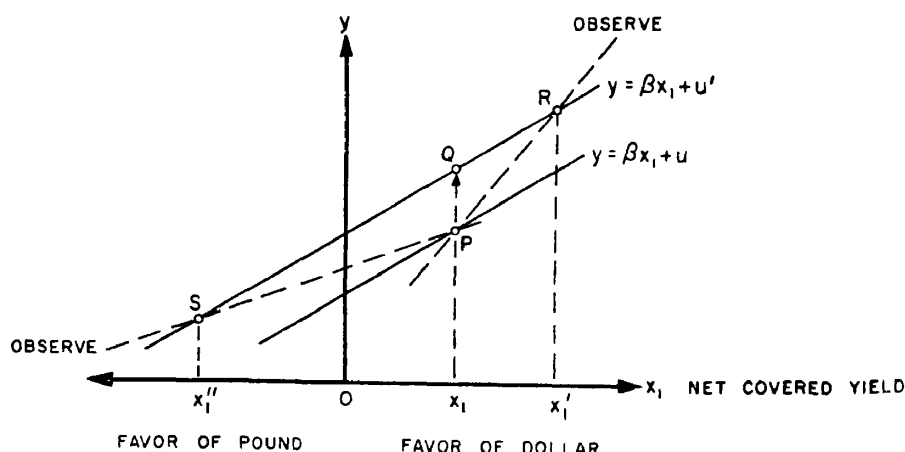
FIGURE 1

Initially, equilibrium prevails at P. Suppose there is a fear that the pound will be devalued, i.e., u rises to u'. Foreigners desire to hold more dollar assets; British exporters delay converting their dollar claims; British importers anticipate the worst and buy dollars in advance, etc. Thereby the schedule rises from P to Q. At the same time the forward

premium on the pound declines, or the forward discount on the pound increases raising the net covered yield from x_1 to x_1' (see equation (2) above). The statistician obtains PR as the slope of his least-squares regression line, whereas the true slope is given by QR. A biased estimate of β is obtained by regressing the capital flow upon the net covered yield.

When the disturbance is sufficiently large, the monetary authorities may manipulate the interest rate. Suppose that the U.K. authorities raise interest rates when the schedule moves from P to Q in such a manner that the net covered yield in favor of the dollar declines from x_1 to x_1''. Equation (4) shows the decline in x_3 necessary to achieve this result. The least-squares regression of y on x_1, in this case, is SP; whereas the true slope is given by SQ. Again a biased estimate of β is obtained.

Mathematically, the deficiency of the least-squares regression of y on x_1 is based upon the following consideration: Let

$$b = m_{y1}/m_{11} \tag{5}$$

be the least-squares estimate of β, where $m_{ij}=(1/n)\sum x_i x_j$ and the variables are measured as deviations from their means. There are two possibilities: (a) $m_{3u}=0$, i.e., the interest rate differential x_3 is independent of the disturbance term u, and (b) $m_{3u}<0$, i.e., the U.S. minus U.K. interest rate differential is negatively related to the disturbance in favor of the dollar.

When $m_{3u}=0$, then the estimate of β is inconsistent. The probability limit of b, plim b, is equation (6).

$$\operatorname{plim} b = \beta + \frac{c_1\sigma_u^2}{(1 - c_2)^2\sigma_{33} + c_1^2\sigma_u^2} > \beta. \tag{6}$$

The term σ_u^2 is the variance of u, and σ_{33} is the variance of x_3. The least-squares regression line yields an unduly high estimate of β. Hence PR has a slope greater than QR in Figure 1.

If $m_{3u}<0$, the probability limit of b is given by equation (7). In this case σ_{3u} is negative.

$$\operatorname{plim} b = \beta + \frac{(1 - c_2)\sigma_{3u} + c_1\sigma_u^2}{(1 - c_2^2)\sigma_{33} + c_1\sigma_u^2}\,. \tag{7}$$

If σ_{3u} is sufficiently great in absolute value, plim b could be less than β. The slope of SP is less than the slope of SQ.

We have proved that the estimates of the sensitivity of capital movements to interest rate changes are misleading. The reason for the bias is that the disturbance which affects the capital flow also affects the net covered yield. A confounding occurs between the effects of the disturbance and the effects of the uncovered interest rate differential upon the capital flow.

2. *Substitution effects are not detected.* An additional aspect of the recent studies that can be improved is the level of aggregation. Both authors investigated the sensitivity of *components* of the capital flow to interest rate differentials. However, when an institution, e.g., a private foreigner, purchases a U.S. Treasury bill as a result of a rise in the U.S. Treasury bill rate, the net inflow of private capital will be less than the value of the bill. As Kenen recognized, there is significant substitution of assets by and between foreign banks and other foreigners. Consider three U.S. short-term assets: dollar deposits, U.S. Treasury bills, and other assets. A schematic version of the substitution between investments in these assets is given in Figure 2.
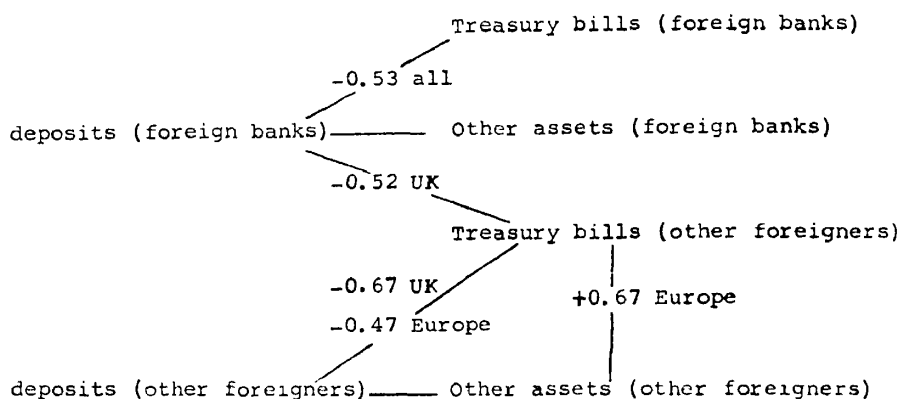
FIGURE 2

There were significant negative correlations between investment (flows) in several important classes of foreign assets.[5] There was a correlation of −0.53 (for all countries combined) between the investment of foreign banks in U.S. Treasury bills and their investment in dollar deposits. There was a negative correlation of −0.52 between the nonbank U.K. investment in U.S. Treasury bills and the U.K. bank investment in dollar deposits. Moreover, there was a negative correlation of −0.67 between U.K. nonbank investment in U.S. Treasury bills and their investment in dollar deposits. A negative correlation of −0.47 existed between European (excluding the U.K.) nonbank investment in U. S. Treasury bills and their investment in dollar deposits.

A rise in foreign investment in U.S. Treasury bills, resulting from a relative rise in U.S. Treasury bill rates, will be partially offset by a reduced rate of investment in dollar deposits. Foreign banks may even draw down their dollar deposits to sell to their customers who wish to

[5] See Kenen [3, Tables A-1, A-3, and A-5].

purchase U.S. Treasury bills. Such a phenomenon seems to have occurred with U.K. bank and nonbank investors in dollar assets.

Moreover, some of the funds which were invested by a foreign institution in Treasury bills may have been diverted from their planned investment in dollar deposits. This seems to have been the case for foreign banks (all countries combined) and for nonbank investors (United Kingdom, Europe excluding the United Kingdom).

It is misleading to estimate the sensitivity of the capital inflow to interest rate differentials by examining the interest sensitivity of various classes of assets, because we would miss the substitution effects. For example, a rise in the U.K. minus the U.S. Treasury bill differential is associated with (a) a decline in the rate of investment in "other dollar assets" (principally bankers' acceptances, commercial paper, and negotiable time certificates of deposit) by foreign banks and other foreigners, but (b) an increase in the rate of investment in dollar deposits by foreign banks and other foreigners.[6] Foreign banks may very well be performing a stabilization function by varying their dollar deposits to accommodate other foreigners' demand for money market assets. Many other examples could be cited. The purpose of this paper is to estimate the *total* effect upon the foreign exchange market of a change in the interest rate differential. Bell and Kenen, however, concentrated upon an explanation of how the various components were affected by interest rate changes. In this study, I shall use two aggregates as the dependent variables:

(A) The stock or flow of short-term liabilities to all nonofficial and noninternational foreigners, payable in dollars.

(B) The stock or flow of total U.S. short-term banking claims on foreigners.

Thereby the substitution effects will be netted out; and the stabilization (or accommodation) effects of foreign and U.S. banks will be contained in our analysis.

II. *A Theory of Short-Term Capital Movements and the Derivation of Two Estimating Equations*

A. *Stocks are the Dependent Variables*

Equilibrium in the foreign exchange market requires that the supply equal the demand for spot dollars and that the supply equal the demand for forward dollars. Demand and supply will be considered as functions of stocks. Later, we consider a flow analysis.

The supply of spot dollars is the stock of dollars existing at the end of the previous period, $S(t-1)$, plus the basic balance deficit (or minus the basic balance surplus) of the United States during the current

[6] See [3, Appendix Table B-6a].

period, plus the foreign official sales (or minus the purchases) of dollars during the period, T.

Let the basic balance (surplus + or deficit −) of the United States be a function of the dollar price of foreign exchange (p) and a disturbance term (w). The latter contains all the influences upon the basic balance surplus not contained in p. It is legitimate to simplify the analysis by using one exchange rate because of arbitrage. If the dollar were to appreciate relative to the ith currency, it would appreciate relative to every currency; and the ith currency would depreciate relative to every currency.

If X is the basic balance surplus (+) or deficit (−) of the United States, then

$$X = c_1 p + w \tag{A1}$$

is the equation determining the surplus. Linear equations will be used in this model to facilitate the subsequent statistical testing. Assumptions are prefixed by an A.

Assume that the foreign governments, or central banks, decide on the magnitude of their dollar sales or purchases each month. If the exchange rate is not at the stabilization limits, then T is determined independently of the interest rates and exchange rate. On the other hand, if the exchange rate is at one of the stabilization limits, the price of foreign exchange is given, and T adjusts to absorb the excess supply or demand for dollars of the private sector.

The supply of dollars at period t is $S(t)$.

$$S(t) = S(t-1) + T - c_1 p - w. \tag{A2}$$

There are two components of the demand for the stock of dollars. First, there is the stock that is hedged in the forward market. Hedgers will consist of short-term investors and borrowers, importers and exporters, whose spot asset (liability) is offset by a forward liability (asset). Let x_1 be the net covered yield on dollar assets. There are two components of x_1: the U.S. minus the U.K. interest differential, x_3; and the forward premium (+) or discount (−) on the foreign currency, x_2. Hence, $x_1 \equiv x_3 - x_2$. The hedged stock of dollar assets will be positively related to x_1.

Second, there is an unhedged demand for a stock of dollars which depends upon the expected appreciation of the dollar and the U.S. minus the U.K. interest differential. The expected appreciation of the dollar is $(p - p^*)$: the difference between the current price of foreign exchange (p) and the expected price of foreign exchange (p^*) prevailing when the investor wants to repatriate his funds.

A complete model would contain an equation which determines p^*

from a function of past exchange rates. I employ a different approach by directly estimating a function of p^*, and substituting this estimated magnitude into my capital movements function (see III below). I may therefore assume that p^* is exogenous.

The unhedged demand for dollars may come from foreign importers who make advance payment on imports because they fear that the dollar will appreciate, foreign investors who believe that x_3 exceeds the maximum depreciation of the dollar within the stabilization limits, etc. Equation (A3) is the demand for dollar assets $D(t)$.

$$D(t) = h(x_3 - x_2) + a_1(p - p^*) + a_3x_3. \tag{A3}$$

Each coefficient is positive. The term with the h coefficient refers to hedgers, and the terms with the a_i coefficients refer to nonhedgers.[7]

Equilibrium in the spot market requires that $S(t) = D(t)$ and is equation (8).

$$(a_1 + c_1)p - hx_2 = S(t - 1) + T + a_1p^* - w - (a_3 + h)x_3. \tag{8}$$

If the exchange rate is not at the stabilization limits, then the dependent variables are p (the price of foreign exchange) and x_2 (the forward premium (+) or discount (−) on the foreign currency). The independent variables are T (the volume of foreign official sales of dollars), x_3 (the U.S. minus the U.K. interest rate differential), and w (the disturbance term: a catchall for the other influences).

On the other hand, if the exchange rate is at one of the stabilization limits, T becomes a dependent variable, and p becomes an independent variable. This model can apply to either a stabilized or a free exchange market.

Full equilibrium in the short run requires that the forward market also be in equilibrium. The stock of forward exchange demanded by hedgers is $h(x_3 - x_2)$, which may be positive or negative.[8] If $x_1 \equiv x_3 - x_2$ is positive, hedged investment in dollar assets is profitable, and hedgers are induced to be long in forward exchange. If x_1 is negative, hedged investment abroad is profitable, and hedgers are induced to be short in forward exchange. Hedgers refer to any institution whose spot position is offset by an opposite forward position.

On the opposite side of the market will be the "speculators" who

[7] Equation (A3) is sufficiently general to account for *all* U.S. and private foreign short-term capital. A negative demand for dollar assets is the demand for foreign assets. The U.S. short-term claims on foreigners, denominated in dollars, consists of loans, collections outstanding, acceptances made for the account of foreigners, and dollars placed in the Euro-dollar market. The first three items are motivated by variables x_3, x_2, and $(p - p^*)$, where the initiative may come from foreigners. The fourth item is motivated by x_3 (the interest rate differential), and the initiative may come from Americans.

[8] The demand for forward exchange is the supply of forward dollars, and is measured in dollars.

take positions in the forward market for the sake of an expected profit
The magnitude of their position depends upon "backwardation"—th
difference between the expected price of foreign exchange and the foi
ward price. Speculators who are short forward exchange sell at a price c
$p(1+x_2)$ dollars and expect to purchase the foreign exchange later at
price of p^* dollars. Their expected profit is $p(1+x_2)-p^*$. If this terr
is negative, speculators will desire to be long forward exchange. Th
volume of forward exchange G that the speculators are short (+) c
long (−) is given by (A4), when the function has been linearized.

$$G = g_1(p - p^*) + g_2x_2. \tag{A4}$$

Equilibrium in the forward market requires that the sum of all pos
tions, by hedgers and speculators combined, must be zero. The functio
$h(x_3-x_2)$ is the long (+) or short (−) spot dollar and long (+) or shoi
(−) forward foreign exchange position of hedgers. Hence, equilibriur
in the forward market requires that $G=h$, or

$$g_1p + (g_2 + h)x_2 = hx_3 + g_1p^*. \tag{9}$$

We do not assume that forward exchange is always supplied by specu
lators. What we claim is that the net covered yield x_3-x_2 and the back

[9] The commercial demand or supply of forward exchange fits into either the hedging or tl
speculative category if it is sensitive to the forward rate, backwardation, or the interest rat
differential.

Consider the importer who always strives to protect himself by either purchasing spot o
forward, according to which method is cheaper. The costs of purchasing spot are $p(1+x_3)$
and the costs of purchasing forward are $p(1+x_2)$. The importer's demand for forward exchang
is an increasing function of (x_3-x_2), i.e., the excess costs of a spot purchase. Since such ai
importer behaves like a hedger, he is subsumed under that category.

Similarly, an exporter who always strives to protect himself by either discounting his clain
on the foreigner and selling his foreign exchange spot or by selling his foreign exchange for
ward, depending on which method is more remunerative, is behaving like a hedger. His sale
of forward exchange are an increasing function of (x_2-x_3).

The net demand for forward exchange, by commercial traders who always protect them
selves, is $h(x_3-x_2)$, $h'>0$.

On the other hand, there may be importers who either cover forward or speculate, depend
ing on the cost. The cost of a forward purchase is $p(1+x_2)$, whereas the cost of waiting unti
payment must be made is p^*. If $p(1+x_2)$ exceeds p^*, he is discouraged from purchasing forwarc
and is induced to speculate. If $p(1+x_2)$ is less than p^*, he is induced to purchase forward ex
change. His demand for forward exchange can be described by the speculative function (A4)
and he is subsumed under the speculator category.

Similarly, an exporter may either sell forward exchange for $p(1+x_2)$ or speculate on selling
for p^*. His sales of forward exchange are positively related to x_2 and negatively related to p^*.
Hence, he is behaving like a speculator in (A4).

When we claim that equilibrium in the forward market requires that the sum of all positions,
by hedgers and speculators combined, must be zero, we are encompassing the forward com-
mercial supply and demand in our equations. Those who decide between spot and forward
operations are contained in the hedging operations. Those who decide between forward opera-
tions and expected prices are contained in the speculation equations.

wardation $p(1+x_2)-p^*$ will vary until the hedging plus speculative excess demand for forward exchange will be zero.

It would appear that the positions taken in the forward market at time t will have repercussions upon the spot market in $(t+j)$ months, if the length of the forward contract were j months. The reader may wonder why equation (8) contained no such lagged repercussions. The reason is that if the profitability of a hedged position (x_1) is the same in $(t+j)$ as it was in t, the hedgers will want to renew their positions. Hedgers will demand spot the same quantity of foreign exchange that they are now delivering in fulfillment of their forward sales at t. As a result, period $(t+j)$ will be just like period t, and no price changes will be required.[10]

Writing equations (8) and (9) in matrix notation, the equilibrium conditions in the exchange market in period t are given by (10). A graphic solution of such a system and its comparative static behavior are found in [5].

Define $W \equiv S(t-1)+(T-w)$. Then the equations determining p and x_2 are:

$$\begin{pmatrix} (a_1+c_1) & -h \\ g_1 & (g_2+h) \end{pmatrix} \begin{pmatrix} p \\ x_2 \end{pmatrix} = \begin{pmatrix} W + a_1 p^* - (a_3+h)x_3 \\ g_1 p^* + hx_3 \end{pmatrix}. \tag{10}$$

The matrix is of rank 2, since the two columns are independent. The independence occurs because there is only one negative element $(-h)$ in the matrix. Hence, a unique solution for (p, x_2) exists. Only p must be positive; x_2 can be either positive, zero, or negative. If x_2 is negative, then the dollar is at a premium, i.e., the forward foreign exchange is at a discount. A zero p would make foreign goods free to U.S. citizens so that p is positive.

Solving for p and x_2, we obtain reduced form equations (11) and (12):

$$x_2 = (A_1 p^* - A_2 W) + A_3 x_3, \tag{11}$$

$$p = B_1 p^* + B_2 W - B_3 x_3. \tag{12}$$

Each coefficient (A_i, B_i) is positive.[11]

[10] The issue of the deferred effects upon the spot market is discussed in Auten [1] in his discussion of Tsiang [6], who raised this question.

If speculative expectations (p^*) at $(t+j)$ differ from those held at t, then dependent variables p and x_1 will change. Both the spot and the forward market at $(t+j)$ will differ from that prevailing at t. It was not the position taken in the forward market at t that produced repercussions on the spot market at $(t+j)$. What produced the changes in the spot market at $(t+j)$ was the change in the values of the independent variables at $(t+j)$ relative to their values at t.

[11] The determinant of the matrix is $J=(a_1+c_1)(g_2+h)+g_1 h$.

(a) $A_1 = c_1 g_1/J$

(b) $A_2 = g_1/J$

(c) $A_3 = [(a_1+c_1)h+(a_3+h)g_1]/J$.

Equations (11) and (12) explain the differences between speculative and normal periods which I have developed elsewhere [4]. A speculative period is one where the price of foreign exchange is expected to change, $p^* \neq p$. If p^* rises, the foreign currency is expected to appreciate, and both the spot price (p) and the forward premium on the foreign currency will rise. This conclusion results from the fact that A_1 and B_1 are positive in equations (11) and (12).

The coefficients of x_3 indicate the effects of a rise in the U.S. interest rate relative to the foreign rate. The forward premium on the foreign currency will rise (A_3 is positive); but the spot price of the foreign currency will fall ($-B_3$ is negative). It will be shown in the statistical section of this paper that the forward rate does not change by the amount of the interest rate differential.

There is another disturbance W which enters the model. If there is a seasonal rise in U.S. exports, W falls. If there is a rise in foreign official sales of dollars, W rises. The solution of the model claims that, if W rises, there will be an increase in the spot price of foreign exchange ($\partial p/\partial W = B_2$) and a fall in the forward premium (rise in discount) on the foreign exchange ($\partial x_2/\partial W = -A_2$). Except for speculative periods when there is a change in p^*, there is a negative relation between the changes spot price and change in the forward premium on the foreign exchange.

The desired stock of dollars (A3) is equal to the actual stock of dollars given by (A2). Hence,

$$S(t) = W - c_1 p = D(t).$$

Substitute the value of p from reduced form equation (11) into the above expression to obtain reduced form equation (13). Here, the stock of dollars is a function of three variables: the interest rate differential x_3, the expected price of foreign exchange p^*, and W. Each coefficient is positive.[12]

Since $W \equiv S(t-1) + T - w$, we could also write the reduced form equation for the desired stock of dollars as (14). In this formulation $S(t-1)$ is explicitly introduced as an independent variable.

Hence A_i is positive.

(d) $B_1 = [a_1(g_2+h) + g_1 h]/J$.
(e) $B_2 = (g_2+h)/J$.
(f) $B_3 = [h(g_2+a_3) + a_3 g_2]/J$.

Hence B_i is positive.

[12] (a) $K_1 = c_1 B_1$
(b) $K_2 = 1 - c_1 B_2 > 0$
(c) $K_3 = c_1 B_3$

$$S(t) = K_3x_3 - (K_1p^* - K_2W) \tag{13}$$

$$S(t) = K_3x_3 + K_2S(t-1) - [K_1p^* - K_2(T - w)]. \tag{14}$$

Interest rate differential x_3 is measurable. However, component w is not measurable, and p^* is completely latent.

A comparison of reduced form equation (13) with reduced form equation (11) suggests a method of estimating the latent variables. From (11), regress x_2 on x_3 and derive the residual (R), the difference between the actual and predicted value of x_2. This residual will be a function of $(A_1p^*-A_2W)$ and will be independent of x_3 (because of the nature of a least-squares regression). Hopefully, residual R will be positively correlated[13] with $Q \equiv K_1p^*-K_2W$ in equation (13). Insofar as R and Q are positively correlated, R will serve as a proxy for all of the independent variables other than x_3. Part of W is measurable—the stock existing at the end of the previous period. We therefore use as our independent variables, in a regression predicting $S(t)$, three dependent variables: x_3, the U.S. minus the U.K. interest rate differential; $S(t-1)$, the previous period stock; and $R(t)$, the residual from a regression of x_2 on x_3 which will capture the effects of $(T-w)$ and p^*. Variables $S(t-1)$ and $R(t)$ were not significantly correlated in estimating equation (15).[14]

$$S(t) = b_0 + b_1S(t-1) + b_2R(t) + b_3x_3(t). \tag{15}$$

B. *Flows are the Dependent Variables*

An alternative theoretical formulation can be constructed to explain the movement of short-term capital. In this formulation, the interest rate differential determines the desired *flow* of capital rather than a stock. There is a formal symmetry between this flow approach and the stock approach discussed in Part II, Section A. Since these flow and stock models are consistent in structure, much of the economic analysis need not be repeated.

There are three equations in the flow model. First, there is the balance of payments constraint: the basic balance (X), plus the short-term

[13] If $R=A_1p^*-A_2W$ and $Q=K_1p^*-K_2W$, then the correlation between R and Q, call it r, depends upon r^* = correlation between p^* and W, σ_1 = standard deviation of p^*, and σ_2 = standard deviation of W.

$$r = \frac{A_1K_1\sigma_1^2 + A_2K_2\sigma_2^2 - (A_1K_2 + A_2K_1)\sigma_1\sigma_2r^*}{[A_1^2\sigma_1^2 + A_2^2\sigma_2^2 - 2A_1A_2\sigma_1\sigma_2r^*]^{1/2}[K_1^2\sigma_1^2 + K_2^2\sigma_2^2 - 2K_1K_2\sigma_1\sigma_2r^*]^{1/2}}$$

There is no assurance that R and Q will be highly correlated positively.

[14] This lack of correlation is probably another weakness of the stock model, as compared with the flow model. Residual R is a function of $A_1p^*-A_2[S(t-1)+T-w]$; hence one would expect a correlation between R and $S(t-1)$, part of itself. It will be seen in Part III that the flow model seems to yield better empirical results than the stock model.

capital flow (y), minus the foreign official sales of dollars (T), must total to zero. Since (A1) describes the basic balance, the equation for the short-term capital flow is (16):[15]

$$y = W^1 - c_1 p, \quad \text{where} \quad W^1 \equiv T - w. \tag{16}$$

When y is positive, there is an inflow; when y is negative, there is an outflow.

The inflow or outflow of short-term capital is either hedged or unhedged. We follow *exactly* the analysis of Part II, Section A in connection with hedging and speculation, *except* that the dependent variables are flows rather than stocks. The demand for the *flow* of dollars is given by (A3), and it must equal the actual *flow* of capital y, given by (16). Equation (17) states the equality between the actual and the desired flow of short-term capital:

$$h(x_3 - x_2) + a_1(p - p^*) + a_3 x_3 = W^1 - c_1 p. \tag{17}$$

The forward market will be in equilibrium when the *flow* of forward exchange demanded equals the *flow* of forward exchange supplied. The excess demand of hedgers is $h(x_3 - x_2)$, and the excess supply of speculators is $g_1(p - p^*) + g_2 x_2$, where supply and demand are now flows. Hence we derive *flow* equation (18) which describes equilibrium in the forward market:

$$g_1(p - p^*) + g_2 x_2 = h(x_3 - x_2). \tag{18}$$

The flow equations (17) and (18) are exactly the same as the stock equations (8) and (9), with the substitution of W^1 for W. Hence x_2 and p are solved by equation (10) with the substitution of W^1 for W. The reduced form solutions are:

$$p = B_1 p^* + B_2 W^1 - B_3 x_3, \tag{19}$$

$$x_2 = (A_1 p^* - A_2 W^1) + A_3 x_3, \tag{20}$$

where A_i and B_i are the same as in IIA.

We may solve for y, the short-term capital flow, by substituting (19) into (16) to obtain (21).

$$y = K_3 x_3 - (K_1 p^* - K_2 W^1), \tag{21}$$

where K_i has the same meaning as in (13).

To estimate $(K_1 p^* - K_2 W^1)$ in equation (21), we use the residual of a regression of x_2 on x_3. This residual should be a function of $(A_1 p^* - A_2 W^1)$ in equation (20). Hopefully, this residual R is highly correlated with $(K_1 p^* - K_2 W^1)$ in equation (21).

[15] $X + y - T = 0$. Since $X = c_1 p + w$, we obtain $c_1 p + w - T + y = 0$. Solving for y, we obtain (16).

Our estimating equation in the flow analysis is (22). Dependent variable y = capital flow is a function of interest rate differential x_3 and the residual R. No lagged variables enter in this analysis.

$$y(t) = d_0 + d_1R(t) + d_3x_3(t). \tag{22}$$

By the property of a least-squares regression, x_3 and the residual R are uncorrelated.

We want to determine which is a better predictor of capital movements: stock equation (15) or flow equation (22).

C. *The Expected Price Change is Exogenous*[16]

In the stock and flow models described above, the expected price p^* was considered exogenous; but $(p-p^*)$, the expected price *change*, is endogenous since p is determined by the model.

It is possible to construct a model where $(p-p^*)$ is exogenous. Let Z be $(p-p^*)$, i.e., a proxy for speculative anticipations. If Z is positive, the dollar is expected to appreciate; if it is negative, the dollar is expected to depreciate. Let y be the short-term flow of capital: $(+)$ = inflow, $(-)$ = outflow. Let y be a function of the net covered yield (x_3-x_2), the uncovered yield x_3, and a proxy for speculative anticipations Z. Equation (23) is the short-term capital movements function.

$$y = h(x_3 - x_2) + a_1Z + a_3x_3. \tag{23}$$

The dependent variable y is a flow. The current exchange rate was explicitly considered in (A3); but is subsumed under the exogenous variable Z in equation (23).

The flow of forward exchange demanded (or supplied) by hedgers is $h(x_3-x_2)$. The flow of forward exchange supplied (or demanded) by speculators is

$$G = g_1x_2 + g_2Z.$$

Given Z, a rise in x_2 raises the backwardation and increases the flow of forward exchange offered by speculators. A rise in Z means that the dollar is expected to appreciate relative to the foreign exchange. Speculators are then induced to sell foreign exchange forward. Equilibrium in the forward market requires that the hedgers demand as much forward exchange as the speculators are supplying. This is equation (24).

$$h(x_3 - x_2) = g_1x_2 + g_2Z. \tag{24}$$

In this formulation, the forward rate can be obtained directly from (24) and is reduced form equation (25).

[16] Peter Kenen was most helpful in suggesting this alternative formulation.

(25)
$$x_2 = \frac{h}{g_1 + h} x_3 - \frac{g_2}{g_1 + h} Z.$$

The forward rate (+ = premium on pound, − = discount on pound) is positively related to the U.S. minus the U.K. interest differential x_3; and it is negatively related to the expected appreciation of the dollar relative to the pound.

Substitute (25) into (23) and obtain y, the short-term flow of capital: equation (26).

(26)
$$y = C_1 x_3 + C_2 Z$$

where

$$C_1 = \frac{g_1(a_3 + h) + a_3 h}{g_1 + h}$$

and

$$C_2 = a_1 + \frac{g_2 h}{g_1 + h} \cdot$$

Both coefficients are positive.

To estimate C_2Z, in equation (26), we use the same method as described in IIB. Regress x_2 on x_3 and obtain the residual R, which is now considered a function solely of

$$\frac{-g_2}{g_1 + h} Z$$

in (25). Insert this residual into (26) as our estimate of a function of C_2Z.

Clearly C_2Z and $-g_2Z/(g_1+h_1)$ will be very highly correlated, so that the residual is an excellent proxy variable. By the property of a least-squares regression, x_3 and the residual R are uncorrelated.

Our estimating equation, in the flow analysis, is

(27)
$$y(t) = d_0^1 + d_1^1 R(t) + d_3^1 x_3(t),$$

which is formally identical with the estimating equation derived from the flow model in IIB. Several different flow models yield the same type of estimating equation for the short-term capital flow.

III. *Statistical Analysis*

Private investors the world over are assumed to have only two choices in holding international money: dollar assets or sterling assets. There will be a short-term capital inflow into the United States if dollar

assets rise in attractiveness relative to sterling assets; and there will be a short-term capital outflow if the reverse case is true. This oversimplification excludes the role of gold speculation, the mark, the Swiss franc, the lira, and the French franc. Hence, the explanatory power of the theory is weakened. However, theoretical simplicity was deemed an important consideration.

There are two aims in this statistical section. First, we want to decide which model of capital flows is inconsistent with the data. Is it the stock model, eq. (15), or the flow model, eq. (22)? Second, when the substitution effects are netted out, how sensitive is the flow of capital to interest rate changes?

The reader is reminded that, to net out the substitution effects, huge and heterogeneous flows of short-term capital have been combined into two categories: U.S. capital and private foreign capital. What is lost by this attempt to net out the substitution effects is the explanation of particular component flows of capital. An aggregative approach is used here because it is the only way of answering the question: what is the *total* effect on capital flows of a given interest rate differential when we take account of a chain of substitution effects?

The flow of capital is described by (22), whether it refers to the flow of U.S. short-term capital or to the flow of private foreign short-term capital. Similarly, the stock of capital is described by (15), whether it refers to the stock of U.S. short-term capital or to the stock of private foreign short-term capital. There is no a priori reason why the coefficients of the U.S. capital equation should be the same as those of the foreign private capital equation, since different parties are involved. Consequently, each regression was performed twice: once for U.S. capital and once for foreign private capital.

A. *Data*

As my interest rate variable I used the Treasury bill rates in the United States and the United Kingdom. There are several reasons for this choice. First, these are policy variables which can and will be influenced by the monetary authorities who want to vary the short-term private capital flows. Any study of the impact of monetary policy upon the international flows of short-term private capital must contain the policy variables as independent variables. I used the yield on new issues in computing the Treasury bill differential. Similar results are obtained by using the market yields in equation (11). Second, there is an extremely high correlation (indicated by the figure in parentheses) between the U.K. minus the U.S. Treasury bill differential and the following:[17] (i) U.K. acceptance rate less U.S. acceptance rate (0.99), and

[17] Kenen [3, Table C].

(ii) U.K. acceptance rate less U.S. short-term bank loan rate (0.98). A smaller but high correlation existed between the Treasury bill differential and (iii) Euro-dollar deposit rate (London) less U.S. Treasury bill rate (0.79).

To obtain an estimate of the latent variables, in either the stock theory eq. (11) or the flow theory eq. (25), I regressed the forward rate on the interest rate differential; and I then calculated the residual, the actual minus the predicted rate.

The dependent variable was the forward rate x_2', the negative of x_2. If x_2' is positive, the dollar is at a premium; if it is negative, the pound is at a premium. The independent variable was the interest differential $I=-x_3$: the U.K. minus the U.S. Treasury bill rates. If I is positive, interest rates are higher in the United Kingdom. If I is negative, interest rates are higher in the United States. The period covered was November 1958–December 1962. Forward rate data were taken from the *Economist;* Treasury bill data were taken from the *Federal Reserve Bulletin.*

Equation (28) is the derived regression equation which corresponds to either equation (11) or (25) in the theory.

$$x_2' = \underset{(0.048)}{0.865I} - \underset{(0.093)}{0.002}, \qquad r = 0.934. \tag{28}$$

Speculative pressure is reflected in the residual. When the residual R is positive, the pressure is in favor of the dollar. When R is negative, pressure is in favor of sterling. Table 1 presents the residuals for the period.

The speculative attacks on the dollar in 1960 are reflected by the negative residuals during that period. Similarly, the massive sterling crisis in June and July, 1961, is reflected in the magnitude of the positive residuals.

The interested reader may wonder if the forward rate can be expected to change by the same amount as the interest rate differential: is the regression coefficient of I significantly different from unity? Equation (28) cannot be used to answer this question, for the residuals are very highly correlated. As a result of this significant autocorrelation, misleading estimates are obtained of the standard errors of the regression coefficients.

If $R(t)$ is the residual in period t, then equation (29) indicates the high degree of autocorrelation.

$$R(t) = \underset{(0.097)}{0.743R(t-1)} - \underset{(0.042)}{0.008}, \qquad r = 0.75. \tag{29}$$

To obtain an efficient estimate of the relation between the forward

TABLE 1—THE DIFFERENCE BETWEEN THE ACTUAL AND PREDICTED FORWARD RATES ON THE DOLLAR RELATIVE TO THE POUND
November, 1958–December, 1962: Residual

November, 1958	0.2958%	December, 1960	−0.7498
December	0.0283	January, 1961	−0.6266
January, 1959	0.1898	February	−0.6708
February	0.1057	March	0.0266
March	0.1342	April	0.0511
April	0.5128	May	0.0519
May	0.1494	June	0.8515
June	−0.0450	July	1.4422
July	−0.1581	August	0.3591
August	−0.0420	September	0.3455
September	0.0523	October	0.1959
October	0.0794	November	0.1974
November	−0.0795	December	0.4337
December	0.2641	January, 1962	0.4303
January, 1960	0.0099	February	0.3906
February	−0.1988	March	0.5755
March	−0.4566	April	0.5811
April	−0.2820	May	0.0453
May	−0.1846	June	−0.0421
June	−0.6732	July	0.0679
July	−0.6471	August	−0.0084
August	−0.8062	September	−0.1169
September	−0.3565	October	−0.1916
October	−0.5910	November	−0.3395
November	−0.4993	December	−0.1033
		Mean	0.0000

rate and the interest rate differential, X_2' and I were transformed to $X_2'(t)-0.743X_2'(t-1)$ and $I(t)-0.743I(t-1)$, respectively; and a regression was run on the transformed variables. The resulting regression is equation (30).

$$(Tx_2') = \underset{(0.081)}{0.740}(TI) + \underset{(0.051)}{0.039}, \qquad r = 0.80 \tag{30}$$

Tx_2' = transformed x_2'; (TI) = transformed I.

The regression coefficient of the forward rate on the interest rate differential is 0.74 and is significantly less than unity. If the forward rate is determined by equation (24), then the ratio of g_1/h in (25) is determined.

$$\frac{h}{g_1+h} = \frac{1}{g_1/h+1} = 0.74,$$

which implies that $(g_1/h)=0.35$. The hedgers' sensitivity to the net covered yield, h, is three times as great as the speculators' sensitivity to

backwardation, g_1. A change in the interest rate differential will only be partially (74 per cent) offset by a change in the forward rate.

B. *The Short-Term Capital Movements Function*

There were four dependent variables in the regression analysis:

SX = the stock at the end of the month of short-term banking liabilities to private (i.e., nonofficial and noninternational) foreigners, payable in dollars.[18]

X = the corresponding flow. $X(t) \equiv SX(t) - SX(t-1)$, since the stocks are given as end-of-month figures.

SY = the stock at the end of the month of U.S. short-term banking claims on foreigners.[19]

Y = the corresponding flow. $Y(t) \equiv SY(t) - SY(t-1)$.

These variables are measured in millions of dollars.

Let us consider the categories of foreign private short-term capital and U.S. short-term capital separately.

1. *Foreign private short-term capital payable in dollars.* According to the stock theory, eq. (15), the stock of foreign private capital is a function of its lagged value $SX(t-1)$, the residual $R(t)$, which is the actual minus the predicted forward premium (+ = favor of the U.S. dollar), and the short-term interest differential (+ = favor of the U.K.): $I(t)$.

The regression of $SX(t)$ on these variables is equation (31) below:

$$(31)\quad SX(t) = 0.9275SX(t-1) + 159.5691R(t) + 4.3021I(t) + 566.0092$$

S.E.[20] =	(0.0387)	(55.0271)	(20.1237)
F[21] =	(574.02)	(8.41)	(0.05)
r[22] =	0.9712		

[18] *Treasury Bulletin*, Capital Movements. Section I, Table II: Short-term banking liabilities to other foreign payable in dollars. The data were taken from the following issues:

Issue	*Data*
a) Dec., 1962:	Jan., 1962–Oct., 1962
b) Aug., 1962:	July, 1961–Dec., 1962
c) Feb., 1962:	Jan., 1961–June, 1961
d) Aug., 1961:	July, 1960–Dec., 1960
e) Feb., 1961:	Jan., 1960–June, 1960
f) Aug., 1960:	July, 1959–Dec., 1959
g) Feb., 1960:	Jan., 1959–June, 1959
h) June, 1959:	Sept., 1958–Dec., 1958

The period November, 1958, through October, 1962, was covered, giving 48 observations.

[19] *Treasury Bulletin*, Capital Movements. Section III, Table 2: Short-term Banking Claims on Foreigners: Total short-term claims.

[20] Standard error of the regression coefficient.

[21] F is t^2.

[22] r is the multiple correlation coefficient.

Since the interest rate variable is not significant, regress $SX(t)$ on the remaining two independent variables and obtain equation (32).

$$\begin{aligned}(32)\quad SX(t) &= 0.9311SX(t-1) + 159.1928R(t) + 546.4275\\ \text{S.E.} &= (0.0345) \qquad (54.4126)\\ F &= (729.23) \qquad (8.56)\\ r &= 0.9712\end{aligned}$$

Each independent variable is statistically significant at the 1 per cent level, and 94 per cent of the variance of $SX(t)$ is explained by $SX(t-1)$ and $R(t)$. The speculative variable is extremely important, but the interest rate is not important, in explaining the stock of private foreign short-term capital.

The flow theory claims that $X(t)$, the flow per month of liabilities to private foreigners, is a function of the residual[23] $R(t)$ and the interest differential $I(t)$. Regress $X(t)$ on $X(t-1)$, $R(t)$, and $I(t)$ to obtain equation (33). Variable $X(t-1)$ is not an integral part of the flow theory.

$$\begin{aligned}(33)\quad X(t) &= -0.2357X(t-1) + 184.1797R(t) - 12.3270I(t) + 77.1702\\ \text{S.E.} &= (0.1457) \qquad (59.0530) \qquad (18.2584)\\ F &= (2.62) \qquad (9.73) \qquad (0.46)\\ r &= 0.4399\end{aligned}$$

Only the "residual," i.e., speculative variable, is statistically significant. Hence, regress $X(t)$ on $R(t)$ to obtain equation (34).

$$\begin{aligned}(34)\quad X(t) &= 151.3429R(t) + 48.0359\\ \text{S.E.} &= (56.0219)\\ F &= 7.30\\ r &= 0.37\end{aligned}$$

The speculative variable is significant at the 1 per cent level. In contrast to the stock theory, the multiple correlation coefficient is low. In no regression equation is the Durbin-Watson statistic suggestive of autoregressivity in the disturbance term.

2. *U.S. short-term capital.* The stock theory claims that $SY(t)$, the stock of U.S. short-term capital, is a linear function of $SY(t-1)$, $R(t)$, and $I(t)$. Regression equation (35) is a test of this theory.

$$\begin{aligned}(35)\quad SY(t) &= 1.0005SY(t-1) - 43.8306R(t) + 23.765I(t) + 11.5762\\ \text{S.E.} &= (0.0208) \qquad (37.9192) \qquad (13.8128)\\ F &= (2324.79) \qquad (1.34) \qquad (2.96)\\ r &= 0.9941\end{aligned}$$

[23] The same statistical residual is used in both stock and flow theories, but their economic meaning is so[illegible]at different.

Drop the speculative variable, which is the least significant variable, and regress $SY(t)$ on $SY(t-1)$ and $I(t)$ to obtain equation (36).

$$(36) \quad \begin{aligned} SY(t) &= 0.9918SY(t-1) + 27.0089I(t) + 36.2692 \\ \text{S.E.} &= (0.0194) \qquad\qquad (13.5747) \\ F &= (2614.56) \qquad\qquad (3.96) \\ r &= 0.9939 \end{aligned}$$

The interest rate variable is significant at (almost) the 5 per cent level. The higher the relative yield of U.K. securities, the greater the stock of claims against foreigners held by Americans. Of course, the major explanatory variable is the lagged value of the stock of claims. In no equation is the Durbin-Watson statistic suggestive of autoregressivity of the disturbance term.

According to the flow theory, the change in U.S. short-term claims against foreigners $Y(t)$ is a linear function of $R(t)$ and $I(t)$. A test of the flow theory is given in equation (37). The variable $Y(t-1)$ is not statistically significant and has therefore not been used. Moreover, it is not part of the flow theory.

$$(37) \quad \begin{aligned} Y(t) &= -51.9627R(t) + 23.1371I(t) + 13.2214 \\ \text{S.E.} &= (34.7920) \qquad (11.4715) \\ F &= (2.23) \qquad\qquad (4.07) \\ r &= 0.3518 \end{aligned}$$

The interest rate differential $I(t)$ is significant at the 5 per cent level and is the single most important variable in explaining the U.S. short-term capital outflow. Although the speculative variable has the correct sign, it is not statistically significant. Again, there is no evidence of the existence of an autoregressive structure.

The interest rate variable is important in explaining the U.S. capital account, and the residual, or speculative index, is important in explaining the private foreign short-term capital account.

3. *Stocks or flows*. The high multiple correlation coefficients obtained in the tests of the stock theory were due to the inclusion of the previous month's stock in the regression equation. Such inclusion was required by the theory, eq. (15), and was not an arbitrary device. In the flow regression equations the previous month's flow was neither statistically significant nor contained in the theory.

To put the theories on a comparable level, let us examine the ability of each theory to explain *flows* of short-term capital. The flow theory is described by equation (22). To explain flows on the basis of the stock theory, take first differences in equation (15) and obtain equation (38).

Since the previous period's flow was already found to be unimportant in explaining current period flows, it was excluded from (38).

$$\Delta S = K_1 \Delta R - K_2 \Delta I. \tag{38}$$

That is, the stock theory claims that ΔR and ΔI are the important variables in explaining short-term capital flows. On the other hand, the flow theory claims that R and I are the important variables in explaining capital flows. Our approach is to include all four variables, ΔR, R, I, and ΔI, in a regression equation explaining flows and see which variables are not statistically significant. In this way, we hope to see which theory is inconsistent with the data.

First, let us examine (X), the flow of short-term dollar liabilities to private foreigners, all countries combined. Regress the dependent variable (X) upon ΔR, R, I, and ΔI to obtain equation (39). X is measured in millions of dollars per month; the other variables are measured in per cent per annum.

$$\begin{array}{llllll} (39)\quad X & = 8.6734\Delta R & +153.3569R & -15.4290I & +55.6881\Delta I & +70.8476 \\ \text{S.E.} & = (90.6558) & (60.9971) & (19.0657) & (59.6374) & r=0.4041 \\ F & = (0.01) & (6.32) & (0.65) & (0.87) & \end{array}$$

The variables ΔR and ΔI, which reflect the stock theory, are not statistically significant in explaining the private foreign short-term capital flow. We may therefore reject the stock theory.

Variables R and I are the explanatory variables in the flow theory. The residual R, which reflects the speculative pressures, is statistically significant at the 5 per cent level. When R is positive, the dollar is expected to appreciate relative to the pound, and a foreign private capital inflow occurs.

The sign of the interest rate variable I is correct. The higher U.K. interest rates relative to the U.S. interest rate, the smaller the private capital inflow. With a one percentage point increase in the U.S. Treasury bill rate, *ceteris paribus*, the private foreign short-term capital inflow would rise by $(15.4)(12) = \$185$ million per year. However, the regression coefficient is not significantly different from zero, so that this magnitude could easily be attributable to a sampling error.

If we regress X on the only significant variable R, we obtain equation (40).

$$\begin{array}{ll} (40)\quad X = 151.3429R + 48.0359 & \\ \qquad\qquad (56.0219) & r = 0.37 \end{array}$$

There will be a private foreign short-term capital inflow of $(48)(12) = \$576$ million per year, if there are no speculative pressures. The exist-

ence and magnitude of the speculative pressure are measured by the difference between the actual premium on the forward dollar and the premium predicted by regressing $(-x_2)$ on $(-x_3)$. A one percentage point residual will change the flow by (151)(12)=\$1,812 million per year. With a residual of 1.44 per cent in July, 1961, the capital flow is expected to rise by (151)(1.44)(12)=\$2,610 million per year! The most important influence explaining variations in the flow of private foreign short-term capital into dollars rather than into sterling (or gold?) is the speculative anticipation of changes in the exchange rate. It should also be noted that the Durbin-Watson statistic is quite large, and we can reject the view that there is autocorrelation in the disturbance terms.

Second, let us examine the flow of U.S. short-term dollar claims on foreigners. Again the flow theory is the better explanation, and the interest rate differential is the single most important explanatory variable. Regress Y on ΔR, R, I, and ΔI to obtain equation (41). Variable Y is measured in millions of dollars per month. A rise in Y means an *increase* in the U.S. short-term capital *outflow*.

$$(41) \quad Y = \underset{(55.0868)}{77.8831\Delta R} - \underset{(37.0648)}{71.6640R} + \underset{(11.5852)}{24.2636I} - \underset{(36.2385),}{3.3605\Delta I} + 12.5299 \qquad r = 0.41$$

Clearly the variables contained in the stock theory, ΔR and ΔI, are not statistically significant. Drop them and regress Y upon R and I to obtain equation (37).

$$(37) \quad Y = -\underset{(34.7920)}{51.9627R} + \underset{(11.4715),}{23.1371I} + 13.2214 \qquad r = 0.35.$$

The interest rate differential is the most important explanatory variable, and it is statistically significant. The probability of obtaining a value of t greater than 2.1 in sampling from a population where there was no relation between Y and I is less than 0.025. A one percentage point rise in the U.S. Treasury bill rate, *ceteris paribus*, would decrease the U.S. short-term capital outflow by (23)(12)=\$276 million per year. The repercussions in the forward market of a change in interest rates have already been taken into account.

The speculative variable R has the correct sign. However, the value of t is -1.5. With random sampling from a population with no relation between Y and R, the probability of obtaining an algebraically smaller value is less than 10 per cent.

Again, the Durbin-Watson statistic is sufficiently high to rule out the presence of autocorrelated disturbance terms.

Table 2 summarizes the statistical findings. Row 1 indicates the capital flow that would occur if interest rates were the same in both

TABLE 2—THE FLOWS OF SHORT-TERM CAPITAL UNDER ALTERNATIVE ASSUMPTIONS

	(1) U.S. Capital[a]	(2) Foreign Private Capital[b]	(3) Col. (1) plus Col. (3)
$R=0$, $I=0$	OUTFLOW $159 mil. per annum	INFLOW $850 mil. per annum	INFLOW $691 mil. per annum
$R=-1$, $I=0$	OUTFLOW $783 mil. per annum	OUTFLOW $990 mil. per annum	OUTFLOW $1,773 mil. per annum
$R=0$, $I=1$	OUTFLOW $436 mil. per annum	INFLOW $665 mil. per annum	INFLOW $229 mil. per annum

[a] Based upon equation (37). Numbers rounded.
[b] Based upon equation (39), with $\Delta R=\Delta I=0$. Numbers rounded.

countries ($I=0$), and there were no speculative pressure ($R=0$). Row 2 indicates the flows that would occur if R were -1 per cent, i.e., if the forward premium on the dollar were one percentage point less than predicted from the regression of the forward premium on the interest differential. Interest rates are assumed to be the same in both countries.

Row 3 is obtained by setting $R=0$ and $I=1$. It indicates the flows that would occur if the U.K. interest rate were one percentage point higher than the U.S. rate, and there were no speculative pressures.

Column 3 indicates the effect of each situation upon the annual quantity of dollars demanded in the foreign exchange market per year.

If there is no interest differential or expectation of exchange rate changes, there is a $159 million U.S. short-term capital outflow and an $850 million private foreign short-term capital inflow: a net inflow of $691 million. If U.S. interest rates were lowered by one percentage point, *ceteris paribus*, the net inflow would fall to $229 million per annum, a decline of $462 million. The U.S. outflow would be $436 million, and the private foreign capital inflow would be $665 million.

If there were no interest differential and the residual R were -1, then the magnitude of the capital outflow would be astounding. There would be a U.S. capital outflow of $783 million and a foreign private short-term capital outflow of $990 million. The total outflow would be $1,773 million per annum.

4. *Conclusions*. (a) Bell found that the interest rate differential was not a significant variable in explaining the stock of U.S. capital; but I found it to be a significant variable (see eq. (36) above). One possible reason for our different results is the level of aggregation. For many components of U.S. capital, the interest rate variable is not significant. In some cases, it even has the wrong sign. I account for these results by claiming that there are substitution effects among assets. When all the components of U.S. capital are aggregated, the substitution effects are

netted out; and the interest rate variable is significant. A more important reason is that he failed to take account of the speculative expectations which I took into account with my *R* variable.

(b) My speculative variable was highly significant in explaining both the stock and the flow of private foreign short-term capital. However, the interest rate effect was not significant in the foreign capital account.

(c) It seems that more fruitful empirical results are obtained by considering the interest rate differential and the speculative variable (the residual) as determinants of the allocation of a *flow* of international capital among countries, rather than as determinants of the allocation of a *stock* of capital among countries. But more work remains to be done on this intriguing subject.

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A THEORY OF THE BARGAINING PROCESS

By JOHN G. CROSS*

Economists traditionally have had very little to say about pure bargaining situations in which the outcome is clearly dependent upon interactions among only a few individuals. Except for a few models which have been based upon strong institutional assumptions, we have had to fall back on Edgeworth's model of bilateral monopoly in which only a "trading area" is delimited with no further restriction of the outcome. Within this area, the solution is said to depend upon the "bargaining abilities" of the individuals, a thoroughly vague concept which is primarily intended to give the whole problem to the psychologists, thus absolving economics of the guilt of leaving the issue up in the air.[1] It is not obvious, however, that the economist's techniques are inadequate for a solution, although the few attempts that have been made have not been met with appreciable success. It is the purpose of this paper to offer a precise analysis of bargaining by means of a positive theory based upon familiar economic concepts. It is hoped that this work will prove to be operational in the sense that the independent and dependent variables may be related to familiar quantities which are already commonly accepted as relevant in the context of bargaining.

It would not be useful for us to enter into any analyses of the extensive literature which relates to this subject; nevertheless there is need for some consideration of the Nash theory and the recent empirical work of Siegel and Fouraker, since an explicit attempt is made to relate our model to their conclusions.

* The author, a member of the staff of the Institute for Defense Analysis, would like to thank William J. Baumol and Harold W. Kuhn for their valuable advice and comments on this paper.

[1] The literature abounds, however, with quasi-bargaining theories introduced in various attempts to fill the gaps in our understanding of oligopolistic markets. There are, moreover, several models which address themselves directly to the bargaining problem (i.e., bilateral monopoly). Besides those mentioned in the text, the most notable are those of F. Zeuthen [19] [20], J. Harsanyi [5], J. Pen [10] [11], J. R. Hicks [6], L. Foldes [4] (this is an outgrowth of the Hicks model), and R. L. Bishop [forthcoming in *Econometrica*]. It would not be fruitful to go into these models here, however, both because extensive criticisms already exist elsewhere (especially see Bishop [1]) and because they have little direct bearing on what is to follow. It should be observed, however, that the contribution of this paper stems from the explicit introduction of expectations into a model similar to Hicks's.

I. *The Nash Theory*

The most precise (but not descriptive) of previous theories is certainly that of J. Nash [9]. Nash conceived of the bargaining problem purely in terms of the increments in utility which each negotiator receives from each point of agreement. Thus "zero" utility for each negotiator relates to the point of total disagreement. Nash then assumed that it is reasonable to expect the outcome of a bargaining process to satisfy the following three conditions:

1. Pareto optimality—the outcome will lie on the northeast boundary of the utility-possibility set (this boundary necessarily has a negative slope).

2. Independence of irrelevant alternatives—consider two different bargaining situations with the same origin (i.e., the same disagreement point), and in which all the possible outcomes of one are included in the other; then, if the actual outcome of the larger game is also a possible outcome in the smaller, it will be the final outcome for the smaller game as well.

3. Symmetry—if the set of possible utility outcomes happens to be symmetric (that is, if for every point $U_1=a$, $U_2=b$ in the outcome set, the point $U_1=b$, $U_2=a$ is also in the set), then the outcome of the bargaining will give $U_1=U_2$. (Again, we are taking U_1 and U_2 as utility increments, assuming $U_1=0$, $U_2=0$ at the point of disagreement.)

Nash chose to formulate a model in which interpersonal comparisons were not a factor. Thus, the predicted agreement point must be independent of any interpersonal utility comparisons whether valid or not. This assumption required the model to obey a fourth rule.

4. The model must predict the same actual outcome despite any linear transformations of the players' utility functions.[2]

On the basis of these four conditions, Nash came to the remarkable conclusion that the only function which can consistently describe the outcome of the bargaining process as he conceived it is the one that maximizes the product of the players' utilities (the utilities being measured as increments from the point of disagreement).[3] Nash originally

[2] Note that this assumption also strengthens condition (3) to state that whenever there exists any linear utility transformation which can make the outcome set symmetric, we must have $U_1=U_2$ with the utilities expressed in terms of the same utility transformation.

[3] The foregoing description of the Nash theory is a very slight modification of the material in Luce and Raiffa [8].

We may sketch a simple proof of the theorem as follows: consider a utility-payoff set bounded by the straight line $U_1+U_2=k$. The symmetry assumption requires the outcome U_1^*, U_2^* to obey the condition $U_1^*=U_2^*=k/2$. Thus the point of agreement must be at the midpoint of the (straight) boundary line of the set. Through shifts in the units in which one or both players' utilities are measured (holding the origins constant), *any* straight boundary line in $U_1 \times U_2$ space can be given the form $U_1+U_2=k$. Moreover, the midpoints of straight lines always refer to the same actual outcomes under linear transformations. Thus the independence of linear

took the position that this function constitutes a positive theory in that it describes actual bargaining outcomes, but more commonly the analysis has been given a normative interpretation, and the Nash solution has been taken as a "desirable" outcome of the bargaining process. Finally, for the sake of future reference, it is useful to point out that in the case of a continuous utility-possibility set, for which the first-order derivatives on the northeast boundary of the set are always defined, the Nash point is the point for which $U_1/U_2 = -dU_1/dU_2$, where dU_1/dU_2 is the slope of the boundary.[4]

Since extended discussions of this model exist in both Luce and Raiffa [8] and Bishop [1], we will restrict ourselves to pointing out two important reasons for attempting to improve upon the Nash model as a general description of the bargaining process. First, it offers no analysis of the dynamic process of disagreement-concession-agreement that constitutes the very essence of the bargaining process. We are given only a solution criterion with no insight into its *raison d'être*. Thus it does not even partially answer the question that often is most interesting to us: under what conditions will the solution *deviate* from an idealized condition, and how will the variation take place? Second, acceptance of the descriptive interpretation of the Nash model would imply acceptance of the conclusion that all the information which is necessary for the analysis is contained in the set of possible utility-payoff combinations. All other variables (time, prices, relative intelligence of the bargainers, the arguments of the utility functions, etc.) must not enter in any essential way because their introduction would necessarily violate one or more of the Nash assumptions.[5]

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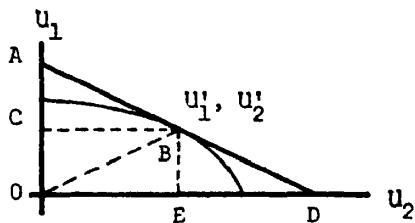
transformations of utility assumption implies that all utility sets bounded by straight lines have the midpoints of those lines as expected outcomes. Furthermore, the congruence of triangles ABC, BEO, and BDE in the diagram guarantees that such outcomes will always satisfy the condition $U_1/U_2 = -dU_1/dU_2$, where dU_1/dU_2 is the slope of the straight line, and it is easy to show that this condition in turn assures us that the product U_1U_2 has been maximized (see footnote 4). Now consider any convex outcome set bounded by $U_1 = f(U_2)$. In our continuous example, we can always find a straight line which is tangent to $U_1 = f(U_2)$ such that a point of tangency, U_1', U_2', includes the midpoint of the straight line (rotate the tangent line around the boundary—at some point our condition must be satisfied). Independence of irrelevant alternatives now ensures that U_1', U_2' is the outcome of the smaller game, and it is obvious that if this point maximizes the product U_1U_2 on the straight line, U_1', U_2' must also maximize the possible utility product of the game bounded by $U_1 = f(U_2)$.

[4] The boundary is defined as $U_1 = f(U_2)$; we want to maximize P where $P = U_1U_2$; $dP/dU_2 = U_1 + U_2(dU_1/dU_2)$; so for a maximum: $U_1/U_2 = -dU_1/dU_2$.

[5] For example, the easiest assumption to violate through the introduction of new variables is symmetry: if the solution depends upon *anything* besides utilities, then even in the case of a symmetric outcome set, we have no reason to expect $U_1 = U_2$ at the point of agreement.

II. *The Empirical Conclusions of Siegel and Fouraker*

Some excellent experimental work on bargaining has been conducted recently by S. Siegel and L. Fouraker [16]. They have performed a series of two-person bargaining experiments set in the context of bilateral monopoly. Besides studying the actual point of agreement, Siegel and Fouraker compared bargaining outcomes under various controlled conditions, studying the effects of complete vs. incomplete information,[6] and variations in the structure of the payoffs.

Below, we have simply listed those conclusions of the Siegel-Fouraker study which seem to be the most relevant to the discussions in this paper:

1. There is a tendency for bargainers to negotiate contracts that are Pareto optimal [16, p. 41].[7]
2. "Increasing the amount of information available to the bargainers tends to lead to a more equal division of the joint payoff" [16, p. 70].
3. Supplementing the higher payoffs to only one player so as to increase the utility to him of these outcomes tends to increase his payoff at agreement [16, pp. 62–70].
4. "Occasionally an opponent would offer an unexpectedly generous bid (this might be unintentional—the opponent would be maintaining a constant payoff plateau or concession rate, but moving toward the Paretian optima in his bids). The subject's usual reaction was to raise his own payoff request—make his next bid one which would yield a higher profit to him than would have been yielded by his own previous bid" [16, p. 81]. In other words, his usual reaction was to raise his expected payoff.
5. There is some evidence that increasing the information to one player alone tends to decrease his payoff at agreement [16, pp. 57–58].

III. *The Components of Bargaining Processes*

We may characterize the bargaining process in terms of two limiting cases: (1) the Pure Bluffing case—each participant plans to give in as the other does, expecting ultimately to achieve agreement at some intermediate point which may even go beyond the point to which the other

[6] Experimentally, this was done by permitting one player to see the other's profit table in cases in which he was to have complete information.

[7] Unfortunately, Siegel and Fouraker's use of a bilateral monopoly model leads to payoff tables in which all the Pareto optimal points also represent the joint maximum; so that from their experiments it is impossible to tell whether the tendencies are specifically toward the joint maximum, or just toward Pareto optima. Siegel and Fouraker tend to use these expressions interchangeably; we have substituted "Pareto optimal" simply because it seems more plausible to us that this is the correct interpretation. It may be, however, that conclusions (1) could be put much more strongly.

expects to concede;[8] (2) the Pure Intransigence or Asymmetrical case—each expects to obtain agreement at his initial demand, anticipating that the other player will make all of the concessions.

The case of pure bluffing alone would be a relatively uninteresting one in which no real disagreement existed at all. That such a case often does exist, however, is clear from certain labor-management disputes in which, just before strike-time, each party sends behind-the-scenes envoys to the other in hopes of discovering its real expectations and finding agreement staring them in the face.[9] It is the asymmetrical case (2) that fosters disagreement in the bargaining problem as we usually think of it. Most bargaining situations, of course, are best represented as a mixture of our two components—that is, the players demand more than they expect to obtain (bluffing), but the expectations themselves are not initially compatible (asymmetry). Because of the importance of the asymmetrical (intransigence) component and shortage of space, we must base our simple model upon this aspect alone, leaving an adequate discussion of bluffing to later studies.

IV. *Notation*

Formal analyses of the bargaining process often have been conducted in terms of utility units rather than physical "payoff" units. In general, however, people do not bargain over utilities—"you take two utiles, and I'll take three"—they bargain in terms of more objective quantities: states of their mutual environment or the division of goods. Depending upon the circumstances, we must measure the players' outcome demands by means of one of two alternative types of variable. In cases in which a fixed quantity of a homogeneous good i is being divided into smaller quantities, we can indicate player j's demand by means of a quantity variable q_{ij}. In this case, we shall have disagreement whenever

[8] The term "bluffing" is commonly used in several distinct ways. The meaning which we have in mind here refers to a player's deliberate misrepresentation of his outcome expectations in order to influence his opponent; that is, he demands x while he expects (or would otherwise expect) to receive y ultimately.

Bluffing can also refer to misrepresentation of other (we would say less basic) aspects of a bargaining situation, especially with respect to a player's own utility function ("We can't possibly offer a higher wage—we would go out of business"). This kind of bluffing does not alter the specific positions of the bargainers; it is simply an aspect of the verbal waffle which accompanies any bargaining process (although, of course, it may have some significant effects).

Finally, bluffing may be meant to refer to specific strategic opportunities which may come up in individual bargaining situations, such as the chance to conceal the existence of certain possible outcomes, misrepresent the actual consequences of disagreement, etc., but as these do not appear to be fundamental to the bargaining process in general, we shall not consider them.

[9] A case in which management clearly harbored such hopes appears in A. H. Raskin's description of the 1962–63 New York newspaper strike [13].

the sum of the players' demands exceeds the total amount available: i.e., whenever

$$\sum_j q_{ij} > M_i$$

where M_i is the total quantity of the ith good. If we are simply dividing a sum of money, this notation would be appropriate. In other cases in which the available amount of the good is variable, unknown, or perhaps not even defined, it is more appropriate to consider an ordered index P which ranges over the various outcome alternatives, and to indicate the outcome demand of player j by means of a variable P_j. Labor-management wage negotiations are the best examples of this case—here we would simply associate P with the wage rate. In a two-person negotiation of this type, we would have disagreement whenever $P_1 > P_2$ (if we associate P_1 with the union demand). The only formal difference between the two notations lies in a reversal of sign between the players in some of our expressions using the P-notation (player II prefers *higher* q_2, but *lower* P_2, as our examples were formulated). Simply to avoid possible sign confusion, therefore, we shall adhere to the quantity notation.

We shall consider the simplest case of a bargaining process—two persons bargaining over the division of a fixed quantity, M, of a single good which is continuously divisible. Following Nash, we may write utility functions for players I and II in terms of the utility increments that they may receive, $U_1 = f(q_1)$ and $U_2 = g(q_2)$, by adjusting the origins of our functions so that $f(0) = 0$, and $g(0) = 0$. So we have (assuming that the first two derivatives of the utility functions always exist):

$$(1) \qquad \begin{array}{lll} f(0) = 0 & f'(q_1) > 0 & f''(q_1) < 0 \\ g(0) = 0 & g'(q_2) > 0 & g''(q_2) < 0 \end{array}$$

V. *The Dynamic Nature of the Bargaining Process*

As any economist knows, time has a cost, both in money and in utility terms; it is our position that it is precisely this cost which motivates the bargaining process. If it did not matter when people agreed, it would not matter whether or not they agreed at all. The influence of time upon bargaining may assume three different forms. First, it appears naturally in a discounting function if the players discount future benefits (an agreement must offer a benefit, even if, as in the case of prestrike labor bargaining, the players are not at a point of physical disagreement; otherwise, there would be no bargaining). Second, the utility of agreement itself may change with the calendar date (the cake gets moldy before we agree on how to cut it, etc.). Finally, there is a fixed cost of

bargaining which recurs in each time period. This last cost may vary from the simple personal inconvenience of having to spend time in this rather than other occupations, to the immense cost in terms of loss of profit and fixed cost of a temporarily unproductive plant which is faced by a strike-bound firm. These costs are all unaffected by the point of agreement, and therefore they have not been included in our "utility" functions.

Since the present value of any anticipated outcome is affected by the time at which it would be reached, it is necessary for each player to estimate the time required for agreement. This time is obviously a function of a player's own demand. For example, player I estimates the amount of time necessary to obtain a payoff quantity q_1 by observing the current demand of player II, q_2, and considering the amount of time that it will take II to concede over the difference (q_1+q_2-M). Player I, however, has no specific knowledge of player II's utility function, and hence he can make no precise estimate of II's relative rates of concession over various payoff demands.[10] In such a case, player I is forced to make some general estimate of II's rate of concession, and in fact it appears to be reasonable for our purposes to assume that player I initially expects some positive concession rate. We shall call this rate r_2. By restricting our simple model to bargaining situations in which bluffing does not occur, we have already assumed that player I does not think of r_2 as a function of his own behavior. The expected time necessary to reach agreement such that player I receives a quantity q_1 is then given by the expression $(q_1+q_2-M)/r_2$, which we shall represent by w.

We shall introduce our time dependence by assuming that the utility functions express utilities at the time of agreement and then appropriately adjusting them back to the present. First, we shall assume the existence of an exponential discounting function[11] such that, with other factors neglected, the present value of a demand for a quantity q_1 is given by the expression $f(q_1)e^{-aw}$. Second, suppose that player I faces a fixed-cost rate C_1, where C_1 is expressed as a utility value per time period. The total expected cost of bargaining, Z, may be expressed in present-value terms as the sum of a stream of costs C_1 appropriately discounted; that is, $Z=C_1e^{-a}+C_1e^{-2a}+\cdots+C_1e^{-aw}$, or in continuous terms:[12]

[10] Player II may greatly prefer an outcome q' over q'' and be nearly indifferent between q'' and the inferior quantity q''', even where $q'-q''=q''-q'''$. In this case, if we knew his utility function, we would expect II to concede more slowly from q' to q'' than he does from q'' to q'''.

[11] A theorem of Robert H. Strotz [18], when applied to the following model, demonstrates rather forcefully that this is a desirable assumption.

[12] This may be thought of as an infinite stream of costs originating at the present time minus an infinite stream of costs originating at the time of agreement. In present-value terms, this is $C_1/a-(C_1/a)e^{-aw}$.

$$Z = C_1 \int_0^w e^{-ax} dx = \frac{C_1}{a}(1 - e^{-aw}).$$

For the purposes of this simple model, we shall postpone any dependence upon calendar date.

Now the total value to player I of insisting on a return q_1 is:[13]

$$U_1' = f(q_1)e^{-aw} + \frac{C_1}{a} e^{-aw} - \frac{C_1}{a}.$$

To decide what outcome to demand, player I chooses the quantity which satisfies his preferences—that is, the q_1 which maximizes the present value of his utility U_1', remembering that w, the time required to reach agreement, is a function of q_1. Differentiating U_1' with respect to q_1 and noting that $\partial w/\partial q_1 = 1/r_2$, we obtain the first- and second-order conditions:[14]

$$\left[f(q_1) + \frac{C_1}{a}\right]\frac{a}{r_2} = f'(q_1) \tag{2}$$

and $f'(q_1) - a/r_2 + f''(q_1) < 0$, or since $f'(q_1) > 0$, and $r_2 > 0$:

$$\frac{f''(q_1)}{f'(q_1)} r_2 - a < 0. \tag{3}$$

VI. *Learning*

Player I demands a payoff quantity, q_1, on the basis of an *expected* rate of concession from the other player, r_2; if player II in fact does not concede at that rate, then clearly I's expectations will alter—that is, r_2

[13] Note that an increase in costs, C, *increases* U_1; that is, increases the utility of agreement. One must bear in mind the fact that U_1' measures the *gain* in utility which player I obtains through agreement over permanent disagreement. This gain is certainly increased as the costs of disagreement are increased.

[14] We observed that any delay in the date of agreement will impose a cost on player I which is given by the discount rate, a. That is, he will lose a per cent of his total expected return for every time-period delay. Thus, since an increase in his demand, Δq_1, will put off agreement by $\Delta q_1/r_2$ units of time, such an increased demand will cost him $\Delta q_1 a/r_2$ per cent of his return or $\Delta q_1 a/r_2 U_1$ "utils," where U_1 is the undiscounted utility which player I expects to receive from his current demand. On the other hand, his return is increased by $\Delta U_1 = \Delta q_1 f'(q_1)$. Since $U_1 = f(q_1) + C_1/a$, it follows that so long as $f'(q_1) > a/r_2[f(q_1) + C_1/a]$; that is, so long as the marginal return from an increase in his demand is greater than its marginal cost, player I will be better off if he increases his demand. He will have maximized his return when equality holds. This is essentially the meaning of condition (2). Condition (3) simply requires that for condition (2) to represent a maximum, the expression $f'(q_1)$ must be decreasing relative to $a/r_2[f(q_1) + C_1/a]$ as q_1 is increased. Conditions (1) are obviously sufficient for this to be the case. We might add here that Siegel and Fouraker tended to favor a satisfying type of model of their bargainers' behavior. It is easy to demonstrate, however (see Section VI), that their results are perfectly compatible with utility-maximizing models.

will change, and, as a consequence, he will demand a different q_1. At this point we are naturally led to examine a process of *learning*. Most generally, a learning model is a time-dependent relation which must satisfy at least the following conditions:

$$\frac{dr_2}{dt} > 0, \quad \text{if} \quad -\frac{dq_2}{dt} > r_2;$$

$$(4) \qquad \frac{dr_2}{dt} = 0, \quad \text{if} \quad -\frac{dq_2}{dt} = r_2;$$

$$\frac{dr_2}{dt} < 0, \quad \text{if} \quad -\frac{dq_2}{dt} < r_2.$$

That is, if player II concedes faster than is expected, player I will increase his estimate of II's concession rate; if player II concedes just as rapidly as is expected, player I will retain his original estimate, r_2, and so on.

Furthermore, it is reasonable to assume that the magnitude of dr_2/dt varies positively with the discrepancy between $-dq_2/dt$ and the expected rate of concession; that is, the greater the error in player I's expectations, the faster his expectations will change. Thus we will assume:

$$(5) \qquad \frac{d\dot{r}_2}{d(-r_2 - \dot{q}_2)} > 0$$

where $\dot{r}_2$ is defined as dr_2/dt, and $\dot{q}_2$ as dq_2/dt.

Later, a more specific characterization of learning behavior will be used, but for the present, we shall be content with just the conditions (4) and (5).[15]

VII. *The Process of Concession*

We may observe from equation (2) that player I's outcome demand, q_1, is a function of time owing to its dependence upon r_2. We can find

[15] Expressions (4) and (5) do seem to correspond to a very general characterization of most existing learning theory. Although work attempting to define mathematically the time path of learning is decidedly in the minority in the otherwise extensive psychology literature on learning, some useful models do exist, notably those of Hull (see E. Hilgard [7, pp. 1–115), and K. Spence [17]), and Bush and Mosteller [3]. Also see A. T. Poffenberger, ed. [12]. It may be argued that even our generalized learning model should follow Bush and Mosteller in making use of statistical variations. We do have some doubts, however, as to the appropriateness of probabilistic elements in a behavioral theory.

his concession rate, $-\dot{q}_1$, simply by differentiating (2) with respect to t and solving for dq_1/dt, which yields:[16]

$$\dot{q}_1 = -\frac{1}{\frac{f''(q_1)}{f'(q_1)}r_2 - a}\,\frac{dr_2}{dt}. \tag{6}$$

In the light of (3), expression (6) requires that q_1 should vary in the same direction as r_2. In other words, if player I discovers that player II is yielding more rapidly than he expected, he will increase his demands. Conversely, he will reduce his demands if he discovers that player II is not giving in as fast as he formerly thought. Thus expressions (3) and (6), in conjunction with our learning theory, would already lead us to expect Siegel and Fouraker's conclusion #4 (see Section I) to the effect that sudden large concessions from one player tend to encourage increased demands on the part of the other. This conclusion also has the advantage of strongly supporting one's intuitive notions as to the consequence of unexpected concessions.

All the previous analysis may be applied to player II just as it was to player I, with similar results. In the case of player II, the expressions corresponding to (2)–(6) may be obtained simply by reversing the subscripts (and substituting b, player II's discount rate, for a). Notice that we have imposed no restriction on the possible signs of the $\dot{q}$'s. If $\dot{q}_1$ is positive, player I is increasing his demands. Often, retraction of an offer during negotiation is thought to be either unethical or at least undesirable because of the unfavorable utility shifts which such behavior may induce in the other player. In fact, Siegel and Fouraker, in their empirical study, deliberately formulated their rules so as to eliminate this possibility (they stipulated that any price-quantity bid, once made, was always good). Nevertheless, such retractions sometimes occur in practice. We will, therefore, retain the possibility of their occurrence, recognizing that forces probably exist which discourage increases in demands during the course of negotiations.

VIII. *Bargaining*

The essential nature of the bargaining process may now be described. We should start with r_1 and r_2 positive (if r_1, for example, were less than or equal to zero, that is, if player I were expected to hold out forever, player II would give in immediately). It is impossible that neither bar-

[16] That is, if r_2 changes by some amount Δr_2, the cost, $a/r_2[f'(q_1)+C_1/a]$, of a unit increase in player I's demand is changed, and he will adjust q_1 until the marginal return $f'(q_1)$ is equal to the new marginal cost $a/r_2+\Delta r_2[f'(q_1)+C_1/a]$. Expression (6) simply represents Δr_2 and the resulting change in q_1 in terms of time.

gainer will shift: if player I is not conceding, for example, player II will learn that his estimate of $-\dot{q}_1$ is too high, r_1 will fall, and player II's demand, q_2, will decrease along with it. If $-\dot{q}_2 = r_2$, then player II will make the entire concession himself. If player II gives in at a rate less than that which player I expects, then r_2 will fall as well, and player I will reduce his own demand, q_1. Thus we have the general case of a convergent bargaining process—whenever both actual concession rates are less than the expected rates.

We should also be aware at this point of a potential instability in this bargaining model: suppose that player II, for example, is a very sensitive learner, and he finds that player I is conceding at a rate significantly below his expectations. If he responds very strongly to this information, he may be found to be giving in at a rate greater than player I expects. Player I will naturally increase his demands in response, the extent of the increase depending upon expression (6) and his learning capacity. Now, if this increase in demands is sufficiently strong, r_1 will again fall strongly (depending upon II's capacity to learn), and II may give in even more rapidly.[17] This sequence may still converge so long as player II concedes faster than player I retracts his position, in which case, the conditions characterizing the point of agreement (to be formulated in a later section) are no different from those of the previous case. It may be, however, that either the sequence does not converge or that it does not converge soon enough, so that the point of "agreement" will simply be at or arbitrarily close to the best possible outcome for player I.

We can observe from expression (6) that the rate of concession varies inversely with the value of the expression $f''(q_1)/f'(q_1)r_2 - a$ or, in the special case of linear utility functions, with the value of the discount rate itself. Thus high learning rates and low discount rates will tend to induce instability in our model. The stringency of the resulting stability condition cannot be evaluated without some information as to the actual form of the learning functions in bargaining situations. One might infer from most of the recent widely publicized negotiations that learning rates are quite low. It is probably true also that the process of bluffing makes a considerable contribution toward stability through its impedance of the learning process.

IX. *The Convergence of Expectations*

Suppose we have two players who are relatively similar in that their abilities to learn and their discount rates are nearly equivalent. Further, suppose that r_2, player I's expectation of II's rate of concession, is

[17] It is not sufficient for instability to have $-\dot{q}_2 > r_2$; we must also have a strong response to this condition on the part of player I; otherwise, player II, not observing much of a change in $\dot{q}_1$, may fail to repeat the cycle, and we will have $-\dot{q}_2 < r_2$.

greater than r_1, and finally, consider the situation at the start of the bargaining process, when neither is giving in. According to our assumption that large errors in expectations bring about more rapid changes in expectations [see expression (5), Section VI], r_2 will fall faster than r_1. Thus we observe the two expected concession rates approaching one another.

This conclusion may be obtained more rigorously by means of a graphical analysis. Let us assume that the utility functions of the two players are linear, their discount rates are equal, and that their learning functions are identical. Suppose further that player II is conceding, but at a rate below that expected by player I. From our discussion in the last section, we concluded that player I will reduce his demand, q_1, at a rate which depends upon r_2 and upon the discrepancy between r_2 and player II's actual concession rate, $-\dot{q}_2$; the larger this discrepancy, the more q_1 is reduced. Thus the greater is the concession rate of player II, $-\dot{q}_2$, for example, the smaller will be the resulting concession rate of player I. When $-\dot{q}_2=r_2$, the expectations of player I are realized, and he retains his previous demand (i.e., $-\dot{q}_1=0$). This information appears on Figure 1. Placing $-\dot{q}_1$ and $-\dot{q}_2$ on the axes, we have drawn a line (F) with negative slope which intersects the $-\dot{q}_2$ axis at the point $-\dot{q}_2=r_2$. This line represents player I's reaction to any concession rate $-\dot{q}_2$, given his expectation, r_2, and may be put in the notational form $-\dot{q}_1=F(-\dot{q}_2, r_2)$. In exactly the same fashion, we construct the line $-\dot{q}_2=G(-\dot{q}_1, r_1)$ relating player II's rate of concession to his expecta-

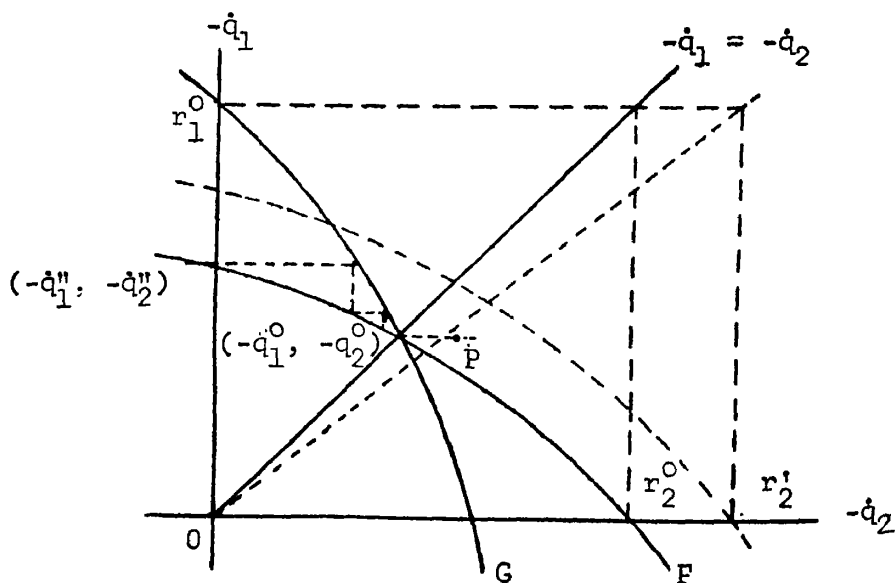



FIGURE 1

tion, r_1, and his opponent's concession rate $-\dot{q}_1$. As the graph is drawn, we have deliberately constructed the special case $r_1=r_2\,(r_1^o\neq r_2^o)$ in order to show the symmetrical (and, as it will turn out, the final-equilibrium) state.

It is evident that under these circumstances, the two instantaneous rates of concession will be given by the point $(-\dot{q}_1^o, -\dot{q}_2^o)$, the point of intersection of the functions F and G (by symmetry of the situation, $\dot{q}_1^o=\dot{q}_2^o$); that is, if we consider the situation from the point of view of a period analysis, we shall move from any point $(-\dot{q}_1'', -\dot{q}_2'')$ to the point of intersection, as in the case of the dotted example on the diagram. Now suppose r_2 is increased, *ceteris paribus*, to r_2'. This will *shift* the line $-\dot{q}_1=F(-\dot{q}_2, r_2)$ to the right to a position such as that shown on the dashed line on the graph. The effect of this shift is to increase $-\dot{q}_1$ immediately, and to move the point of intersection of F and G up to the left. We may now expect to obtain the relationship $-\dot{q}_1>-\dot{q}_2$, which, in our symmetrical example, implies the condition $-\dot{r}_2>-\dot{r}_1$; that is, r_2 is falling faster than r_1, tending to *shift* the function F down faster than the function G, restoring equality.[18]

Furthermore, it is important to show that the ratio r_1/r_2 will tend toward a value of unity.[19] Let us arbitrarily retain r_1 at a value of r_1^o and increase r_2 from r_2^o to r_2', and attempt to preserve the new value of the ratio $r_1/r_2(=r_1^o/r_2')$ over time. One way to accomplish this would be to retain $\dot{r}_1$ at its previous value and increase $\dot{r}_2$ in the same proportion as r_2 was increased in going from r_2^o to r_2'. This condition would be satisfied by some point P (see Figure 1) at which $-\dot{q}_1=-\dot{q}_1^o$ and $-\dot{q}_2>-\dot{q}_2^o$. However, no point above and to the left of P could possibly satisfy this condition since at any such point we would have $-\dot{q}_1>-\dot{q}_1^o$ and $-\dot{q}_2<-\dot{q}_2'$. Since the new intersection of our response functions F' and G is above and to the left of P, we must have r_2 falling too rapidly relative to r_1 to prevent the ratio r_1/r_2 from increasing. This process will continue until we reach a point at which $r_1/r_2=1$, and where, as a consequence, $\dot{r}_2=\dot{r}_1$.

It should be observed that we are assuming here that, as r_2 rises, the slope of the function F does not flatten so rapidly as to permit an intersection below and to the left of P. It is sufficient for this condition that every point of F moves upward and to the right as r_2 is increased; that is, that the new function F nowhere intersects the original function F

[18] This analysis oversimplifies the problem somewhat by neglecting the fact that the curves F and G are also shifting as the concession rates $-\dot{q}_1$ and $-\dot{q}_2$ approach their intersection. For the purposes of this paper, we shall generally assume that the convergence to point $(-\dot{q}_1^o, -\dot{q}_2^o)$ is instantaneous. A period analysis can be constructed, however, containing both phases of the problem, and such a model gives substantially the same results as the one given above.

[19] If r_1/r_2 does not converge toward unity, it is essentially trivial to point out that r_1 and r_2 approach one another as they both approach zero.

in the positive quadrant. We must require, therefore, that the response functions F and G always satisfy the conditions $\partial F/\partial r_2 \geq 0$; $\partial G/\partial r_1 \geq 0$ (in the positive quadrant); that is, for example, that a *ceteris paribus* increase in r_2 induces a higher concession rate from player I. It may be observed that our second learning condition (5), $d\dot{r}/d(-\dot{q}-r)>0$, virtually guarantees that this restriction will be satisfied so long as the second-order terms [e.g., $f''(q_1)/f'(q_1)$ in the denominator of (6)] do not increase rapidly as r_2 or r_1 are increased.[20] In our special case in which the utility functions are linear, of course, this condition must be satisfied.

Two other points should be noted here. First, our demonstration of the equilibrium behavior in our model does not depend on any linearity assumptions, although, of course, in a case in which the utility functions are not linear, we cannot expect the equilibrium point to be identical to that of a linear model. Second, it is not likely that our conditions $\partial F/\partial r_2 > 0$; $\partial G/\partial r_1 > 0$ are violated at *all* points on the functions F and G. There may be at worst a limited number of values of r_2, r_1 for which the ratio r_1/r_2 may be expected to display equilibrium properties [$r_1/r_2 = \text{const.}$]. Thus the conditions of the previous paragraph may be taken to be uniqueness conditions; if they are violated over only limited ranges, our equilibrium analysis is valid, and we are only prevented from pointing to one single expected outcome.

If it happens that the players are not identical (in learning abilities, discount rates, etc.), these differences are easily introduced into our model. For example, if player I is a more able learner than player II, he will concede more rapidly for every value of $-\dot{q}_2$ (if $-\dot{q}_2$ is less than r_2) than in the previous case. Thus the slope of F, $d\dot{q}_1/d\dot{q}_2$, will be greater than before, and our diagram now has the form (for $r_1 = r_2$) which is drawn in Figure 2. When $r_1 = r_2$, we expect to find player I conceding more rapidly than player II. It follows that r_2 will tend to fall faster than r_1, and thus the equilibrium state must have r_2 smaller than r_1. On the other hand, if player I has a higher discount rate than player II, the function $-\dot{q}_1 = F(-\dot{q}_2, r_2)$ has a form similar to the dotted example in Figure 2. Through reasoning similar to that used above, we may conclude that, in this case, r_2 will tend to be larger than r_1.

We have drawn the graph in such a manner that the response of each player to a discrepancy between his expectations and reality is not too great: this represents the case of a stable bargaining process. In fact, we can see from the graph that whenever the function G intersects the function F from lower right to above left, the negotiation will be stable. If we define the slope of the function F, $\partial F/\partial(-\dot{q}_1)$, as $-A$ and the

[20] That is, we are asking that a change in r_2 not be accompanied by a second-order shift in the denominator of (6) which is sufficient to nullify the effect of the change on $\dot{q}_1$.

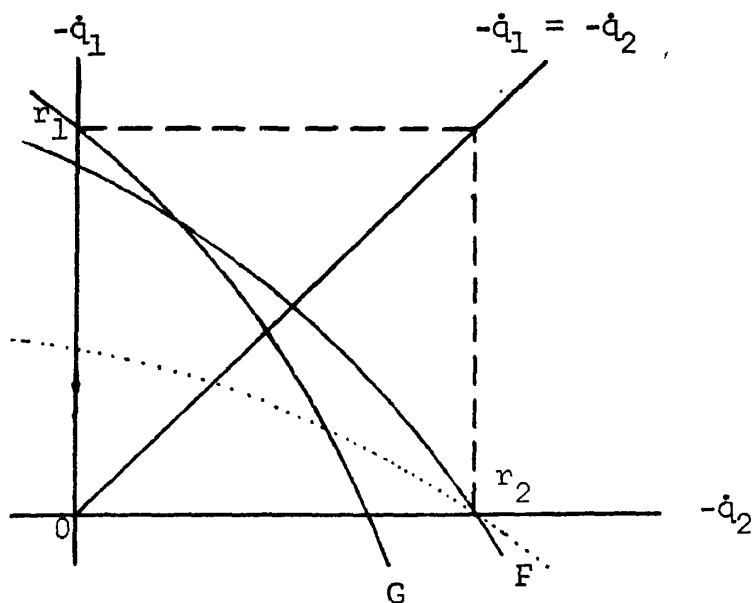



FIGURE 2

slope of the function G, $\partial G/\partial(-\dot{q}_1)$, as $-B$, then our stability condition is $1/B>A$, or $AB<1$.

If the two lines had intersected one another in the opposite fashion, we would have had instability on two counts: we would not expect to find the actual rates of concession at the point of intersection of F and G (a period analysis now takes us away from that point, as one can readily observe from the dotted time path in Figure 3), and the condition $r_2>r_1$ tends to make r_1 fall faster than r_2. In this case, we are unable to make any prediction concerning the outcome of the negotiation.

X. *The Point of Agreement and the Nash Solution*

It is now useful to characterize the point of agreement formally. Agreement is defined by the situation in which the sum of the players' demands is equal to the available supply, that is: $\bar{q}_1+\bar{q}_2=M$. Let us divide equation (2), the utility-maximization expression for player I, by the similar utility-maximization expression for player II as follows:

$$\frac{f'(\bar{q}_1)}{g'(M-\bar{q}_1)} = + \frac{f(\bar{q}_1)+\dfrac{C_1}{a}}{g(M-\bar{q}_1)+\dfrac{C_2}{a}}\,\frac{a}{b}\,\frac{r_1}{r_2}\,. \tag{7}$$

If we follow Nash's example and shift the origins of our utility func-

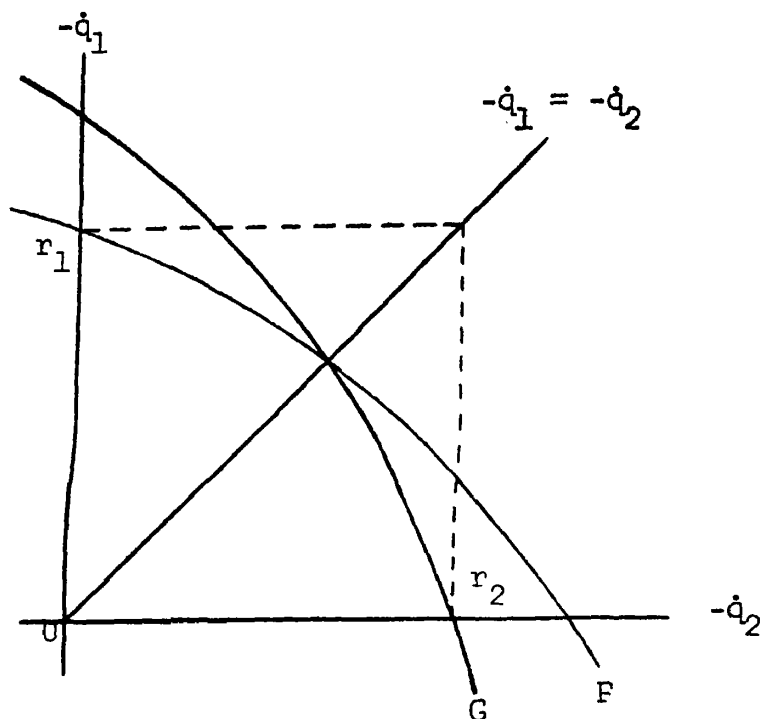



FIGURE 3

tions U_1' and U_2' so that the point of disagreement is at the origin,[21] and if we consider the utilities at the *time of agreement* (i.e., neglect the time-discounting factors), we obtain the following utility functions:

$$U_1 = f(\bar{q}_1) + \frac{C_1}{a}$$

(8)

$$U_2 = g(M - \bar{q}_1) + \frac{C_2}{a} \cdot$$

That is, the utility of agreement to each player is equal to the utility of his payoff [e.g., $f(q_1)$] *plus* the cost saving which results from the existence of a state of agreement rather than a state of disagreement (e.g., C_1/a). If we transform expression (7) into utility terms according to this definition of the utility functions, we obtain:

$$\frac{dU_1}{dU_2} = -\frac{U_1}{U_2}\frac{a}{b}\frac{r_1}{r_2} \qquad (9)$$

where dU_1/dU_2 is the slope of the Pareto optimum curve in $U_1 \times U_2$ space, at the point determined by $\bar{q}_1$.

[21] The functions U_1' and U_2' were formulated without taking into account the bargaining (disagreement) costs C_1/a and C_1/b.

Now we apply the conclusions of the previous sections to the special linear and symmetric situation. In the case of similar players (players with equal discount rates, so that $a=b$, and equivalent learning abilities), we concluded that we had an equilibrium relationship between expectations when $r_1=r_2$. In this case, expression (9) becomes simply

$$\frac{dU_1}{dU_2} = -\frac{U_1}{U_2},$$

the expression which characterizes the Nash solution to the bargaining problem (see Section I). Thus we obtain the rather satisfying result from our model that in the special case of identical players, the bargaining outcome can be expected to be the Nash solution.

We need not rely on linear utility functions for this conclusion. Suppose we impose the Nash condition of "symmetry" upon the bargaining situation in a slightly less general form. Assume that if $q_1=q_2$, then there exists some linear utility transformation such that:[22]

$$f(q_1) = g(q_2).$$

It follows by differentiation that for q_1 always to equal q_2:

$$f'(q_1) = g'(q_2),$$
$$f''(q_1) = g''(q_2).$$

We may conclude, therefore, that at the point where $q_1+q_2=M$ (and of course $q_1=q_2=M/2$):

$$\frac{f''(q_1)}{f'(q_1)} = \frac{g''(q_2)}{g'(q_2)}.$$

Making our assumption of "similarity" between the players ($a=b$, and the learning abilities are identical), then the preceding relations ensure that equation (6) becomes perfectly symmetrical for the two, and we clearly have an equilibrium state at $r_1=r_2$. That is, by equation (9), we have the Nash Point as the solution, and since $dU_1/dU_2=-1$ at q_1, $U_1=U_2$.[23]

However, even given our "similarity" assumption, the solution to our model as given by (10) is generally *not* the Nash solution. The dependence of our results on second-order derivatives as well as first is

[22] The Nash assumption is weaker than this because it only requires that for any q_1 there exists some q_2 (not necessarily equal to q_1), such that $f(q_1 = g(q_2)$ and $q_1+q_2 \leq M$.

[23] It must be remembered at this point that the condition $r_1=r_2$ is only achieved if the bargaining process takes enough time for the initial discrepancies to be eliminated. Otherwise, the solution which is derived from our model will differ from the actual results. In fact, as we shall show later, some error is almost always to be expected, although our solution will always serve as the focus toward which a stable bargaining process will tend.

sufficient to show that the two solutions will be different in all but fortuitous cases. This difference is due to the introduction of physical payoffs as important variables in the model.[24]

XI. *Further Conclusions on the Equilibrium Point*

Several other conclusions follow at once from the analysis. We concluded earlier that if player I is a more sensitive learner, the bargaining process will tend to yield the condition $r_2 < r_1$ (i.e., player II expects player I to concede more rapidly than player I expects player II to concede). Referring to equation (9), it is clear that a decrease in r_2 relative to r_1 at agreement results in a smaller value of U_1 relative to U_2 (and a larger dU_1/dU_2) than before. Hence the better a learner he is, the more the outcome will go against a player! It does not follow that such a player is made worse off by his superior ability—it may be that the saving in time which results from his more rapid concession outweighs the loss in undiscounted utility of the outcome. Naturally, the other player gains, and he probably gains more than the faster learner.[25] One cannot help noticing a striking resemblance between this conclusion and Siegel and Fouraker's observation (#5, Section II) to the effect that increasing the information available to a player tends to reduce his payoff. We may suggest that in their experiments, increasing one player's information concerning the other player's payoff utilities sharpened the first player's awareness of the other's rate of concession and simply increased his learning rate.[26]

We also concluded earlier that if player I used a higher discount rate (a) than player II, the bargaining process would tend to yield the condition $r_2 > r_1$. Referring to equation (9), we find this to have ambiguous consequences. Compared to the symmetrical case, as we have just

[24] That is, nonlinear utility functions are relevant in the bargaining process, and hence (referring to Section I) we do not satisfy either symmetry (in its general sense—our special case is not sufficient for the Nash theorem) or Independence of Irrelevant Alternatives even in the case of "similar" players. This model does satisfy Pareto optimality (in this simple model, there are no alternatives which are not optimal) and independence of linear transformations of the utility functions.

[25] The potential gain of a more rapid learner is only offered as a possibility—in general, he probably will not gain on balance. Note that this conclusion applies only to a player's learning *rate*. If one player has additional insight into the *nature* of the other player's learning process (e.g., its functional form or some relevant parameters), and this type of knowledge is not symmetrically held by the other, then the first player can almost certainly gain. The players' relative insights into one another's learning behavior is another important parameter which, unfortunately, has had to be neglected in our simple model.

[26] Perhaps it should be emphasized that the Siegel-Fouraker game was two-dimensional, and that in such a case, it is practically impossible for a player to tell whether an opponent's movement from a point (x, y) to a point (x', y'), where $x > x'$ and $y < y'$, constitutes a concession or not. Hence possession of information about the utilities associated with these points would greatly increase one's ability to estimate a rate of concession.

shown, a higher value of a tends to decrease the ratio r_1/r_2 by reducing player I's concession rate, and this alone would increase player I's outcome utility. However, a high discount rate also leads player I to sacrifice some payoff units at the start of the negotiation in order to hasten agreement [see equation (2)]. The net effect of these two forces is uncertain, however, and so we should have to use specific examples in order to obtain definite results.[27]

An inspection of equation (2) coupled with conditions (1) supports the intuitively reasonable hypothesis that high values of the bargaining costs, C_1 and C_2, will induce the players to make relatively lower payoff demands throughout the negotiation. This initial reduction in demands is certain to reduce the time required for the bargaining process, simply because agreement is reached with higher values of r_1 and r_2 than before. Intuitively, this conclusion is quite plausible; it may be taken, for example, as a major explanation for the vastly different amounts of time which are taken by different kinds of negotiations. A housewife normally spends only a few minutes bargaining over the price of a household item (on the rare occasions when she bargains over them at all) simply because the inconvenience which would accompany such a bargaining process is so large relative to the value of the good, while a labor dispute may last for months, and international negotiations in which the physical costs of bargaining are virtually nil (relative to the importance of the outcome) may last for years (e.g., test-ban talks, talks over Berlin, trade negotiations, etc.).[28] We can also observe that *rising* costs during negotiations will bring about additional concessions. For example, in prestrike labor-management negotiations, the costs are very low compared to costs during the strike. Thus so long as the expected date of agreement comes before the strike deadline date, a bargainer will make relatively large demands, but he will make significant concessions before he will permit the expected time of agreement to extend beyond the strike date. If the strike comes, nevertheless, his rate of concession will be reduced because he is no longer faced by a deadline beyond which bargaining costs are again made higher (unless there is a

[27] Bishop [1] has concluded that higher discount rates always tend to reduce a player's payoff, primarily because his model does not consider the dynamics of concession and hence misses part of the influence of the discount rate. Assuming a linear learning model [see equations (10), Sec. XII] and assuming *equal* learning rates, it is not hard to show that our model also yields this result. We might expect the opposite conclusion if the opponent's learning ability is relatively high.

[28] Of course, these second two examples depend upon negotiations between two groups rather than two individuals, and the reader may wish to make some qualifications here. The significance of a shift from individual to group negotiations is probably vastly overrated, however. It is perhaps conceivable that groups should have lower learning rates than individuals, but the most important characteristic of groups—the difficulty of formulating consistent preference functions—is irrelevant from the point of view of our model.

threat of government intervention or some similar event which imposes ıdditional utility costs on him). Thus we would expect labor negotiations to follow a pattern of relatively stable (and low) concession rates ıt first, rapid concession as the strike deadline approaches, and lower ;oncession rates afterward—a sequence which in fact is commonly observed.[29]

It should be observed that rising costs can bring about a semblance)f negotiation without any changes in expectations (i.e., without any ;quilibrating process) taking place.[30] Thus, for example, as public offi:ials bring increasing pressure upon striking unions and their employers, :oncessions—the symbols of "responsible collective bargaining"—come ıbout, even to the point of agreement. In the extreme case, the outcome lepends only upon the initial conditions and the relative pressures mposed on the two sides, and hence it is essentially arbitrary, having ıo relationship to the Nash Point or any other theoretical solution. A nore appropriate analysis would probably treat this case as a three-way)argaining process—a problem which is beyond the scope of this paper.

We must also point out one rather awkward point in our theory—hat is, that our "Nash Point" before a strike is by no means equivalent o the "Nash Point" after a strike. Only if the discounted stream of bar;aining costs which appear after the strike are the same for both parties i.e., $C_1/a=C_2/b$ so that in equation (8), U_1 will equal U_2 whenever $(\bar{q}_1)=g(M-\bar{q}_2)$] will the Nash solution be the same in both cases. This ; really a consequence of our procedure which classifies any state in hich agreement does not prevail as a disagreement point. Nash cerainly meant his solution to be constant over time, and for this reason, : might be desirable to add this further condition (that the discounted ost streams must be equal) to our earlier set of conditions under which ur model yields the Nash Point as its solution. On the other hand, such procedure would obscure the fact that the bargaining process is heavily ıfluenced by current conditions, and that the costs of such possibilities ; strikes or lockouts may never even be relevant in arriving at agreeıent; to evaluate the agreement in terms of the worst possible form of onflict would not seem to be realistic.

At this point, we may indicate how our model may help us to under:and two more of Siegel and Fouraker's observations. Their conclusion 2, Section II) that increasing (utility) information to both players ;nds to increase the equality of the payoffs may be taken as an indicaon that such information (again simply that of having a table of the

[29] For example, see Reynolds [14, p. 182]. We may infer from Raskin's description of the cent New York newspaper strike [13] that this phenomenon appeared in that case as well.

[30] Thus increasing costs (or decreasing value of the payoff over time) are used by both Bishop ıd Foldes to obtain determinate bargaining models.

opponent's monetary returns as well as one's own) tended to make expectations more realistic as well as more symmetric (when presented with a relatively symmetric bargaining situation, one would tend to expect a symmetric outcome). Furthermore, both players were now in possession of an important common source of information—we would expect this to make their learning abilities relatively more equal. Since we have no reason for believing the players' discount rates to be very different, and since the monetary payoff functions were fairly symmetric, this would all tend to favor the Nash Point where $U_1 = U_2$.

Siegel and Fouraker's conclusion (#3, Section II) that adding to the utility of higher payoffs for one player tended to increase his physical payoff at agreement is easy to treat. Their method was to supplement the return to one player of any payoff above some q' by a fixed increment. Thus the utility function for player I (if he is the favored player) could be written:

$$U_1 = f(q_1 + \xi)e^{-aw}$$

where

$$\xi(q_1, q') \text{ is defined as } \begin{Bmatrix} k & \text{if} & q_1 > q' \\ 0 & \text{if} & q_1 < q' \end{Bmatrix}.$$

We may expect a player in these circumstances to behave just as we described in our previous model, except when his payoff approaches q', at which point he will refuse to concede for a considerable time. A concession from q' to $q'-1$ would involve the utility loss associated with $k+1$ payoff units, while saving only as much time as is associated with one payoff unit. This reluctance can be expected to lower his opponent's estimate of his concession rate and cause player II to concede instead (which in turn reduce the usual decline in r_2). Thus even if player I does eventually go below q', the outcome may be expected to favor him. The fact that in several of Siegel and Fouraker's cases, the payoffs to the favored players were significantly above q' would indicate that there was some bluffing taking place as well, causing a player to balk at an inflated demand and forcing the other player to concede much more sharply than before.

We do not mean to imply that we have explained Siegel and Fouraker's results—no empirically untested model could do so—but it is significant that results so similar to theirs can be obtained from our model.

XII. *A Linear Model*

The conclusions of the foregoing model were very qualitative in nature; in fact, we were able to describe a definite solution (the Nash

solution) only in a very special case in which the players, for all intents and purposes, were identical. In this section we will make use of a simple linear example of our general model with the principal intent of obtaining a more detailed analysis than we have been able to make heretofore. The results which we derive here, of course, will not be those of a general model, but they should serve nevertheless as useful aids in the estimation of magnitudes which are involved in less restricted bargaining processes.

We shall simplify our learning model by assuming that the players modify their expectations according to a constant proportion of the *error* in their expectations:

$$\dot{r}_2 = \alpha[-\dot{q}_2 - r_2]$$
$$\dot{r}_1 = \beta[-\dot{q}_1 - r_1] \tag{10}$$

where α and β are characteristics related to the rates at which players I and II are able to learn.[31] Expressions (10) do satisfy both of the conditions for learning which are set out in expressions (4) and (5) in Section VI.[32]

We have already made limited use of a linearity assumption on the players' utility functions as one means of deriving the Nash solution to the bargaining process (in the case of similar players). For the purposes of this model, we assume that player I has the utility function $U_1=\xi_1 q_1$ where q_1 is his share of the benefits of agreement (this function is deliberately chosen so that $U_1=0$ when $q_1=0$). Substituting this function into equation (2), we may solve for player I's demand:

[31] These really represent an exponential theory of learning: if $\dot{q}_2$ were constant over time, equations (10) would imply:

$$r_2(t) = [-q_2 + (r_2)_{t=0}]e^{-\alpha t}.$$

Exponential forms do appear in the psychology literature: see for example, Poffenberger [12, pp. 296–300].

[32] This assumption also permits us to solve for the slopes of the response functions F and G which we defined as $-A$ and $-B$ respectively (see Section IX). Thus $-A$ may now be written

$$-A = \frac{\partial \dot{q}_1}{\partial \dot{r}_2}\cdot\frac{\partial \dot{r}_2}{\partial \dot{q}_2} = \frac{\alpha}{\dfrac{f''(q_1)}{f'(q_1)}r_2 - a}$$

with a similar expression for $-B$. We noted (in Section IX) that when the condition $AB>1$ holds, our model of the bargaining process is unstable. Thus it is sufficient for stability to have $A<1$, $B<1$, for which, in turn, sufficient conditions are $\alpha<a$, $\beta<b$. In other words, we are assured of stability if each individual's rate of learning is less than his rate of discounting. This conclusion corresponds to our intuitive discussion of instability in Section VIII. If the players' learning rates are high (α, β large) and if they tend to respond markedly to changes in their expectations (a and b small), then an increase in one player's demand (or a reluctance to concede) will give rise to a relatively large concession from the other which, in turn, will reinforce the first player's demands for a larger share of the outcome.

$$q_1 = \frac{r_2}{a} - \frac{C_1}{\xi_1}\frac{1}{a}. \tag{11}$$

Putting this and our linear learning model into our general model, we may solve for the players' outcome demands as functions of time:

$$\begin{aligned} q_1(t) &= \frac{1}{a}\left[k_1 d^{x_1 t} + k_2 e^{x_2 t}\right] - \frac{1}{a}\frac{C_1}{\xi_1} \\ q_2(t) &= \frac{1}{b}\left[m_1 e^{x_1 t} + m_2 e^{x_2 t}\right] - \frac{1}{b}\frac{C_2}{\xi_2} \end{aligned} \tag{12}$$

where $k_1 > 0$, $k_2 \gtreqless 0$, $m_1 > 0$, $m_2 \gtreqless 0$, $x_1 < 0$, and $x_2 < 0$ are various dynamic parameters.[33]

By making use of this model, we may, of course, derive all of the general conclusions which we obtained earlier. It should be emphasized here that although r_1 and r_2 approach their equilibrium relationship only asymptotically, equations (12) indicate that a state of agreement is approached more directly. This fact may be more clear from the example of equations (12) which is plotted on the graph in Figure 4. Here we have graphed player I's outcome demand (q_1) and the residual which player II's demand implicitly leaves to player I, $(M - q_2)$, against time. Agreement occurs when $q_1 = M - q_2$.

For certain values of t, equations (12) may yield values for q_1 and q_2 which are beyond the resources of the bargaining situation (the dashed lines in Figure 4). In these cases, a player simply demands the maximum payoff which is possible. Notice, though, that the mechanism of our model does not operate under such circumstances, since the rate of change of such a player's demand is zero even when his expectations are changing. It is not until both players are within the boundaries of the bargaining situation that our analysis will hold.

[33]
$$\begin{aligned} x_1 &= \frac{-\alpha - \beta + \sigma}{2(1 - AB)} \\ x_2 &= \frac{-\alpha - \beta - \sigma}{2(1 - AB)} \\ k_1 &= \frac{1}{\sigma}\left[\beta A r_2' + \tfrac{1}{2}(\alpha - \beta + \sigma) r_1'\right] \\ m_1 &= \frac{1}{\sigma}\left[\alpha B r_1' + \tfrac{1}{2}(-\alpha + \beta - \sigma) r_2'\right] \\ k_1 + k_2 &= r_1' \\ m_1 + m_2 &= r_2' \\ \sigma &= \left|\left[(\alpha - \beta)^2 + 4\alpha\beta AB\right]^{1/2}\right| \end{aligned}$$

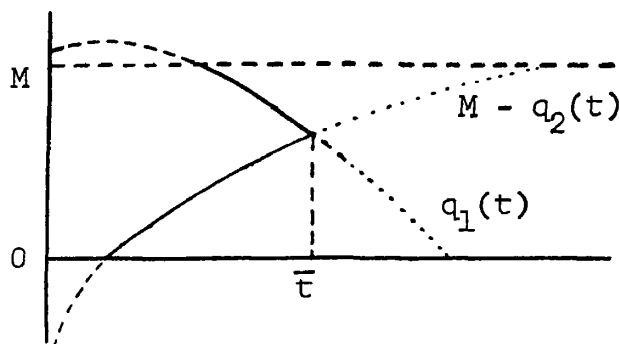



FIGURE 4

We can determine the amount of time necessary for agreement, $\bar{t}$, by solving the agreement condition:

$$q_1(\bar{t}) + q_2(\bar{t}) = M$$

for $\bar{t}$. If we substitute equations (12) into this expression, we could, in principle, obtain $\bar{t}$. The resulting expression is too complicated for direct solution, however, and so we will simplify our example further by applying the "similarity" conditions ($a=b$, $\alpha=\beta$) to the model. We may then solve the above equation for $\bar{t}$:

$$\bar{t} = -\left[\frac{1}{\alpha} + \frac{1}{a}\right] \ln \left[\frac{\dfrac{C_1}{a\xi_1} + \dfrac{C_2}{a\xi_2} + M}{\dfrac{r_1'}{a} + \dfrac{r_2'}{a}}\right]. \tag{13}$$

From equation (11), and the essential bargaining condition that $q_1+q_2>M$ at time $t=0$, we may conclude that the expression in the brackets of equation (13) must be positive but less than 1, and therefore that the logarithm of that quantity must be negative.

From expression (13), we may again deduce the conclusion that higher values of C (higher bargaining costs) decrease the necessary amount of bargaining time. It should be remembered that higher bargaining costs hasten agreement only by reducing the player's demands at any given level of expectations. Bargaining costs in this model have no influence over the players' expectations (of one another's concession rates) or their rates of change. Thus the level of costs (if it remains fixed) does not affect the rate at which concession takes place.

Expression (13) also indicates that higher rates of learning (for both players) reduce the time required by the bargaining process, as will higher rates of discounting. We note, however, that although an increase in player I's discount rate, a, reduces $\bar{t}$, it need not reduce the product

$a\bar{t}$; if it does reduce this expression, too, then the higher discount rate is advantageous to the two players because it increases the expected value to each of them of the whole bargaining process—that is, the discount factor $e^{-a\bar{t}}$ is nearer unity.

Knowing the total bargaining time, $\bar{t}$, it is possible to determine the actual point of agreement by substituting the value for $\bar{t}$ [equation (13)] into equation (12) in order to obtain $q_1(\bar{t})$ and/or $q_2(\bar{t})$:

$$(14)\quad q_1(\bar{t}) = \frac{M}{2} - \frac{1}{2}\,\frac{C_1}{a\xi_1} + \frac{1}{2}\,\frac{C_2}{a\xi_2} + \frac{r_1' - r_2'}{2a}\left[\frac{\dfrac{C_1}{a\xi_1} + \dfrac{C_2}{a\xi_2} + M}{\dfrac{r_1'}{a} + \dfrac{r_2'}{a}}\right]^{a+\alpha/a-\alpha}$$

where r_1' and r_2' are the expected concession rates at the start of the bargaining process. We observe that the first three terms on the right of equation (14) correspond to the solution for the symmetric case which we derived in Section X.[34] Thus, equation (14) is significant in that the far right-hand term gives us the *deviation* of the actual bargaining outcome from the "ideal" solution which would appear if bargaining took an infinite amount of time. The fact that the condition $r_1/r_2 = 1$ is only approached asymptotically implies that since agreement in this model takes a finite time, the outcome will not correspond to the "predicted" solution if the initial expected concession rates (r_1' and r_2') are not equal. Besides the obvious conclusion that greater discrepancies between the two initially expected concession rates result in a greater error in our model in predicting the actual outcome, we observe from equation (14) that small values of C_1 and C_2 (the bargaining costs) will tend to decrease the deviation as well. This last conclusion is simply a consequence of the fact that high costs tend to reduce the time taken by the bargaining process and thus give the ratio r_1/r_2 less time to adjust to its stable value. Higher rates of learning will decrease the error in spite of their tendency to accelerate bargaining, because they bring about more

[34] That is, equation (7) becomes

$$\frac{\xi_1}{\xi_2} = +\frac{\xi_1 q_1 + \dfrac{C_1}{a}}{\xi_2(M - q_1) + \dfrac{C_2}{a}}$$

which reduces to

$$q_1 = \tfrac{1}{2}\left[M - \frac{C_1}{a\xi_1} + \frac{C_2}{a\xi_2}\right].$$

This is simply the symmetric Nash outcome which we mention in footnote 3.

rapid adjustments of r_1 and r_2 as well. The impact of changes in the discount rate, a, is ambiguous, although in most reasonable cases increases in the discount rate can be shown to increase the error.[35] This is because increases in the discount rate tend to decrease bargaining time by reducing initial demands [see equation (3)] as well as by increasing the adjustment rates of r_1 and r_2 (the first of these tends to increase the error, the second to decrease it—hence the ambiguity).

Finally, it must be remembered that the error term here is really only approximate. Even if the players' learning behavior and the utility functions actually are linear as we have assumed, our results depend upon the instantaneous learning model which was chosen for our example. We are actually studying only the behavior of the intersection of the functions F and G (see Section IX) without regard to any time lags which may appear in arriving at that intersection. Lagged models would naturally alter our error term somewhat.

XIII. *A Numerical Example*

In conclusion, it may prove to be interesting to put some numbers which correspond roughly to our own experience into this model in order to get some indications of the magnitudes of the quantities which are involved. For the sake of variety, we will assume that the total amount of the payoff commodity which is to be divided is variable or unknown, in which case the P-index measure of the players' outcome demands is more appropriate than our quantity notation (see Section IV). Such a model is obtained simply by substituting P_1 for q_1, and $M-P_2$ for q_2 in all our equations, where M is now to be interpreted as the maximum value to which the P-index can attain (M is constant in this interpretation, even if the total quantity whose division is indicated by P is not). As we have already pointed out, except for a few changes in sign (which are necessary to preserve the meaning of our expressions), the model is unchanged, and as it happens, equations (13) and (14) retain their forms completely (with P_1 substituted for q_1).

Imagine a labor-management negotiation in which the wage may range between 0 and 100 (at 100, the firm goes out of business—the total surplus which is being divided implicitly by the wage becomes zero).[36] We have a complete solution only for the symmetrical case; so we must assume the same discount and learning rates for both parties. Suppose the discount rate (a) is .25, and the learning rate (α) is .20,

[35] Take logs and compare the expressions d/da ($\ln a$) with d/da $[(a-\alpha)/(a+\alpha)]$.

[36] This, of course, makes linear utility functions impossible over the whole range of P—we shall only assume that the functions are linear in the relatively small range over which the bargaining takes place.

both on a yearly basis. Neither bargainer in a union-management dispute has any illusions about being able to obtain either the entire surplus or a substantially larger portion of it for himself, nor does either expect to be able to obtain large rapid concessions from the other—experience has shown that this is unlikely (and analysis of a model in which the players expect rapid concessions would suffice to show how such experience may come about). Thus let us assume initial expected concession rates of $r_1'=12.5$ and $r_2'=17.5$ where these are also on a yearly basis.

Consider first a case with no bargaining costs. In this case, expression (13) gives us a $\bar{t}=1.67$; that is, it takes over a year and a half to reach agreement. On the other hand, the deviation from the "predicted" solution is only -1.87; that is, the wage turns out to be 48.13 instead of 50 as is predicted by the simple model. This is not a large deviation considering the large discrepancy between r_1' and r_2' (i.e., the initial demands—given by equation (11)—are given by a union demand of 50, and a management offer of only 30).

Now let us add bargaining costs to the situation. Suppose the costs to the union are fairly high, and $C_1/\xi_1=3$, while the costs to the management are lower: $C_1/\xi_1=1$. These are sufficient to shorten the bargaining time to about .3 years, or four months (bargaining costs which summed to five or more would bring about immediate agreement[37]). In this case, however, the deviation from the predicted outcome is -7.33; that is, the union gets a wage 7.33 less than would be predicted. It should be noticed also that the disproportionate costs have shifted the expected (Nash) solution. The predicted solution is now 46, having moved in management's favor because of the higher bargaining costs to the union. In view of these results, it is not hard to surmise why international negotiations, where learning and discount rates are probably lower than in the above example, expectations may be more optimistic, and bargaining costs are practically zero, can last for years, although for that same reason we would expect that the outcomes will be fairly near to the Nash solution.

The above results are fairly sensitive to changes in the data. For example, a decrease in the learning rate from .20 to .15 in the case of zero bargaining costs has the consequence of increasing the length of the bargaining time to 1.95 and of increasing the error in the general prediction to -4.83. On the other hand, more similar values r_1' and r_2' would bring about a considerable reduction in the deviation (i.e., if

[37] We have not put down the costs C_1 or C_2 explicitly; we are simply assuming the costs to be such that when they are appropriately adjusted by the utility factors ξ_1 and ξ_2, they may be compared to a change in 3 and 1 units of P (in one year) respectively.

the difference $r_i' - r_i'$ were reduced to 2.5, the deviation would be halved).

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COMMUNICATIONS

The Overvaluation of the Dollar: A Note on the International Price Mechanism

During the past few years it has become clear that the U.S. dollar is over-lued. The extent of the resulting deficit is indicated by average annual :reases in foreign holdings of liquid dollar assets and sales of monetary ld of between $3 and $3.5 billion over the period 1958–62.[1] An estimate the degree of overvaluation using the purchasing-power-parity approach s been constructed by H. S. Houthakker [5], who concludes that the dol- · is overvalued with respect to the major European currencies by 20 per nt. This paper presents an alternative upper-limit estimate of the magni-de of the existing exchange-rate disequilibrium and concludes that an ervaluation of more than 10 per cent is highly implausible. It also pro-les evidence that, aside from speculative forces, the foreign exchange ırket will be highly stable.

Our procedure will be to construct a model of the effects of a devaluation the balance of payments and to develop estimates of the parameters in is model. An estimate of the overvaluation, or the magnitude of devalua-ın which would be required to close the apparent deficit, can then be calcu-:ed. The major problem is that of obtaining estimates of the demand and pply elasticities of exports and imports. Most of the existing statistical timates of the demand elasticities for imports and exports are subject to vere downward bias, and estimates of the elasticities of supply of exports d imports are, as far as I know, nonexistent.[2] There are, however, more liable estimates of the domestic supply and demand elasticities of particu-r goods. This paper will make use of the fact that a country's elasticities demand for imports and supply of exports depend upon the domestic sup-y and demand elasticities for the goods in question and upon the ratios of

[1] Kenen [6] presents an excellent discussion of the problems involved in estimating the de-:e of disequilibrium.

[2] The downward bias in the demand elasticities stems from two main sources. First, since : demand for imports is the excess of the domestic demand over the domestic supply of im-rt goods, the quantity demanded will be a function of the domestic supply conditions as ll as relative prices and incomes. Failure to take shifts of the supply function into considera-n results in statistical estimates of the price elasticity of demand for imports which are too r. Second, in the great majority of the estimates, no attempts were made to incorporate long-ı adjustment into the statistical models. Since domestic supply of import goods is likely to pond fully to price changes only after a considerable lag, the omission of long-run adjust-nt is likely to result in a substantial downward bias of the price elasticity of demand for im-:ts. Estimates which used data for the interwar period were further hampered by lack of :iability of prices relative to incomes. More detailed discussion of these issues is given by cutt [8] and Harberger [2] [3]. More recently a study by Ball and Mavwah [1] of the S. demand for imports has incorporated long-run adjustment and utilized data for the ıtwar period.

domestic production and consumption to exports and imports. Existing statistical studies will be drawn upon in estimating the demand and supply elasticities for export and import goods. The ratios of domestic and foreign consumption and production to exports and imports will be calculated from data provided by the World Trade Information Service. At every juncture an attempt will be made to ensure that the errors are in the direction of overstating the degree of overvaluation.

I. *Analytical Framework*

The effects of a devaluation on the balance of payments can be divided into two parts: its effects on capital flows and on the balance of trade. The former will arise from changes in expectations and in domestic interest rates relative to foreign interest rates. A devaluation may affect the values of exports and imports in three ways: by raising the prices of foreign goods relative to domestic goods, by changing domestic and foreign real incomes, and by stimulating capital flows. Real incomes will be altered by movements in the terms of trade and changes in the level of output and employment. Relative prices will be changed by variation of the exchange rate and movements in domestic and foreign price levels. The trade balance will be affected by a capital flow to the extent that the transfer is financed partly out of imports in the country losing capital and spent partly on imports in the recipient country.

At constant aggregate demand, the demand function for exports and supply function for imports in domestic currency will shift upward by the amount of the devaluation. The higher the elasticities of demand for exports and imports, the more favorable will be the effect of the devaluation on the trade balance.[3] Lower supply elasticities imply a less favorable effect on the trade balance when the elasticities of demand exceed unity.

Aggregate demand may be affected by a devaluation in three ways. First, the rise in the domestic prices of import and export goods will reduce the real money balances of the public. Assuming that the public's desired level of real cash balances has not changed, this will lead to an attempted conversion of real and financial assets into money. Interest rates will rise and aggregate demand will fall. Similarly, the fall in the prices of internationally traded goods abroad will result in an excess of actual over desired real money holdings, reducing interest rates and increasing aggregate demand. A rise (or fall) in the prices of internationally traded goods may also have a wealth effect on the demand for money in that the real value of the communities' holdings of government bonds and note issue has fallen (or risen). Second, there will be an increase in domestic aggregate demand and a fall in foreign aggregate demand as a direct consequence of an improvement in the trade balance. Later empirical evidence will justify an assumption at this point that a devaluation of the dollar will improve the trade balance. Finally, a deterioration of the terms of trade of the United States and the

[3] The demand elasticities include the real income effects of changes in the terms of trade at constant output and employment.

associated decline in real income may decrease the fraction of money income saved, thereby raising the level of aggregate demand. Similarly, an improvement in the terms of trade abroad may increase the fraction of money income saved and reduce aggregate demand.[4] Of the three influences outlined above, the trade-balance effect is likely to dominate in the present problem. Because of the unimportance of the international sectors in the United States and the rest of the world as a whole, the effect of a devaluation on real money balances would be small. Since imports are a small fraction of domestic income and exports a small fraction of foreign income, the effects of changes in the terms of trade are also likely to be small. On the other hand, as this paper will show, the effects of a devaluation on the trade balance will be substantial.

The ultimate change in aggregate demand and its effect on the balance of trade will depend on two circumstances: the presence or absence of full employment and price flexibility, and the monetary and fiscal policies of the governments involved. Assuming less than full employment and no offsetting government policy in the United States, the rise in aggregate demand will increase both real output and interest rates.[5] In the absence of price flexibility and offsetting monetary and fiscal policy abroad, interest rates and real output will decline. The demand for U.S. imports will increase and the demand for exports will decline, moderating the effect of the devaluation. Full employment can be established at home and maintained abroad by alternative combinations of monetary and fiscal policy. The greater the U.S. government's reliance on fiscal policy and the foreign reliance on monetary policy, the greater will be the rise in domestic relative to foreign interest rates. A rise in interest rates at home relative to abroad would bring about a capital inflow into the United States.

For purposes of subsequent analysis two working assumptions are adopted: (1) the U.S. government applies a combination of monetary and fiscal policy which will bring about full employment, maintain interest rates at their initial level, and finance any change in demand for nominal money balances resulting from the rise in the prices of import and export goods; and (2) foreign governments offset the balance-of-trade and terms-of-trade effects on aggregate demand by fiscal policy, reduce the money supply sufficiently to eliminate the excess demand or supply of real money balances resulting from the declines in export and import goods prices, and thereby maintain the initial level of interest rates. Foreign incomes and interest rates are thus unaffected by the supposed devaluation. In the United States, interest rates remain unchanged and output increases until all resources are fully employed. Since under these assumptions capital flows

[4] Much will depend upon whether the community views the real income effect of changes in the terms of trade as permanent or temporary. If the former is true, saving is likely to change in the same percentage as real income so that the fraction of income saved will remain constant.

[5] The rise in interest rates is ensured by the tightness in the money markets and the improvement in the trade balance.

will be unaffected by the devaluation, the change in the balance of payments will be identical with the change in the trade balance. Toward the conclusion of this paper the assumption of constant interest rates will be qualified.

The following mathematical model can now be applied. Let

$$X = f_x\left(\frac{P_x}{r}\right) \tag{1}$$

$$M = f_m(rP_m, y) \tag{2}$$

$$B = XP_x - rMP_m \tag{3}$$

where P_x and P_m are export and import prices in the currency of the producing country, y is real output per capita in the United States, X and M are the quantities of exports and imports, r is the dollar price of foreign currency, and B is the trade balance in domestic currency.

Differentiating the above equations with respect to r, translating the results into elasticity form, and solving for dB/dr we get

$$\frac{dB}{dr} = MP_m\left[S\,\frac{dP_x}{dr}\cdot\frac{r}{P_x} - S\eta_x\left(\frac{dP_x}{dr}\cdot\frac{r}{P_x} - 1\right) - \frac{dP_m}{dr}\cdot\frac{r}{P_m} + \eta_m\left(\frac{dP_m}{dr}\cdot\frac{r}{P_m} + 1\right) - \epsilon_m\,\frac{dy}{dr}\cdot\frac{r}{y} - 1\right] \tag{4}$$

where η_x and η_m are the demand elasticities of exports and imports in absolute values, ϵ_m is the income elasticity of demand for imports, and S is the ratio of the initial dollar value of exports to the initial dollar value of imports. The elasticities of export and import prices with respect to the exchange rate equal

$$\frac{dP_x}{dr}\cdot\frac{r}{P_x} = \frac{\eta_x}{\eta_x + \delta_x} \tag{5}$$

$$\frac{dP_m}{dr}\cdot\frac{r}{P_m} = \frac{\eta_m}{\eta_m + \delta_m} + \frac{\epsilon_m}{\eta_m + \delta_m}\,\frac{dy}{dr}\cdot\frac{r}{y} \tag{6}$$

where δ_x and δ_m are the supply elasticities of exports and imports.[6]

Upon making the appropriate substitutions we can express the percentage devaluation required to close a given deficit as

[6] These relationships can be derived from the following supply and demand equations for exports and imports:

(5)′ $\log X_D = -\eta_x (\log P_x - \log r)$ (5)[1]

(5)″ $\log X_S = \delta_x (\log P_x)$ (5)[11]

(6)′ $\log M_D = -\eta_m (\log P_m + \log r)$ (6)[1]

(6)″ $\log M_S = \delta_m (\log P_m)$ (6)[11]

$$\frac{dr}{r} = \frac{\dfrac{dB}{rMP_m} + \dfrac{1+\delta_m}{\eta_m+\delta_m}\epsilon_m R}{S\eta_x\left(\dfrac{1+\delta_x}{\eta_x+\delta_x}\right) + \eta_m\left(\dfrac{1+\delta_m}{\eta_m+\delta_m}\right) - 1} \tag{7}$$

where dB/rMP_m is the needed change in the trade balance relative to the initial value of imports, and R is the percentage by which domestic output would have to rise for full employment to be achieved.

II. *Quantitative Estimates*

Exports and imports are defined to exclude those items which are unlikely to be sensitive to changes in the exchange rate. These are military transactions, income of investment, and miscellaneous government services. Average imports and exports for 1958–62 as defined above were $18,754 million and $22,340 million respectively. Merchandise made up 83 per cent of exports and 79 per cent of imports. The devaluation must increase the excess of exports over imports by about $3.5 billion. The deficit as a fraction of the dollar value of imports equals about .2, and S is in the neighborhood of 1.2. Since the U.S. economy has at present no more than 8 per cent unemployment, the maximum plausible improvement in real output might be about 6 per cent. On the assumption that imports are no more luxurious than the typical good consumed, an income elasticity of unity would be reasonable.

The Demand Elasticities for Imports and Exports

The demand elasticity for a country's imports of a particular commodity can be expressed as

$$\eta_i = K_i\bar{\eta}_i + (K_i - 1)\bar{\delta}_i \tag{8}$$

where $\bar{\eta}_i$ is the elasticity of the domestic demand, $\bar{\delta}_i$ is the domestic supply elasticity, and K_i is the ratio of home consumption of the good to imports.[7] The elasticity of demand for imports will be $\sum_i W_i\eta_i$ where W_i is the share of the ith good in the total value of imports. The domestic demand elasticities of the individual goods, $\bar{\eta}_i$, are defined allowing for changes in the same direction of the prices of exports and other import goods. For any commodity, $\bar{\eta}_i$ will be less than the ordinary demand elasticity by a larger amount, the greater the substitutability in consumption between the good and other import and export goods, the greater the supply elasticities of other imports, the smaller the supply elasticities of exports, and the greater the shares of the low supply-elasticity exports and high supply-elasticity imports in total exports and imports.[8] The domestic supply elasticities of the individual

[7] Imports can be expressed as the excess of domestic consumption over domestic production. Thus $M_i = C_i - O_i$. Equation (7) can be obtained by differentiating this and translating the results into elasticity form. At this point it is assumed that the market area for the country's export goods is fixed.

[8] The lower the supply elasticities of the exports of, for example, the devaluing country, the greater will be the tendency for export prices in home currency to rise. Similarily, the greater the supply elasticities of imports, the greater the rise in import prices.

import goods are defined to allow for the external effects on costs which result from simultaneous expansion or contraction of all export and import goods industries.

For purposes of estimating the plausible magnitudes of the import and export demand elasticities, it is assumed that the domestic and foreign elasticities of supply and demand for all export and import goods are the same. Our excess demand relation then becomes:

$$\eta = \left(\sum_i W_i K_i\right)\bar{\eta} + \left(\sum_i W_i K_i - 1\right)\bar{\delta} \tag{9}$$

where $\bar{\eta}_i = \bar{\eta}$ and $\bar{\delta}_i = \bar{\delta}$ for all i. This equation can be used to express the demand elasticities for either U.S. exports or imports by the inclusion of the appropriate subscripts.

Empirical estimates of $\sum_i W_{xi} K_{xi}$ and $\sum_i W_{mi} K_{mi}$ can be constructed from data provided by the World Trade Information Service [13] [14] [15]. For each export good the ratio of foreign production to U.S. exports equals

$$\frac{Of_{xi}}{X_i} = \frac{Ou_{xi}}{X_i} \cdot \frac{Of_{xi}}{Ou_{xi}}$$

where Ou_{xi} and Of_{xi} are the domestic and foreign outputs of the ith good. The data sources give the ratios by commodity of U.S. production to exports for about 60 per cent of the value of goods exported. For each product the average ratio for the years 1957–59 was calculated. These ratios were weighted giving $W_i(Ou_{xi}/X_i)$.[9] To calculate the weighted ratios of foreign production to exports, these must be multiplied by Of_{xi}/Ou_{xi}. The ratios of free-world production outside the United States to U.S. production were calculated for a wide range of goods from United Nations' data [12]. The ratios of free-world value added, again excluding the United States, to value added in the United States by broad industrial classification were obtained from the same source. For about 90 per cent of the value of exports these ratios exceed unity.

A problem arises with respect to the empirical application of these ratios. Production in certain areas of the world may not be in the same market as U.S. output. That is, transportation costs may exceed the price difference between the United States and other areas. A small variation in export prices may not affect prices in these separated areas. This isolated production, and consumption, must be eliminated from the foreign output figures used in calculating Of_{xi}/Ou_{xi}. There is no way, however, to determine with any precision the magnitude of the adjustment required.

In an attempt to adjust for the above fact we assume that all Of_{xi}/Ou_{xi} equal unity. This amounts to saying that the United States produces half of that portion of free-world output that is in the same market. As a result,

[9] Those exports for which data were not available were assigned ratios equal to the weighted average of those in the industrial classification to which the particular exports belong.

$\sum_i W_{xi}(Ou_{xi}/X_i)$ becomes an estimate of $\sum_i W_{xi}K_{xi}-1$. Our procedure yields an estimate of $\sum_i W_{xi}K_{xi}$ equal to 13. Had the values calculated above for the appropriate classification or the actual commodities been used, this estimate would have been equal to 26. The ratios are thus adjusted downward by approximately half.

The ratios of new supply (domestic production plus imports) to U.S. imports for various agricultural products, primary products, and primary manufactures are given in the World Trade Information Service data. Subtracting unity from these we get Ou_{mi}/M_i. Multiplying by the weights and summing over i, we obtain $\sum_i W_{mi}K_{mi}-1$. Because no allowance was made for that part of domestic production which may not be in the same market as imports, we arbitrarily reduce this by one-half. The resulting empirical estimate of $\sum_i W_{mi}K_{mi}$ is 4. It is based on about two-thirds of merchandise imports.

Using the above estimates of the ratios, the demand elasticities for imports and exports can be expressed

$$\eta_x = 13\bar{\eta}_x + 12\bar{\delta}_r \qquad (10)$$
$$\eta_m = 4\bar{\eta}_m + 3\bar{\delta}_m.$$

Statistical estimates of the demand elasticities for various products can be found in studies by Stone [10], Harberger [4], and Schultz [9, pp. 189–91]. The evidence suggests that, if long-run adjustment is considered, most nonagricultural goods will have price elasticities of demand close to unity or above. The demand elasticities for farm products appear to be substantially lower. These estimates will be overstatements of the price elasticities to be used here in that they include a wider range of substitution effects. Another bias, however, will work in the opposite direction. Consumption which was not part of the same market as U.S. output may enter as export prices fall. And consumption which is initially in the same market as imports may leave as import prices rise.

A number of economists, beginning with Nerlove [7], have calculated long-run supply elasticities for individual farm products substantially in excess of unity. For nonfarm commodities the elasticities are undoubtedly much higher since a smaller proportion of the inputs are dependent upon natural resources. Again, however, these estimates will overvalue the supply elasticities we wish to obtain, in that the production of all export and import goods will change. And they will tend to undervalue the elasticities because no allowance is made for increases or decreases in the size of the market as prices vary. The parameters $\bar{\delta}_x$ and $\bar{\delta}_m$ represent the abilities of the U.S. and foreign economies to shift resources between the production of domestic and international goods. The domestic supply elasticity of a country's import goods will be greater, the smaller the share of international goods in total output. The supply elasticity of U.S. exports will be larger, the greater the initial degree of unemployment.

If available statistical estimates of the demand and supply elasticities are used, there is some chance that the degree of overvaluation will be under-

stated. Since the objective of this paper is to give an upper-limit estimate of the overvaluation and lower-limit estimates of the elasticities, it is desirable to choose supply and demand elasticities that are inconceivably low. It seems unreasonable that the elasticity of demand for the typical commodity in the long run, say three or four years, could be less than .3, allowing for the biases considered above. Similarly, it does not seem plausible that a fall in the price of U.S. export goods of, say, 10 per cent could bring about anything less than a 5 per cent decrease in domestic supply, given three or four years for adjustment to take place. Accordingly we let the supply elasticities equal .5.

Substitution of $\bar{\eta}_x=\bar{\eta}_m=.3$ and $\bar{\delta}_x=\bar{\delta}_m=.5$ into equation (10) gives estimates of the demand elasticities of exports and imports of 9.9 and 2.7 respectively.

The Supply Elasticities of Exports and Imports

The supply of a country's exports is the excess of domestic production over domestic consumption. For the *i*th good, the elasticity of supply can be expressed as

$$\delta_i = (q_i + 1)\overset{*}{\delta}_i + q_i\overset{*}{\eta}_i \tag{11}$$

where q_i is the ratio of domestic consumption to exports, and $\overset{*}{\eta}_i$ and $\overset{*}{\delta}_i$, the domestic demand and supply elasticities, are subject to all of the qualifications applied to the supply and demand elasticities of import goods. The supply elasticity of exports can be written $\sum_i Z_i\delta_i$ where Z_i is the fraction of the total value of exports contributed by the *i*th good. Assuming as before that $\eta_i=\eta$ and $\delta_i=\delta$ for all i, we have

$$\delta = \left(\sum_i Z_i q_i + 1\right)\overset{*}{\delta} + \left(\sum_i Z_i q_i\right)\overset{*}{\eta}. \tag{12}$$

This equation can be applied to both U.S. exports and imports by addition of the appropriate subscripts.

Empirical estimates of $\sum_i Z_{xi}q_{xi}$ and $\sum_i Z_{mi}q_{mi}$ have in effect been calculated already. For each good, Ou_{xi}/X_i is an estimate of the ratio of U.S. production to exports. Weighting by Z_{xi} and summing, we have $\sum_i Z_{xi}q_{xi}+1$. This equals 12 and is arbitrarily reduced to 6 to take account of the fact that some U.S. production may not be in the same market as exports. On the import side, q_{mi} equals

$$\frac{Ow_{mi}}{Ou_{mi}}\cdot\frac{Ou_{mi}}{M_i}.$$

We assume that $Ow_{mi}/Ou_{mi}=1$ for all i. As in the case of exports, this will result in a substantial understatement. The resulting estimate of $\sum_i Z_{mi}q_{mi}$ is 7.

The supply elasticities of exports and imports can thus be written

$$\delta_x = 6\overset{*}{\delta}_x + 5\overset{*}{\eta}_x \tag{13}$$

$$\delta_m = 8\overset{*}{\delta}_m + 7\overset{*}{\eta}_m.$$

Applying the same elasticities of demand and supply of export and import goods as were used before, we get supply elasticities of exports and imports of about 4.5 and 6.1 respectively.

Product Differentiation

There is additional bias in our estimates of the demand elasticities of exports and imports due to the imperfect substitutability of many U.S. goods for their foreign counterparts. The poorer the substitutability of the ith U.S. good for ith good abroad, the lower will be the demand elasticity for exports and/or imports of this good.[10] A fall in the price of U.S. exports relative to the prices of these commodities abroad will cause foreign demand to shift from foreign output to domestic production. The foreign supply price of the export goods will fall. If the goods are perfect substitutes, the shift in consumption will occur until the domestic and foreign prices are equal. Where they are not perfect substitutes, the shift in demand will stop before prices are equalized.

The excess-demand formulation can be modified to allow for imperfect substitutability of domestic and foreign goods by dividing the importing country's consumption into imports and domestic goods. Thus

$$M_i = C_{Fi} + C_{Di} - O_{Di} \tag{14}$$

where C_{Fi} and C_{Di} are the importing country's consumption of imports and domestic goods respectively, and O_{Di} is domestic production. Differentiating this with respect to the foreign price of imports and assuming that the prices are the same initially at home and abroad, we derive

$$\eta_i = \bar{\eta}_i + (K_i - 1)\theta_i\bar{\eta}_i + (K_i - 1)\theta_i\bar{\delta}_i, \tag{15}$$

where θ_i is the ultimate relative change in the domestic price divided by the relative change in the foreign prices of the good in question, and the other parameters are as defined previously. The term $\bar{\eta}_i$ is the domestic demand elasticity of the ith good, assuming that the domestic and foreign prices vary proportionally. If the domestic and foreign versions of the good are perfect substitutes, the price ultimately falls by the same amount at home and abroad, θ_i equals unity, and (15) becomes identical with (8). If there is no substitutability, θ equals zero and $\eta_i = \bar{\eta}_i$. Summing over i and assuming that $\bar{\eta}_i = \eta$, $\bar{\delta}_i = \delta$, and $\theta_i = \theta$ for all i, (15) becomes an empirically manageable relationship. The extent of product differentiation can be allowed for by an appropriate assumption about θ. It would seem reasonable to regard θ as equal, on the average, to no less than one-half. This means, for example, that, if the U.S. price of export goods falls by 10 per cent, equilibrium will be re-established when the foreign price has fallen by no less than 5 per cent.

[10] In this context we define an industry as those goods which go by the same name—e.g., automobiles, tires, etc.

This allowance for product differentiation reduces the estimated demand elasticities of exports and imports to 5.1 and 1.5 respectively.

III. *Conclusions*

The application of the above empirical magnitudes to formula (7) indicates an overvaluation of the dollar of 4½ per cent, not counting the effects of product differentiation. Had we utilized the ratios of foreign to U.S. production which are available, rather than assuming these equal to unity, and had we not reduced the ratios $\sum_i W_{mi}K_{mi}$ and $\sum_i Z_{xi}q_{xi}$ by one-half, the degree of overvaluation would have been only half as large. The use of demand and supply elasticities closer to available statistical estimates would have reduced the estimated overvaluation even further. The downward adjustment of the ratios was made to allow for the fact that all world production may not be in the same market as U.S. import and export goods. We have assumed that for the typical commodity one-half of world production is outside the market for U.S. exports and imports. The expansion or contraction of the size of the market as prices vary will tend to make the demand and supply elasticities of import and export goods higher than would be indicated by available statistical studies. On the other hand, the fact that the prices of all import and export goods move simultaneously in the same direction removes some of the substitution effects which are included in the statistical estimates. Inclusion of the allowance for product differentiation will raise the estimated overvaluation to about 7 per cent. It seems inconceivable that the long-run elasticities of demand and supply for traded goods could be less than .3 and .5 respectively. However, for those who might disagree with this judgment, the results are calculated for selected alternative values of the elasticities in Table 1. In the case where product differentiation is included, θ is assumed equal to .5.

The assumption that no changes in capital flows would result from a decline in the external value of the dollar must now be qualified. A continuation of the present commitment of the U.S. government to achieve full employment by fiscal policy could lead to a rise in domestic interest rates. It is also possible, of course, that the improvement in the trade balance would give license for an easing of monetary policy. Even if the depressing effects of the devaluation on aggregate demand abroad were eliminated by fiscal policy alone, a rise in foreign interest rates would be unlikely. The use of monetary policy would certainly reduce them. Thus there is no good reason to assume that a large capital outflow from the United States would result, making the required devaluation greater.

In our judgment the evidence strongly suggests that the dollar is overvalued by not more than 10 per cent. Indeed, quite plausible values of the parameters imply that the overvaluation is substantially less. This conclusion and the method by which it is derived might be criticized on the ground that too little weight is given to available econometric estimates of the demand elasticities of exports and imports. However, while subjective guesses about the parameters are admittedly imprecise, the econometric estimates have long been regarded as downward-biased. The estimates

TABLE 1—ESTIMATED ELASTICITIES AND DEGREE OF OVERVALUATION[a]

Basic Elasticities		Derived Elasticities								Overvaluation[b]	
		Supply		Product Differentiation			No Product Differentiation			Product Differ.	No Product Differ.
Demand	Supply	Exports	Imports	Marshall-Lerner Coefficient[c]	Demand: Exports	Demand: Imports	Marshal-Lerner Coefficient[c]	Demand: Exports	Demand: Imports	Per cent	
.01	.2	1.25	1.67	.79	1.27	.32	1.56	2.53	.64	35.4	17.2
.1	.2	1.70	2.30	1.35	1.90	.55	2.22	3.70	1.00	20.0	11.7
.2	.3	2.80	3.80	2.27	3.20	.95	3.59	6.20	1.70	11.4	7.0
.3	.5	4.50	6.10	3.88	5.10	1.50	5.70	9.90	2.70	6.7	4.4

[a] Capital flows are assumed to be unrelated to the level of or movements of the external value of the currency. The initial deficit is assumed to be $3.5 million.

[b] This table gives only one set of parameter values that will result in a given devaluation.

[c] This is the generalized Marshall-Lerner coefficient, equal to $S\eta_x\left(\frac{1+\delta_x}{\eta_x+\delta_x}\right)+\eta_m\left(\frac{1+\delta_m}{\eta_m+\delta_m}\right)-1$.

presented here represent an attempt to do as much as possible with existing theoretical knowledge, ordinary common sense, and the inadequate empirical evidence currently at hand. They should be regarded as a check on, rather than a substitute for, more careful statistical estimates. As such, they give some indication of the magnitude of the bias in earlier calculations.

Finally, the empirical results of this paper indicate the virtual impossibility that, aside from speculative influences, the international price mechanism could be unstable. In none of the examples presented in Table 1 is the sum of the export and import demand elasticities less than unity. It is possible, of course, to choose estimates of the basic demand and supply elasticities which would make the sum of the elasticities of demand for exports and imports arbitrarily small. Under these circumstances, however, unless the domestic demand and supply conditions for export and import goods differ very substantially from conditions abroad, the supply elasticities of exports and imports will also be extremely low, and the stability of the price mechanism will be ensured. And it can be further concluded that for values of the parameters that most economists would consider reasonable, the foreign exchange market will be not only stable but highly so.

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Liability in Law and Economics

What is the appropriate rule for the compensation of persons damaged by accident?

I. *The Rule in Law*

American law contains a number of alternatives.[1] Usually, compensation is required to be paid when the defendant is at fault; sometimes it is given whether or not he is at fault; and sometimes it is not given whether or not he is at fault.

1. In general, persons suffering injuries in accidents are compensated for the damage done them *as a result of negligence*.[2] Liability is, that is to say, usually derived from "fault." It is not enough that the person who suffers damage prove that another was its author; he must also show that the injury was done intentionally or that the actor had exposed others to a foreseeable and unreasonable risk of harm. The negligence rule contains the possibility that harm can be innocently caused.

2. In a few cases, the rule of strict liability or liability without fault applies. Negligence need not be shown, and the exercise of reasonable or diligent care is not a sufficient defense. It suffices that harm was done by the activity to require that compensation be given. Strict liability has been construed to cover, for example, cases in which the probability of damage to others is very high; when many suffer, if harm is done; when the return on the activity which causes harm is high for those who engage in it, relative to the loss of those who suffer damage; or when a danger is introduced into the community that is not common to the community. Strict liability has been held to flow from the keeping of wild animals, the manufacture and storage of high explosives, the trespass of domestic animals upon the property of another, the spread of fire, and the dusting of crops with chemical sprays.

The instance of injuries from accidents at work is, in one sense, analogous to that of strict liability. Compensation is given willy-nilly and without regard to fault. Workmen's compensation statutes have been enacted in all of the states and, in addition, for some classes of employment, by the Congress. In all of them, the principle of liability without fault rules. No attention is given to the question of negligence, either on the part of employer or worker. It is enough to establish that the injury is work-connected and to define the injury.

3. In other cases, the doctrine of "assumption or risk" applies, and those

[1] The law has been taken largely from the second volume of Harper and James [2].
[2] The meaning of "negligence" in the law will be discussed in Part II.

suffering damage cannot hold others accountable for harm they suffer. Here there is no liability at all and no compensation for injuries. The doctrine governs where the risk is inherent in a relationship freely and voluntarily entered into and where the risk is known or obvious. The owner of an automobile, for example, cannot be held liable by one who borrows it, if the borrower knows it is mechanically defective.

Where the rule requires compensation, what is its optimal magnitude? Here, too, there are alternatives in the U.S. law. Sometimes it is just about equal to the loss suffered; sometimes it is more than this, and sometimes less.

(a) The most common rule is that the injury should be "repaired" or that the damaged person should be "made whole." Compensation, that is to say, should be measured by the "extent of the injury." This includes money losses, such as loss of earnings and the capacity to earn, and medical and other expenses; and nonmoney losses, such as mental and physical anguish.

(b) If the injury was intended or a wrong was flagrantly done, exemplary or punitive damages (exceeding the actual damage suffered) will be allowed. Where there is a "cause of action" (as assault and battery, for example) and there has been no actual damage shown, nominal damages will nonetheless be allowed and, in this case, too, compensation will exceed the actual damage suffered.

(c) In the common law there is no right to recover for "wrongful death"; the right to compensation of survivors derives from statute and, in many instances, the legislation puts a ceiling upon the amount recoverable which is not infrequently less than the loss incurred by survivors. Air lines engaged in international transport operations are limited in their liability, by international convention, to $8,300-$16,600 for injuries and death of their passengers caused by accidents,[3] and this, too, is often smaller than the value of the loss.

II. *The Rule of Economics*

Putting to one side ethical questions implicit in the compensation principle, the primary economic object of a liability rule applied to activities causing personal injuries or death is the prevention of accidents, and this because either of these occurrences deprives society of the output the injured or dead person may have produced had the accident not occurred.

The social purpose is served by the contrivance of incentives for the prevention of accidents or for the prevention of injuries when accidents do occur. The incidence of accidents is a partial negative function of the quantity of resources devoted to accident-prevention. The larger the incentive, the larger will be the quantity of resources put to this use, and the smaller will be the number of accidents. The incentive may take the form of costs imposed upon those whose behavior causes accidents. But it is unlikely that the social welfare is maximized by the prevention of *all* accidents because the cost of achieving a zero incidence would undoubtedly be too great. What is wanted is the use, in preventing accidents, of that quantity of resources such that the value of the extra resources used to save the marginal life is equal to the value of that life and the equimarginal condition is satisfied. Then a unit of resources

[3] This will be discussed in detail in Part III.

put to this use will have the same yield as in any other. Yield is measured, in this case, at least by the expected output of the marginal life sayed, net of the expected lifetime consumption of the relevant person, discounted at an appropriate time rate. To this is added an amount considered to be equal to the value put upon life by the deceased person. Thus the whole loss to be associated with death caused by accident is the sum of the loss to the rest of society (the deceased's discounted net output) and the loss to the deceased himself (the state of "living" treated as a consumption good). If either less or more resources are put to this use than is implied by this principle, there is social waste. Either too few or too many accidents occur, too few or too many injuries or deaths ensue, and, given the cost, too much or too little of otherwise lost output is retained.

That there may be "too few" accidents is not ordinarily seen because it is thought by many that the value of human life is infinite, but neither society, in the allocation of collective resources, nor individuals act as though it were. If people behaved as though they valued their own lives infinitely, they would never act in ways with which there is associated a probability of loss of life that is positive. But this is positive even in the ingestion of food prepared by others or prepared from ingredients produced by others, since the expected probability that it contains toxic elements is greater than zero. Just as we observe that people consume such food, we observe, too, that they do not shut themselves away to live as recluses, protected from the hazards of the roadways and of association with others in society.

Every activity—say, every industry—can be perceived to divide into parts. The product of the one is its nominal object—plate glass or painted house exteriors; the products of the others are the reduction by some magnitude of the probability that some undesired event will transpire—accident, theft, illness, etc. These products may be called un-accidents, un-thefts, etc.

The resources used by an industry can be seen to be devoted either to one or another of the parts into which it has been perceptually divided; some of them are factors in the production of the nominal object of the industry, some are factors in producing fewer unwanted side effects. Here we treat only one of these secondary products—un-accidents.

The equimarginal rule defines, in principle, the optimal size of the "accident-prevention industry" and specifies that it can be too large.

This is a rule derived from economics that spells out the socially optimal extent to which care should be taken to prevent harm to others. It appears to be different from the rule of the law. There the care which is required to be exercised can be found in the construction the courts have put upon the word "negligence."

"Negligence" is defined as "conduct . . . which falls below the standard established by law for the protection of others against unreasonable risk of harm." The standard of conduct required by the law is said to be an objective one and it is what a reasonably prudent person would do in the circumstances. The actor is presumed to be able to foresee the consequences of his act as much as they would be so foreseen by a reasonably prudent man; when he undertakes an activity (driving an automobile or pulling teeth, for example),

he is presumed to have the skill possessed by reasonable, competent, and experienced persons engaged in these activities; he must take account of his own physical, mental, or emotional disabilities and must act as a reasonably prudent man would who was similarly handicapped. What is said to be the most widely quoted definitional statement of negligence appears in an English case [2, p. 928]:

> Negligence is the omission to do something which a reasonable man, guided upon those considerations which ordinarily regulate the conduct of human affairs, would do, or doing something which the prudent and reasonable man would not do.

The rule of law is said to be objective (in the sense that it does not examine the intent of the author), but the outcome of every case depends upon the subjective judgment of judge or jury on how a hypothetical "reasonable man" would act in like circumstances. Economics applies a different standard, and this is the standard of the maximizing man who is defined in terms that permit mensuration, at least in principle.

III. *The Execution of the Rule of Economics: The Case of International Air Transport*

Can a system be devised that would tend to cause the optimal amount of care to be taken that harm not be done to others? This can be conveniently discussed in the context of a particular class of harm; that done by accidents in international air transport has been chosen as the frame of reference.

If an accident occurs on a domestic flight in the United States and a passenger is injured, the common law rule of negligence applies to determine whether the air-line is liable and, if it is established that it is, the line is bound to make the injured passenger "whole" by compensating him fully for his losses.

If, however, the accident occurs in the course of an international flight, another rule applies. The Warsaw Convention of 1929,[4] which became effective in 1933 and to which the United States adhered in 1934, limits the liability of air lines to each passenger to 125,000 gold francs (about $8,300) unless wilful (intentional) misconduct can be shown.

This is, of course, very much less than the value of the losses of some passengers and their survivors from accidental injury or death and is quite probably less than the mean value of the loss of all passengers who have been victims of air-line accidents. A Civil Aeronautics Board study of passenger insurance settlements in cases not covered by the Warsaw Convention (i.e., mainly generated by accidents in domestic flight) indicated, for the period 1950-60, mean compensation of $25,281 for 792 passenger fatalities and of $19,926 for 225 serious injuries [7]. The incomes of American international air travelers are much higher than those of all Americans.[5]

[4] Convention for the Unification of Certain Rules relating to International Carriage by Air, signed in Warsaw on 12 October 1929 [4, Part 2, pp. 3000 ff.]; the Convention also appears in Drion [1, Annex I, pp. 346 ff.]. The International Civil Aviation Organization advises that, on March 31, 1964, 94 countries had adhered to the Convention.

[5] This can be inferred by a comparison of census data [5, Table 1, p. 2] and data on the incomes of overseas air travelers [3, Fig. J., p. 18].

The limitation of liability was strongly criticized, mainly on ethical grounds, and The Hague Protocol was negotiated in 1955 to modify the Warsaw Convention by retaining limited liability but raising the limit to $16,600. The Protocol secured sufficient country ratifications to become effective, for the ratifying countries, on August 1, 1963. The President has asked the U.S. Senate to consent to ratification, but (autumn 1964) it has not done so.

The Hague Protocol reopened the whole question of U.S. policy and produced an interagency review of policy and a sometimes acrimonious contention in which parties in interest participated.

A. *The Alternative Rules in Air Transport*

A number of alternatives were suggested or suggest themselves:

1. Renounce the Warsaw Convention and let tort law govern so that air lines found at fault will compensate victims for the whole of their losses.

2. Let liability be limited to $8,300, or $16,600, or some other larger number.

3. Let liability be limited and require air lines to purchase automatic insurance, payable upon mere showing of injury or death from air transport accidents, in some amount—say, $25,000—for each of its passengers in international travel.

4. Let liability be limited to zero, so that air carriers would sell pure transport services, perhaps returning the passenger's payment for the ticket if an accident causes nonfulfillment of contract, and have each passenger buy, at his own explicit cost, insurance that would assure him or his survivors recovery of loss suffered in an accident.

It is necessary, first, to see whether aircraft accident rates can be diminished by the construction of incentives for accident-prevention for, if they cannot be diminished, the search for the optimal rule is pointless. It is sometimes said that the desire of the air carriers that they imperil neither their equipment nor their custom and the desire of air crews that they not imperil their lives already compel the exercise of a degree of care such that the incidence of accidents is at an irreducible minimum so far as human behavior can control. That this is not correct is clear from an examination of the causes of accidents. The CAB found in 1959, for example, the following, among other, causes of accidents [9, pp. 20-21]:

> . . . pilot errors such as misuse of engine and propeller controls, improper use of flaps, failure to extend gear, excess of aircraft stress limits, improper pre-flight planning, failure to maintain flying speed, inadequate maintenance-inspection, improper operation of ground facilities, improper training and supervision of ground personnel, failures of the power plant, airframe, landing gear and equipment, operation in inclement weather, and defects in airport terrain.

Almost all aircraft accidents seem, at least to the inexpert eye observing the bare recital of their causes, to be preventable by the use of more resources in the selection of air crews and ground personnel and in screening them for retention, in the design of aircraft, in the construction of airports, and in the

holding of aircraft and other equipment, crews and other staff, and passenge
idle while flights are delayed to await more favorable weather.

It is necessary, secondly, to distinguish the transfer payment aspect fro the economic aspect of the rule of liability. The former is concerned with t payments that should be made by those who cause harm to those who a harmed so that justice is best done; on this economics is silent. The latt approaches the problem from a real resource vantage point; it asks: whic rule, given the cost of saving some fraction of air passenger human resourc from extinction or impairment of the capacity to produce goods and service will cause the correct quantity of real resources costs to be incurred in acc dent prevention?

B. *An Appropriate Rule in Air Transport*

An appropriate rule is a modification of alternative 1 in III A above an contains the following components:

1. The air carrier should be held liable for all harm which *it* does (i.e which could have been avoided, if its behavior had been different).

2. When liable, the liability should be without limit, and the carrier shoul compensate those it has harmed to the full extent of their losses.

3. Carriers should be self-insured or, if not, liability insurance premium rat should be different among carriers (in principle, among flights for a give carrier) and proportional to the different degrees of care exercised by differe carriers (for which differentiated carrier accident rates can be taken as proxie and to different values of passenger loads among carriers (and among flights

The first two components of the rule are necessary to secure the exercise the appropriate degree of care. Limited liability will not do unless it is draw at a sufficiently high level so that it is higher than the loss prospectively be suffered by any passenger, and then it becomes irrelevant; at any low level than this, the rule of limited liability promotes an insufficient attentic to care, measured by the quantum of resources used to prevent accidents.

Unlimited liability will not, of course, cause that line of behavior to ens which will eliminate all preventable accidents, and it should not. Ration maximizing air lines will find a margin at which it will pay to permit acciden to occur rather than incurring the cost of preventing any more of them. Th accident rate will, that is to say, be positive. The social welfare will b maximized by such behavior in the same way that it is by the use of some co crete and steel for the building of sports arenas and baseball stadiums rath than accident-preventing railroad overpasses.

The third component of the appropriate rule is essential in order that th consequences of behavioral decisions that incorporate less than the appropria quantum of care not be pooled among carriers. If liability insurance premiu rates are equal for all carriers, the care of some of them produces an incenti for lack of care of others. An air line may spend to buy accident-preventi resources or it may spend to compensate the victims of accidents. The tw forms of expenditure are substitutes for one another. How much is spent f the one purpose and how much for the other depends upon the relative pric of the two. If the accident experience of all is pooled by the actuaries in th

calculation of liability insurance premiums, the price of compensation to victims is lower for the "careless" firm, relative to the price of accident prevention, than it would be if its own experience established its own premium rate. It could be expected, therefore, to substitute compensation to victims for accident-prevention. There are, of course, differences in accident rates among air lines and classes of air lines.[6]

Air carrier self-insurance would accomplish the same object as appropriately differentiated insurance rates. Each would suffer the consequences of its own lack of care and, therefore, each would have an incentive to take care of the magnitude appropriate to the hazard imposed by the objective circumstances *it* confronts.

Self-insurance would also secure the correct allocation of accident-prevention resources among flights. Passengers on some air routes have higher mean earnings than those on others.[7] Using earnings as a proxy for productivity tells us that the social cost of the death or injury of high-income earners is larger than that of low-income earners. Therefore, the social optimum requires the exercise of greater care (the use of more accident-prevention resources per head) to prevent harm to the former than to the latter. The liability rule here proposed, if it incorporates self-insurance, will cause the maximizing behavior of the carriers to coincide with the requirements of the interests of society in the aggregate. This is so because the cost to the carrier in payments for compensating accident victims will be the larger, the higher the earnings of passengers on any given flight and, thus, the larger will be its incentive to devote resources to assure the safe execution of the flight. If the carrier carried liability insurance, premium rates ought to be differentiated by flight (say, by route as a first approximation) to take account of the different mean social worths of different passengers so that larger incentives are given for the prevention of accidents that may harm more-highly-valued passengers.

The cost of a changeover from the present rule of limited liability of air carriers in international operations to one of unlimited liability would apparently not be large. A CAB study estimated that insurance costs would rise, if the United States withdrew from the Warsaw Convention, by 18 cents per 1,000 revenue miles and that this might cause the fare from New York to London to rise by 62 cents. [7].

[6] The mean number of passenger fatalities per 100 million passenger-miles flown was, for the decade of the 1950's, 0.61 for U.S. certificated route air carriers in domestic operations, 0.79 for U.S. certificated route air carriers in international operations, and 2.69 for U.S. supplemental air carriers in passenger operations. Computed from [6, p. 79] and [8, pp. 526-27]. Accident and fatality experience by air line is available in [9].

Accident rates are, of course, only a *ceteris paribus* function of the quantity of care exercised by the carriers. The frequency of landings and takeoffs, the quality of the airports used and of their guidance equipment, and climatological differences among routes flown by different air lines are among a host of other variables that will affect accident rates. Each line should, however, presumably take account of the relevant variables and of the estimated values of each in making decision about *its* employment of accident-prevention resources.

[7] The median family incomes of passengers flying overseas out of New York in 1956-57 were more than $15,000 for those terminating in the Caribbean, other than Puerto Rico; $13,500 for travelers to South America; and $10,000 for travelers to Europe [3, p. 18].

The solution to the liability problem here offered appears a dismal one, for it says that it is not worth while to safeguard all from harm and that some are less deserving of care than others. But it really will enlarge human welfare, while an apparently happier solution will really have an opposite and adverse effect upon it.

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Factor Marginal Products and Decreasing Opportunity Cost

Upward convexity of the production-possibility curve has been an exceedingly convenient assumption to make when dealing with problems of allocative efficiency. Constant returns to industry scale permit us to say that competitive markets equate marginal transformation and substitution rates, and this condition for efficiency becomes a sufficient one through convexity of the production set.

One way of providing for it in a general-equilibrium model has been to assume differences in factor intensities between industries. To put the argument loosely, if manufacturing is labor-intensive and agriculture land-intensive, a shift of output from the latter to the former leads to a rise of wages and a fall of rent. Manufacturing, since it uses relatively more labor and less land, undergoes a rise in cost compared to agriculture; and this fact makes the transformation curve convex.

A variant of the same argument is to assume that at least one sector has a factor employed only there, which gets increasing rent as output in the sector expands. This is a special case of the different-intensities argument. A rather different but related explanation is to assume that natural units of a factor, like labor, have unequal efficiencies in particular employments and differ in

their comparative efficiencies between different uses. When labor shifts from making X to making Y, it is first the units with the highest Y-efficiency in relation to their X-efficiency which make the move. Subsequent units that move are relatively less effective in this sense, and so opportunity cost rises. A last, and still different basis for convexity is that there are differing degrees of aversion to different kinds of employment, and the wage differential of the expanding sector must increase in order to attract the more reluctant units of labor.[1]

It is the first of these reasons for convexity that we shall examine here. In order to exclude the others, we assume from this point on that each factor, whether defined broadly or narrowly, is alike with respect to efficiency in each employment and preferences between employments, and that total supply of each factor is completely inelastic.

I. *Convexity with Two Factors*

Bishop and Samuelson have given a simple way of showing that, barring scale economies, the transformation curve for two goods must lie outside a straight line connecting the two end points of the curve [3]. Figures 1a and 1b illustrate this proposition.

Since all resources are fully employed at all times, the mix of factors used at point y, with complete specialization in manufactures, must be the same as at point x, where only agricultural goods are produced. Both sectors operate at constant returns to scale, and consequently it is always possible to move along a straight line from y to x by transferring resources in the fixed proportions prevailing at the extreme points. In general this straight line is not the most efficient path because different sectors would not combine factors in the same way. With free mobility and flexible prices, there is trading of resources until every firm in every industry has the same marginal transformation rate for corresponding pairs of factors. The economy moves onto the contract curve and in the process increases at least some outputs. Once on the curve it maximizes any one output, given all the other quantities. Thus movement to the contract curve means movement to a transformation curve which is above the diagonal everywhere except at the end points.

A more difficult question is how the transformation curve looks between the extremes. In the case of two commodities and two factors we infer the answer from the difference and nonreversibility of factor intensities, and the law of variable proportions. Ferguson [1] has illustrated how one can divide a given stock of one resource (say, capital) in a fixed way between two sectors and vary the allocation of the other factor between them, to get one of a family of convex transformation curves. We get an envelope of such curves by shifting successive doses of capital from one to the other employment; and this envelope turns out to be convex as well.

It is fairly easy to give an analytical proof of convexity in the case of two factors and two commodities by using the properties of homogeneous first-degree production functions. Let the two goods be manufactures (M) and

[1] A good historical survey of theories on this subject is given by Richard E. Caves in *Trade and Economic Structure* (Cambridge, Mass. 1960), pp. 30-37.

Figure 1a
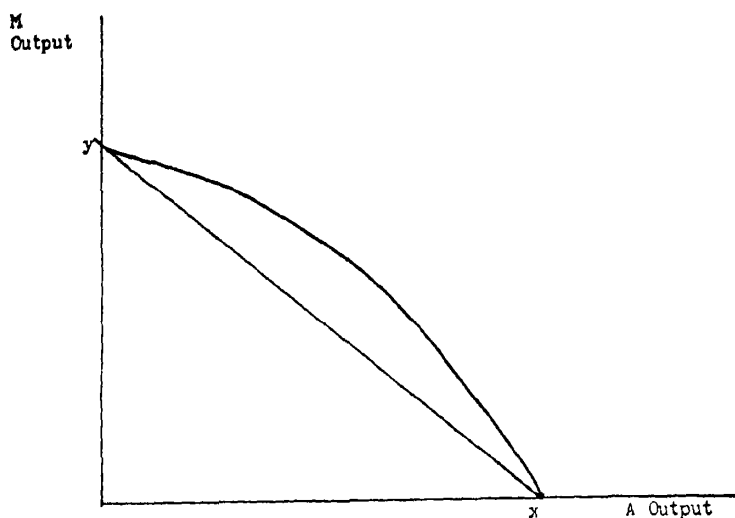
Figure 1b
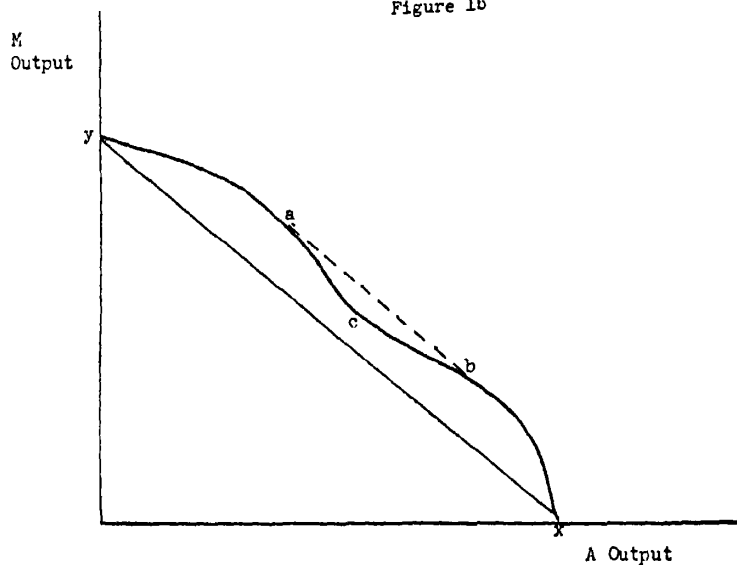
FIGURE 1

agricultural goods (A), and the two factors be labor (L) and capital (K). So we have

$$M = f(L_M, K_M) = L_M f_L + K_M f_K \tag{1}$$

$$A = g(L_A, K_A) = L_A g_L + K_A g_K$$

(from Euler's theorem).

Dividing the first through by M and the second by A, we get

$$a_{LM}f_L + a_{KM}f_K = 1 \tag{2}$$
$$a_{LA}g_L + a_{KA}g_K = 1$$

where the a_{ij}'s are the input coefficients of the ith factor into the jth good. But we know from the fact that the production functions are first-degree and homogeneous that

$$a_{LM}df_L + a_{KM}df_K = 0 \tag{3}$$
$$a_{LA}dg_L + a_{KA}dg_K = 0.$$

Consequently, when we differentiate the functions (2), it is also true that

$$f_L da_{LM} + f_K da_{KM} = 0 \tag{4}$$
$$g_L da_{LA} + g_K da_{KA} = 0.$$

Therefore, when differentiating each price, we have simply

$$dp_M = a_{LM}dw_L + a_{KM}dw_K \tag{5}$$
$$dp_A = a_{LA}dw_L + a_{KA}dw_K.$$

If manufacturing is labor-intensive then $a_{LM}/a_{LA} > a_{KM}/a_{KA}$, and an increase of its output at the expense of agriculture means a rise of wages and a fall of capital's rental. This fact in turn is enough to assure that $dp_M > dp_A$; that is, manufactures become more costly in terms of agricultural goods.[2]

Parenthetically, we may add that if factor intensities were to reverse themselves, all that would happen would be a flattening of the transformation curve at the point where the factor ratios become equal. But in either direction the factor ratios move apart again (if in different directions); and this fact is enough to make the curve convex.

II. *Effect of More Than Two Factors*

When there are three factors rather than two, say, labor (L), capital (K), and land (R), the foregoing proof of convexity is no longer valid. The last pair of equations then read:

$$dp_M = a_{LM}dw_M + a_{KM}dw_K + a_{RM}dw_R \tag{6}$$
$$dp_A = a_{LA}dw_M + a_{KA}dw_K + a_{RA}dw_R.$$

Let us assume that the following inequalities apply:

$$\frac{a_{LM}}{a_{LA}} > \frac{a_{KM}}{a_{KA}} > \frac{a_{RM}}{a_{RA}}.$$

There is no longer anything to guarantee that the price of manufactures would rise in relation to agricultural goods, even though wages rise and land rents fall. There is generally an element of cost change due to the intermediate factor, in this case capital, and the rental on capital could go either up or

[2] M. C. Kemp suggests a different proof involving factor ratios. However, it is less illuminating for present purposes. See *The Pure Theory of International Trade* (Englewood Cliffs, N.J. 1964), Ch. 2, Problem 2.1.

down. The size of the ratios tells us nothing about the share in costs of particular factors, and so the change in price of the intermediate factor could tip the balance in either direction. Such values as the following would lead to a fall in the relative price of M as output shifts in that direction:

$$a_{LM} = 2 \qquad a_{KM} = 2 \qquad a_{RM} = 1$$

$$a_{LA} = 1 \qquad a_{KA} = 3 \qquad a_{RA} = 2$$

$$dw_L = +.01 \qquad dw_K = +.03 \qquad dw_R = -.01.$$

The foregoing may not be quite enough to convince the skeptical reader that concavity is just as possible as convexity when there are three or more factors. He may have a lingering suspicion that there is some restriction in a general-equilibrium system that prevents factor prices from moving quite in the way here described. In order to dispel such doubts we shall shift our line of attack and examine what happens to the marginal product of one particular factor in each employment. If that factor's marginal product rises in the expanding sector and falls in the contracting one, we get decreasing opportunity cost and thus concavity.

We suppose as before that resources shift from agriculture to manufacturing and that labor becomes scarcer and land more plentiful. We expect their marginal products to move in accordance with their changing scarcities. What happens to capital, as the intermediate factor, depends on its technical relations to the other factors. If we apply Hick's [2] definition of complementarity to the case of constant returns to scale, then factors L and K are complements when an increase of the ratio L/K raises K's marginal product and lowers that of L. Hicks calls them substitutes when the opposite happens to their marginal products; and we can call them neutral if changing their ratio has no direct effect on either of the marginal products.

Now let us suppose that in the M-sector capital is complementary to land but neutral with respect to labor. In the A-sector, capital is complementary to labor, but neutral with respect to land. Moreover, labor and land are complementary to one another. Now, when the M-sector expands and the A-sector contracts, both tend to substitute land for labor in response to shifts in factor prices. Consequently labor's marginal product goes up in both sectors, and the marginal product of land goes down in both; but that of capital rises in the expanding M-sector and falls in the contracting A-sector. If capital's marginal transformation rate into manufactures has increased and its transformation rate into agricultural goods has decreased, then clearly M-goods must have gotten cheaper in terms of A-goods. We have M-expansion at decreasing opportunity cost.

We should also examine whether there are further adjustments in factor proportions, due in particular to the behavior of capital's marginal rate of transformation into M and A, which might finally make M cost more in terms of A after all. Firms in minimizing costs would tend to make further adjustments in factor proportions. They have already reduced employment of labor per unit of output and increased employment of land, in order to adjust their marginal products to their changing factor scarcities. In the case of capital

the initial changes in marginal products are as already described. But now a substitution of capital for the other factors takes place in M, because its marginal product went up at the initial employment per unit of output. For the converse reason there is a reduction in employment of capital per unit of A.

It is not difficult to see that these secondary adjustments can only temper but not reverse the change in relative costs of M and A. A shift in a factor's marginal-product schedule is the same in its effect on product-cost as a change in the factor's price. When firms in A economize on capital because of its reduced effectiveness, they must still end up with higher costs than they had before the change, even though they have modified the cost increase. Firms in M have already enjoyed a cost reduction and now reduce costs further by appropriate substitution of capital for the other factors. Thus after all is done, we are still left with M cheaper in terms of A.

Referring to Figure 1b, one might further object against our concavity argument, since it is always possible to move along a straight line between points x and y, why is it not possible to move in the same way between points a and b? That is, can one transfer composite fixed units of resources in such a way as to get a constant gradient between a and b? The answer is no, because M and A do not have the same factor mix either at a or at b. Consequently factor scarcities change in the process of moving, and both sectors must alter their factor combinations. It is this fact which makes the production path go through c, as the transformation rate of capital into M and A changes in the way described in the previous paragraphs.

A pair of industry-production functions with the factor relations described above are the following:

$$M = a_1 L_M^{3/4} R_M^{1/4} + a_2 K_M^{1/2} R_M^{1/2}$$

$$A = b_1 L_A^{1/4} R_A^{3/4} + b_2 L_A^{1/2} K_A^{1/2} \quad .$$

We assume that a_1 is large enough compared to a_2, and b_1 great enough compared to b_2, to make M labor-intensive and A land-intensive. With p representing the price of M in terms of A, the return to capital is

$$W_k = \tfrac{1}{2}a_2 \left(\frac{R_M}{K_M}\right)^{1/2} \cdot p = \tfrac{1}{2}b_2 \left(\frac{L_A}{K_A}\right)^{1/2},$$

and therefore

$$p = b_2 \left(\frac{L_A}{K_A}\right)^{1/2} \div a_2 \left(\frac{R_M}{K_M}\right)^{1/2} .$$

The system is completed with equations for the three factor supplies, which we take as given:

$$\bar{L} = L_A + L_M$$

$$\bar{K} = K_A + K_M$$

$$\bar{R} = R_A + R_M .$$

So long as factor substitution continues to be possible in both sectors, a shift of resources from A toward M leads to a fall of L_A/K_A and to a rise of R_M/K_M, so that p must fall.

However, we need a further restriction on the shape of the transformation curve, because the curve as a whole must lie above the diagonal connecting its end points, as we showed above. And this is true no matter how many factors of production there are. If the production functions trace out a concave section for the transformation curve, other parts of the curve must have enough convexity to keep it above the diagonal.[3]

A common-sense justification of the change from concavity back to convexity is that under a given state of the arts there is some limit to substitution against a scarce factor. This can be taken care of rather simply in the particular model above by introducing two side conditions:

$$a_{LM} = \frac{L_M}{M} \geq k_1$$

$$a_{RA} = \frac{R_A}{A} \geq k_2.$$

When one of the limits is reached, we get an inflection point, and the curve changes from concave back to convex. This situation is illustrated in Figure 2.

Points q and r are such inflection points. The former is reached when L_M/M reaches its lower limit k_1, and further increments of M-output therefore call for constant additions of labor. But the A-sector continues to substitute land and capital for labor, to make labor's maginal product rise. Thus, going northwest from point q, each successive unit of A given up releases less labor and therefore yields less additional M-output.

Going southeast, the inflection point r is reached when R_A/A reaches its lower limit k_2. From this point on, manufacturing economizes on land per unit of output, but agriculture cannot, as land becomes scarcer. The rising marginal product of land in the M-sector means ever smaller additions to A-output per unit of lost M-output; and so we get convexity at the lower end of the curve.

A last question on the shape of the production-possibility curve is what happens as the number of goods and factors increases. Generalizations become more difficult, and I shall make only one. Each good has a different production function, and it is possible for shifts of production to take place either between two individual goods or between groups of goods. So long as there are at least three factors, it would seem possible a priori to have increased output in one direction take place at either decreasing or increasing cost in terms of one or more other outputs; and the number of goods involved has no particular effect on the relative likelihood of the one or other shape of the curve.

III. *Equilibrium in the Concave Portion*

I come now to the question whether there can be equilibrium on the production-possibility curve when the production point is on a concave portion. It

[3] This point was first made to me by J. G. Witte.
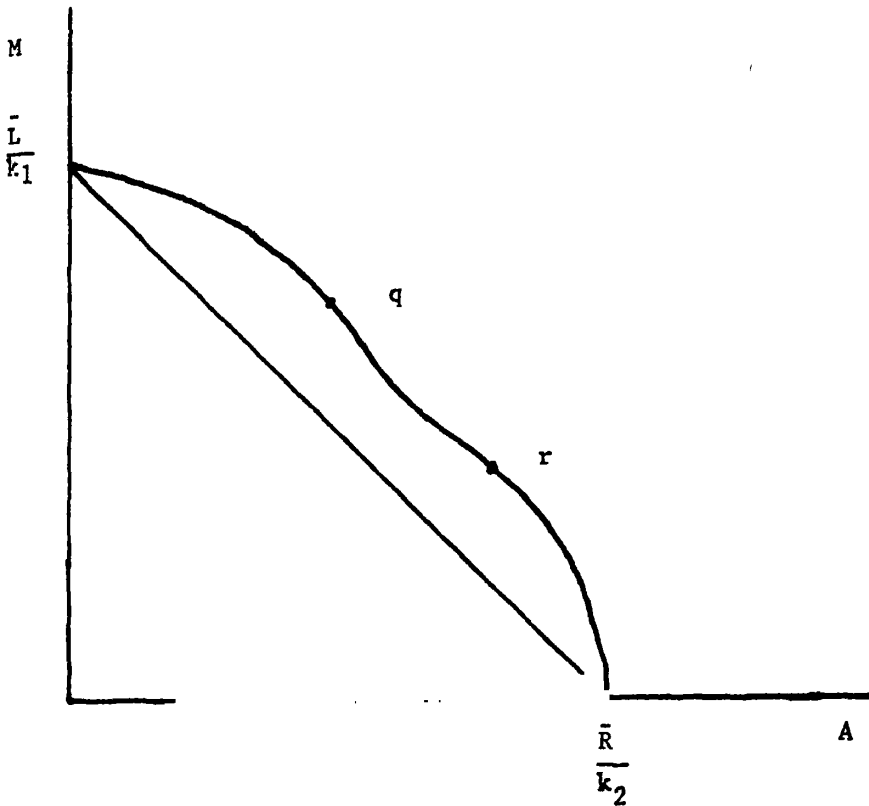
FIGURE 2

should be emphasized that the curve is not a blown-up average of the production-possibility curves of firms. Firms will never, in competitive equilibrium, be on an increasing-returns portion of their production functions; and expansion of a sector will generally take place both through expansion of existing firms and through the addition of more firms. What happens is something like the situation under external economies of scale: Resources change in cost and in effectiveness as a result of changed relative demands for their services. Consequently the cost curves of all firms in the expanding sector together undergo a downward shift. It differs from external economies in that there is no difference between marginal social and marginal private costs or between social and private marginal products. The forces affecting curvature are fundamentally the same as when there are only two factors; but the presence of three factors rather than two introduces more possibilities for the shape of the transformation curve.

Now suppose that an expansion of one sector has been brought about by means of a government subsidy of that sector, financed by a tax on the other sector. It may under certain conditions be possible to cancel the subsidy once the desired shift of output has been obtained, and the new output point would be stable. This would be the case if industry supply sloped downward more

steeply than demand at the initial equilibrium, but eventually became less steep and intersected demand again from below at the higher output. The initial equilibrium is "imperfectly" stable so long as the underlying demand and supply conditions prevail. The individual firm would have no reason to change output because it has rising marginal cost. But if all firms together were induced to expand or contract output there would be a cumulative tendency to move toward a different equilibrium.

If demand fell more steeply than supply the subsidy to output would have to be permanent, given the object of keeping output at the permanently higher level. If the subsidy were removed, output would eventually return to the initial point. For a closed economy a set of consumption-indifference curves more strongly concave than the production-possibility curve would assure that demand is steeper than supply. For an open economy we need world demand for the country's output to slope off more steeply than the country's supply. In terms of foreign-trade offer curves this condition is fulfilled when the offer curves cut from the normal forward direction.

FRANZ GEHRELS*

* I wish to make a correction in the interpretation of Figure 1, to be amplified in a later note: it would be *physically* possible to move along the line *AB*, but the price system cannot bring such a movement about.

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The Foundations of the "War on Poverty"

Poverty has become an important term in contemporary America. Much has been said in recent months about its level and the prospects for its removal, with most of the discussion being concentrated upon the increasing reluctance of poverty to disappear in the face of economic growth. In this paper an attempt will be made to explore more fully the basis for the growing concern over the incidence of poverty, concern that has culminated in the current "war on poverty."

As representative of the thinking that has inspired the antipoverty crusade, I quote a brief passage from a statement by Walter Heller in his capacity as chairman of the Council of Economic Advisers. These remarks were made before a legislative subcommittee conducting hearings on the "Economic Opportunity Act of 1964" and read in part, "Clearly, we cannot rely on the general progress of the economy—or on job creating programs alone—to erase poverty in America."

To me, this statement suggests that there exists in the United States a sizable number of individuals who are relatively unaffected by the general course of economic progress. For lack of a better term, these people may be considered to be in the "backwash" of American economic life: in effect, non-

participants in the increasing affluence of this society. But what is the evidence to support this belief in what I shall henceforth call the "backwash" thesis? Its general nature can be gleaned from a more extended quotation from the Heller statement:

> From 1947 to 1956, the proportion of American families with annual income below $3,000 (in today's prices) dropped from 32 to 23 percent—a rate of 1 percentage point a year. But in the period since 1956, this proportion edged down only from 23 to 20 percent—and the actual number of persons in poverty scarcely fell.
>
> Much of this distressing slowdown can be traced to slower economic growth, to economic slack, to unemployment which has not dropped below 5 percent of our labor force for 76 consecutive months.
>
> The new tax cut will sharply step up our rate of economic growth. By creating 2 to 3 million new jobs, it will open exits from poverty at a faster pace. But open exits mean little to those who cannot move—to the millions who are caught in the web of poverty through illiteracy, lack of skills, racial discrimination, broken homes, and ill health—conditions which are hardly touched by prosperity and growth.
>
> A surprisingly large percentage of poor persons already have some kind of a job. In 1962, 70 percent of poor families had at least one income earner, and 23 percent had two or more. The cause of poverty here is not lack of jobs, but lack of higher skills and productivity needed to yield a decent income. As the demand for labor rises, many part-time or laid-off earners will receive higher wages. But most of the poor earners already have full-time employment, and added jobs will not help. They must be equipped with the knowledge, skills, and health to find and hold better jobs [7, pp. 26-30].

In summary, this statement appears to say: (1) There has been a decline since 1956 in the rate at which we are eliminating poverty. (2) Part of this decline is due to the general sluggishness of the economy in this period. (3) However, a substantial part must also be attributed to the presence of a "hard-core" group of people who are not basically affected by the normal processes of economic growth.

These propositions will be taken as fairly typical of the content of the backwash thesis as it has emerged in the form of the war on poverty. Other statements of the thesis can be found, but this particular one has the distinct advantage of emanating from authority. Thus, it is less vulnerable to the charge of being a "straw" man.

The remainder of this paper will be devoted to an evaluation of the backwash thesis, as stated here, with the discussion being divided into two parts: a consideration of the relationship between poverty and the backwash premise and an empirical evaluation of the validity of the backwash thesis.

I. *The Concept of Poverty*

At the outset let us define what we mean by the term "poverty." Obviously, such a definition requires some type of value judgment and is sufficient in itself to warrant an extended discussion. However, this paper is concerned with

the analytics of poverty rather than with its moral overtones and, consequently, will not attempt any justification of a particular definition of poverty. Instead, we will abide by consensus and accept the common definition of a level of income of less than $3,000 per year for a family as being indicative of poverty. (This also implies acceptance of the $1,500 boundary for single individuals even though the empirical evidence which will be presented refers only to families.)

The $3,000 boundary is used in the Heller statement and provides the basis for the contention that there has been a drastic "slowdown" since 1956 in the rate at which poverty is being dissipated in the United States. There can be no general quarrel with the data that Heller cites. Table 1 presents estimates of the percentage of families with income of less than $3,000 per year (in 1963 prices) for various years in the period 1947-63. These show a decline from 31.7 per cent to 22.2 per cent (9.5 percentage points) between 1947 and 1956 and a further decline of only 3.7 percentage points between 1956 and 1963. This amounts to an average decline of 1.06 percentage points between 1947 and 1956 as compared to only 0.53 percentage points between 1956 and 1963. Thus, the rate at which poverty was eliminated among families in the latter period was exactly one-half that of the earlier interval.

There are two possible frameworks within which such a decline may be interpreted: one which assumes a linear relationship between time and the percentage of families in the poverty class and another which assumes a non-linear relationship. To understand the differences between these two approaches, let us commence with an assumption that there is a linear relationship. Now, there is nothing magical about the elapse of time which guarantees a decrease in the percentage of families in the poverty category: rather, time

TABLE 1—PER CENT OF FAMILIES WITH LESS THAN $3,000 ANNUAL INCOME, MEDIAN FAMILY INCOME, AND UNEMPLOYMENT RATE, UNITED STATES, 1947–1963

Year	Per Cent of Families with Less than $3,000 Annual Income (1963 prices)	Median Family Income (1963 prices)	Unemployment Rate (per cent of Civilian Labor Force)
1947	31.7%	$3,896	3.9%
1950	31.2	3,961	5.3
1953	25.8	4,542	2.9
1954	27.4	4,458	5.6
1955	24.4	4,738	4.4
1956	22.2	5,051	4.2
1957	22.2	5,072	4.3
1958	22.5	5,052	6.8
1959	21.4	5,337	5.5
1960	20.9	5,451	5.6
1961	20.9	5,506	6.7
1962	19.7	5,651	5.6
1963	18.5	5,873	5.7

Sources: [8] [10].

in this instance serves as a proxy for some other variable and it would seem reasonable to assume that this variable is some measure of economic growth and progress in the United States. This being true, a further assumption to the effect that this variable has had the same relative importance through time leads to the conclusion, in the linear case, that there has been some significant change in the relationship between economic progress and the rate of elimination of poverty in the 1956-63 period vis-à-vis 1947-56. Very simply, the linear interpretation of poverty-elimination rates implies a fundamental alteration through time in the relationship between economic progress and the extent of poverty.

However, there are difficulties inherent in such a line of reasoning. For one thing, the economic-progress variable has changed significantly between the two periods. For example, the data of Table 1 show an annual rate of increase of median family income of 2.93 per cent over the interval 1947-56 as contrasted to an annual rate of increase of 2.18 per cent since 1956. Of course, the Heller statement takes this into consideration; but it then seems to imply that this is insufficient to account for the magnitude of the decline in the poverty-elimination rate.

The second difficulty with the chain of reasoning just described lies in the linearity assumption itself. On the basis of our general knowledge of the character of income distributions, a strictly linear relationship between economic progress and the number of families contained in the poverty class would be suspect, for it suggests that the income distribution has a positive intercept at its lower extreme. But beyond that, a strictly linear relationship argues that families below the $3,000 boundary fall into only two groups: one which is completely unresponsive to changes in economic conditions and another in which all families are equally responsive to such changes. Thus, with a given change in economic conditions there are only two transformation rates between poverty and nonpoverty which need to be considered: a nonzero and a zero one. Symbolically, this implies:

$$P = B + (a + \alpha E) \tag{1}$$

where a is a constant and P denotes the percentage in the poverty class, B the group (in percentage terms) completely unresponsive to changes in economic conditions, and E some measure of economic progress in the economy. Differentiating (1) shows that $dP \,/\, dE = \alpha$, which is merely the poverty-elimination rate.

The restrictive character of the linearity assumption can be seen by relaxing the condition of only two possible transformation rates, one zero and one nonzero. If this is done, (1) becomes

$$P = \sum_{i=1}^{n} w_i(a_i + \alpha_i E) \tag{2}$$

where w_i = the weight of the ith group in the poverty class and the other symbols retain their previous meaning except that they refer to subgroups of the poverty class when the subscript i is appended.

Now, if the w_i's remain constant through time, (2) can be simplified and differentiated to produce

$$\frac{dP}{dE} = \sum_{i=1}^{n} w_i \alpha_i, \tag{2a}$$

which is not conceptually different from the poverty-elimination rate derived from (1). However, the assumption of invariant w_i's is a strange one. Given groups among the poverty class with differential transformation rates, those with higher rates will be shifted out of the class more quickly with a consequent lowering of the aggregate weighted transformation rate (i.e., the poverty-elimination rate). Thus, abandoning the simple assumption of only two transformation rates (zero and nonzero) suggests that the poverty-elimination rate will decrease through time as economic conditions continue to improve. In short, a less restrictive set of assumptions suggests a nonlinear relationship between economic progress and the extent of poverty.

It would seem from this discussion that, depending on what type of relationship is assumed between the extent of poverty and economic progress, two different versions of the backwash thesis are possible. On the basis of a linear relationship, a decline in the poverty-elimination rate would be indicative of an intertemporal "shift" in the parameter associated with the economic progress variable.[1] On the other hand, an assumed nonlinear relationship merely implies a poverty-elimination rate that declines with additional economic progress, a situation which produces a residual group in the society who are increasingly impervious to the benefits of economic progress. In this case, the size of this hard-core group is vital in evaluating the backwash thesis.

II. *Empirical Results*

At this point we are in a position to commence an empirical examination of the backwash thesis in its various forms. It is proposed to do this by considering the basic relationship between economic progress in the United States and the extent of poverty. The rationale of this procedure should become apparent as we proceed. As a first step, some measure of the level of economic activity in the economy is necessary. The measure selected in this case is the median family income during the time periods under consideration, expressed, of course, in 1963 prices. This measure is chosen on two grounds: (1) It is available from the same set of data from which the magnitude of the poverty group is estimated. (2) It is independent of the validity of the backwash thesis, i.e., the truth of the backwash proposition will not affect an estimate of median income (since all those in the backwash group fall on the low side of the median), whereas it would affect the behavior of a simple mean, for example.

These data for the years 1947-63 were first used to estimate a simple linear regression of the form

$$\Delta P = a + b\Delta M + u \tag{3}$$

where M denotes median family income and u is a random-error term. The

[1] This form of the backwash thesis is strikingly similar to the structural-unemployment argument which has been so generally rejected [1] [2] [3] [4] [5] [6] [9].

results were:

$$\text{(3a)} \qquad \Delta P = .4574 - \underset{(.0026)}{.0095}\Delta M, \quad R^2 = .93.$$

First differences were used in estimating the regression in order to minimize the technical problems that arise as the result of regressing two time series with strong trend factors against one another. The results suggest a powerful relationship between the poverty-elimination rate (ΔP) and changes in median family income. In fact, such changes account for over 90 per cent of the variation in the poverty-elimination rate. This would seem to reflect adversely on any contention that there has been some basic shift in the relationship between the extent of poverty and economic progress since 1956.

This still leaves the nonlinear form of the backwash thesis to be considered. In order to explore it a regression of the form

$$\text{(4)} \qquad P = ab^{-M}u$$

was estimated with the following results:[2]

$$\text{(4a)} \qquad \log P = 1.946277 - \underset{(.000015)}{.000116}M, \quad R^2 = .98.$$

Regression (4a) demonstrates a clear relationship between the level of median income and the percentage of families in the poverty class. However, an examination of the residuals of (4a) shows an interesting pattern. In general, the value of P which is predicted from (4a) is less than the actual value in years of relatively high unemployment and greater than the actual in years of relatively low unemployment. Accordingly, an unemployment variable was explicitly included in the regression. Again, a logarithmic form was used with the following results:

$$\text{(5)} \qquad \log P = 1.945100 - \underset{(.000003)}{.000121}M + \underset{(.001431)}{.005086}U, \quad R^2 = .99+$$

where U denotes the annual average unemployment rate (see Table 1 for data).

The addition of the unemployment variable improves the fit noticeably and

[2] Spurious correlation as the result of regressing the actual time-series data against one another would not be anticipated, given the results of regression (3a) which employed first-difference data. First differences can be used with (4) with the regression taking the form

$$\frac{\Delta P}{P} = m + n\,M + u$$

where m is a constant and n represents the logarithm of the constant b in (4). The results of fitting such a regression are:

$$\frac{\Delta P}{P} = .012417 - \underset{(.000025)}{.000331}M, \quad R^2 = .93.$$

These results would seem to dispel the fear of spurious correlation.

increases the significance of the coefficient associated with median family income markedly. The partial correlation coefficients are .99 for M and .77 for U, both of which are significant at the .05 level.

The results of (5) rather clearly indicate that the behavior of P can be explained by the levels of median family income and unemployment in the system. An additional test of this conclusion was conducted by recomputing regression (5) for the years 1947-62 and using the results to predict P for the year 1963. The predicted P was 18.3 per cent, a change of -1.4 percentage points from the actual value of P for 1962. From Table 1 it can be seen that the actual P for 1963 is 18.5 per cent. Thus, the actual change between 1962 and 1963 is about 85 per cent of the predicted change, a discrepancy which could easily be accounted for by sampling errors in the data.

III. *Conclusions*

The data and analysis presented to this point argue that the combination of a lagging real growth rate and high unemployment rates are the primary causes of the decline in the poverty-elimination rate. However, this does not completely disprove either the backwash thesis or the proposition that economic growth is not an adequate device for eliminating poverty. The existence of a nonlinear relationship between the extent of poverty and the economic-activity variables admits the existence of a declining poverty-elimination rate and, consequently, the possibility that the future impact of economic growth and progress on the extent of poverty will be minimal. In fact, on the basis of a projection into the future, the Council of Economic Advisers estimates that at 1947-56 levels of activity, economic progress would only reduce the extent of poverty among families to 10 per cent by 1980 and at post-1956 levels of activity to only 13 per cent, [8, p. 60]. This suggests that economic progress will be a relatively ineffective instrument for eliminating poverty.

Fortunately, the regressions developed in our analysis can also be used to make projections of this type. By making various assumptions as to the growth rate of median family income and levels of unemployment, estimates of the extent of poverty in various years can be calculated. For example, we can calculate what the value of P would have been in 1963 if 1947-56 growth rates (2.93 per cent per year for median family income) and 4 per cent unemployment had been maintained. Rather than the actual 18.5 per cent, it would have been approximately 16.6 per cent.

In Table 2 projections for the years 1970 and 1980 based on two sets of assumptions are presented. The one set assumes the 1947-56 growth rate in median family income of 2.93 per cent and 4 per cent unemployment, while the other assumes the 1957-63 annual growth rate of 2.18 per cent and 6 per cent unemployment. For comparative purposes the Council of Economic Advisers' estimates are also presented. The estimates from our regressions predict values for the extent of poverty in 1980 that are about 35 per cent below the Council of Economic Advisers' estimates. Apparently, the latter are estimated by calculating a percentage-rate decline in poverty ($\Delta P/P$: equal to approximately 4.0 per cent a year in the 1947-56 period) and then using this rate to solve

the following expression:

$$P = \frac{P_0}{\left(1 + \frac{\Delta P}{P}\right)^t} \tag{6}$$

where the subscript o denotes a base period and t the number of years elapsed. This expression produces a 9.7 per cent estimate for 1980 on the basis of 1947-56 performance and 13.8 per cent assuming 1957-62 performance (there will be some discrepancies between these calculations and the Council estimates due to the data in this paper being based on 1963 prices rather than 1962 as in the Council data).

The marked difference between the Council projections and those derived from our regressions is at first puzzling, for both are based on an assumption of a nonlinear relationship between time (a proxy for economic progress) and the extent of poverty. Unfortunately, though, expression (6) implicitly assumes declining growth rates in the years ahead. To understand why this is so, consider the basic relationship between the extent of poverty and the economic-activity variables which has been validated by our regressions:

$$P = ab^{-M}c^{U}. \tag{7}$$

Differentiating (7) produces

$$\frac{dP}{P} = - dM \log b + dU \log c. \tag{7a}$$

Further, assume the unemployment rate remains unchanged. In this case, the second term of the right-hand side of (7a) becomes zero, and we are left with

$$\frac{dP}{P} = - dM \log b. \tag{8}$$

Now, the Council of Economic Advisers-type estimate assumes dP/P constant over time. Consequently,

$$- dM_0 \log b \equiv - dM_1 \log b \tag{9}$$

TABLE 2—ESTIMATES OF PERCENTAGE OF FAMILIES IN POVERTY CLASS, UNITED STATES, 1970 AND 1980

Year	Council of Economic Advisers' Estimates		Regression Estimates	
	1947–56 Poverty Elimination Rate	1957–62 Poverty Elimination Rate	1947–56 Growth Rate of Median Family Income and 4% Unemployment	1957–63 Growth Rate of Median Family Income and 6% Unemployment
1970	—	—	12.6%	14.2%
1980	10%	13%	6.4	8.7

Sources: [8] and calculations.

where the subscripts denote successive time periods. Equation (9) implies a constant dM over time which, assuming monotonic growth, necessitates persistent declines in the growth rate of median family income.

The declining growth rate implicit in the Council of Economic Advisers' projections results in an understatement of the impact of economic progress on the extent of poverty. The estimates from our regressions suggest that economic growth and progress have a much more significant role to play than the Council's estimates would indicate. Consequently, the extent of the hard-core poverty group and the significance of the backwash thesis are overstated by the Council's projections.

Of course, all of this has implications for the world of economic policy. After all, a projected 1980 hard-core poverty group consisting of one family in every ten is a much more compelling reason for action beyond the realm of mere stimulation of economic growth than is a projected hard-core group numbering one family out of sixteen. The former is strongly suggestive of a need for selective programs designed to accomplish some of the ends suggested in the Heller statement which was quoted earlier, namely, higher skills, better health, etc. However, a 6 per cent estimate of poverty by 1980 raises questions as to how much selective programs can be expected to accomplish. Objectionable as such a level of poverty may be to some (and this is ultimately a matter of a person's value judgments), in all probability, there is some irreducible minimum below which the value of P cannot be pushed short of direct-subsidy programs. Ten per cent undoubtedly leaves a substantial amount of room for progress before that minimum is reached; but 6 per cent? That may be quite a different matter. In short, the case for substantial antipoverty programs of a selective character is weakened by the findings of this paper. Rather, greater consideration should be given to the role which economic growth can play in eliminating poverty.

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Comparative Labor Effectiveness and the Leontief Scarce-Factor Paradox

In his celebrated study of the structure of U.S. foreign trade, Wassily Leontief [4] came to the surprising conclusion that the United States specializes in labor-intensive lines of production. Specifically he found that an average million dollars' worth of exports embodies considerably less capital and somewhat more labor than would be required to replace from domestic production an equivalent amount of competitive imports:

	Exports	Import Replacements
Capital (dollars, 1947 prices)	2,550,780	3,091,339
Labor (man-year)	182.313	170.004
Capital-Labor Ratio (dollars per man-year)	$14,010	$18,180

It would thus seem that this country resorts to foreign trade in order to economize its capital and dispose of its surplus labor. Since the United States is known to have a far higher capital/labor ratio than its trading partners, this finding contradicts the basic Ohlin model which explains international trade in terms of relative factor endowments. Under it a country would export those products which are intensive in its relatively abundant resource and import those which are intensive in its relatively scarce resource.

A wealth of statistical and theoretical material has been published in the past decade in an attempt to reconcile the empirical finding with the Heckscher-Ohlin theorem. Leontief himself maintained that the explanation lies in the superiority of U.S. labor. If with a given quantity of capital, one man-year of U.S. labor is equivalent to, say, three man-years of foreign labor, then the U.S. labor force must be multiplied by three in comparing the relative amounts of labor and capital with that of foreign countries. That would mean that in 1960 the U.S. labor force numbered not 70 but 210 million of "equivalent" foreign man-years, making it a relatively labor-abundant country. Since it exports relatively labor-intensive products, the finding is consistent with Ohlin's theory.

I. *Comparative Labor Effectiveness*

This paper is devoted to an empirical examination of Leontief's hypothesis concerning the superiority of U.S. labor. The problem does not easily lend itself to statistical measurement because of the difficulty of isolating the quality of labor from other factors affecting productivity.[1] The degree of mechanization, the volume of production, and the length of the production runs must be held constant. (The last two factors are important in determining productivity [9, chap. 10] [8], but they have a strong influence on the degree of mechanization [9, p. 56].) And the alleged superiority of U.S. management and organization, which Leontief emphasized, is admissible only if it increases the productivity of U.S. labor much more than it raises the efficiency of U.S. capital. Since no such a priori assumption can be made, this factor should also be kept equal.

Thus, it is necessary to compare labor effectiveness here and abroad under restrictive *ceteris paribus* assumptions. Leontief's contention "could be proved, if it could be shown that there are cases where the amount of capital employed per unit of a product is the same in America and some representative country abroad, and that that of labor is three times as much in the latter than in the former." [2, p. 41]. In addition to testing this proposition, the labor-effectiveness comparison would be of general interest to business and academic economists. However, one reservation must be borne in mind. Even the "isolated" labor-effectiveness factor does not have a "neat" bearing on the Leontief explanation because the shortage of skilled workers sometimes forces the use of less sophisticated machinery.

For the purpose of making the comparison it was decided to rely on the informed judgment of managers and engineers who are familiar with production conditons here and abroad. A wealth of information is available to companies operating subsidiaries in one or more foreign countries. Although their U.S. and foreign plants (producing similar products) often vary in organization and degree of mechanization, firms can be asked to "hold these factors constant" and concentrate on the characteristics of the labor force. This point is illustrated in the following quotations from one respondent:

> Due to the fact that the equipment used in our overseas plants is on a less sophisticated basis than that used in our Stateside plants, it is a bit difficult to eliminate this difference and end up, as you state, with just the qualities of the labor force. I shall attempt to evaluate this to the best of my ability, and I do feel that from my travels and knowledge of the working forces of our various plants that I do have a good basis for evaluation.

To be sure such an evaluation can provide only a rough order of magnitude. But if many managers and engineers of companies producing diverse products provide consistent answers, and when a fair proportion of the replies is based

[1] Although aggregate production functions can embody a variable representing labor effectiveness, outside information is necessary to establish the reasons for variations in that factor. See [3].

on time-motion studies (made for the purpose of establishing wage-incentive systems), such responses cannot be taken lightly.[2] Moreover, only a fair degree of accuracy may suffice to establish or reject Leontief's contention, and the figures would be revealing even if subject to a high margin of error.

II. *The Survey*

A directory of U.S. firms operating abroad [1] was used to identify names and addresses of possible respondents. After excluding all nonproducing organizations, such as advertising agencies, law and accounting firms, brokers, and insurance and finance companies, questionnaires were sent to about 2000 companies. An individually typed, personally signed cover letter accompanied each questionnaire, explaining in detail the intent of the inquiry and assuring confidential handling of the material. Firms were requested to compare the labor time required per unit of output in their operations here and abroad under similar organization and degree of mechanization, and to state the reasons for any differences. They were then asked whether the comparison applied to the labor force in general or only to their particular firm. If the latter was the case, they were asked to supply the reasons for the difference. Finally, they were requested to compare the general labor forces in the United States and the foreign country. These questions were supplemented by inquiries concerning the relative ease of recruiting employees and of training them here and abroad.[3] Part of the questionnaire is reproduced in the Appendix.

Altogether, 913 replies were received. Evidently, the Directory used contained many companies which merely do business, but have no production facilities, abroad. Consequently, 669 of the respondents did not supply the necessary information, for the following reasons:

Reason:	Number of Firms
Have no overseas production facilities	446
Manufacture only abroad and not in the U.S.	15
Have no direct control over foreign operations (e.g., produce only on a license or contract basis)	76
Operations not comparable (either product line or productive process)	37
Information not available	45
Company has a standing policy against responding to such requests; has no staff to fill questionnaire	26
Producing abroad for too short a period to draw conclusions	24
TOTAL	669

[2] The survey thus demonstrates a method of obtaining directly from business firms information relevant to economic problems. I suspect that one reason for the relatively high response rate was business interest in the results, as evidenced by the large number of respondents who requested a copy of the findings.

[3] Most firms find it easier to recruit workers in the United States than in Europe because of the labor shortage prevailing there. It is also easier to recruit skilled workers in the United States than in Latin America, but the reverse is true for unskilled labor. In Canada, conditions are similar to those prevailing in the United States.

With respect to the duration and the cost on training workers, the responses varied a great deal within each geographical area, and no consistent trend could be established.

Many of the 244 firms which supplied usable data had plants in two or more countries, yielding a total of 367 observations. A wide array of products and industries is covered in the comparison, and the number of countries represented exceeds 30. Canada, Europe, and Latin America are the areas appearing in the greatest frequency.

Three points must be made before presenting the results. First, inter-countries comparisons are sometimes overshadowed by the large differences in labor effectiveness within countries. The great variations between northern and southern Italy illustrate the point. Nevertheless, this reservation does not preclude the use of some representative figure for the country. Second, the labor employed in U.S. subsidiaries abroad may not be representative of the general labor force of a nation. Particularly in underdeveloped areas it may be above average, either because labor is attracted to U.S. firms[4] or because these firms are more selective in their recruitment practices and spend more time and money on the initial training. Since the questionnaire specifically inquired about such cases, and also gathered data on recruitment and training, it was possible to allow for this factor in the analysis. In tabulating the results, the comparison of the general labor forces was used when it deviated from the one pertaining to the individual company because of the reasons stated above. Thirdly, it was not always possible to isolate out the management factor. In 35 observations (7 per cent of the cases) ability of management and the effectiveness of the organization was given as one of the reasons for the differences in labor effectiveness. Interestingly enough, some of these firms indicated that this reason accounts for half the differences between the American and the foreign labor, while the characteristics of the workers are responsible for the other half.

III. *Main Results*

Table 1 presents the results of the survey, pertaining to all countries. It shows that U.S. labor is indeed superior to its foreign counterpart, but the superiority is nowhere near the level suggested by Leontief. Neither is it sufficient to "convert" the United States into a labor-abundant country. A realistic factor by which to multiply the number of U.S. workers in order to allow for the difference in effectiveness is 1⅕ or at most 1¼, but not 2 or 3.

Table 2 presents a frequency distribution of the reasons offered by the respondents for the differences. The total number of reasons does not coincide with the number of observations because the companies indicating a labor input ratio of 100 per cent did not give any reasons, while many other firms offered several reasons. The most common explanations for the relative inferiority of foreign labor are inadequate training and skill (code number 2), lack of proper motivation and drive (number 4), and low level of education (1). Also important are lack of discipline (number 5), union work rules (8), excessive job security (9), and inflexible attitude (3).

There are significant variations between countries and regions in the foreign/U.S. labor input ratio, and in the reasons advanced by responding firms (Tables

[4] Some respondents in Europe indicated a reverse attitude on the part of managerial personnel who prefer indigenous over foreign companies.

TABLE 1—COMPARATIVE U.S.-FOREIGN LABOR TIME REQUIRED PER UNIT OF OUTPUT

Foreign/U.S. Ratio	Number of Observations	Per Cent of Total
Below 100%	5	1
100%	104	29
101–120%	113	31
121–140%	71	19
141–160%	37	10
161–200%	27	7
Above 200%	10	3
TOTAL	367	100%

Mean Ratio	130%
Median Ratio	120%
Per cent of observations falling within 20 percentage points either side of the median	79%

3 and 4). In Canada the median ratio is 100 per cent, indicating equal effectiveness of labor. That the median is a good representative of all responses is shown by the fact that 93 per cent of the 71 observations fall within 15 percentage points (i.e., between 85 and 115 per cent) of the median. For Europe the labor input ratio is 120 per cent,[5] with 88 per cent of all observations falling within 20 percentage points of the median. The Latin American median of 130 per cent is also a fairly good representative of all responses. But in the underdeveloped countries of Africa and Asia, the ratio varies between 115 and 500 per cent, and the median of 175 is not representative. Also, the number of observations there is quite small.

Lack of education and inadequate skill and experience appear to be the predominant reasons for the differential in most underdeveloped areas, followed closely by lack of motivation and drive. In most European countries, the training, motivation and "union rules" factors play an important role, while in Germany the most frequent reason offered is mental inflexibility and desire to achieve perfection beyond what is required.

This comparison applies to a composite of the labor skills employed by the responding companies, and no attempt was made to classify workers into the broad categories suggested in Leontief's second article [5, table 2]. It should be noted in this connection that such a breakdown cannot be dissociated from the amount and intensity of the capital used. The number of engineers relative to that of unskilled workers employed by a company is often a function of the level of sophistication of its capital equipment.

IV. *Comparison with Other Studies*

It is the conclusion of this survey that while U.S. labor is more effective than its foreign counterpart, the superiority is not large enough to make the United

[5] Many responding firms emphasized that European labor is continuously improving.

TABLE 2—REASONS FOR DIFFERENCES IN LABOR EFFECTIVENESS BETWEEN U.S. AND FOREIGN WORKERS

	Number of Frequencies	Per Cent of Total
(A) *Reasons Relating to the Individual Worker*		
(1) Too low a level of education	68	14%
(2) Inadequate training, skill, practical experience, know-how, and mechanical aptitude (at times arises from lack of "manufacturing tradition")	102	22
(3) Mental inflexibility; lack of adaptability to changes in accepted procedures. Too much thoroughness and attempt to approach perfection (examples: polish components unnecessarily, or make components better than strict drawing requirements)	25	5
(4) Lack of motivation,[a] incentives, and drive. Lack of orientation toward efficiency (sometimes even laziness and apathy). "Spread the work" attitude	87	19
(5) Lack of interest in work, discipline, loyalty and dependability. Excessive absenteeism. Prejudice against owners and managers. Too high labor turnover; too many interruptions in production.	34	7
(6) Little personal initiative; slow ability to make decisions	14	3
(7) Low level of health and nourishment (of either indigenous or immigrant labor).	18	4
(B) *National and Labor Union Policies:*		
(8) Union rules concerning work conditions, job definition, conditions of overtime work, etc.; excessive power of shop stewards.	30	6
(9) Excessive job security created by union and/or gov't policies	25	5
(10) Wage structure: Resistance to wage-incentive plans; wage structure based on seniority and number of dependents rather than performance; too much fringe benefits compared to direct payment	14	3
(C) *Conditions in the Country*		
(11) Climate (heat and humidity) interferes with efficiency	6	1
(12) Overemployment in country (leads to excessive turnover and to the "hoarding" of labor through make-work jobs necessitates use of marginal workers; forces companies to open small plants in villages where workers can be found; language barrier, where immigrant labor is present)	21	4
(13) Low effectiveness of management; inadequate supervision; poor organization.	35	7
Total	479	100%

[a] Included here are several responses relating to difficulties created by the European social class structure. (See also *New York Times*, Financial Page, July 4, 1964.)

States a relatively labor-intensive country. This conclusion would remain intact even if the 20 per cent median superiority were doubled to account for the possibility that superior management increases the effectiveness of labor more than that of machinery. These results are consistent with two other

TABLE 3—FOREIGN/U.S. LABOR REQUIREMENTS PER UNIT OF OUTPUT BY REGION AND COUNTRY

Country or Region	Mean (per cent)	Median (per cent)	Per Cent of Observation Within 15 Percentage Points of Median	Per Cent of Observation Within 20 Percentage Points of Median	Number of Observations
Canada	102	100	93%	94%	71
Europe	123	120	67%	88%	171
Germany	115	115	93%	93%	28
Benelux	118	120	66%	91%	32
Italy and France	128	120	56%	81%	33
United Kingdom	126	120	73%	88%	51
Other Europe	126	119	68%	84%	27
Japan, Australia, New Zealand, South Africa	132	110	66%	69%	35
Latin America	149	130	49%	72%	76
Mexico	130	125	71%	71%	24
Other Lat. Am.	156	135	59%	65%	52
Africa, Far East, Middle East	242	175	36%	36%	14

TABLE 4—REASONS FOR DIFFERENCES IN LABOR EFFECTIVENESS IN SELECTED COUNTRIES

Reasons Code (see Table 2)	Percentage Distribution of Frequencies in								
	Germany	Benelux	Italy & France	U. K.	Other Europe	Japan, Aust., So. Africa, New Zealand	Mexico	Other L. A.	Africa, Far and Middle East
1	0%	6%	3%	10%	8%	12%	22%	23%	28%
2	6	21	27	14	19	21	31	23	22
3	25	11	10	5	6	2	0	0	3
4	15	17	14	20	22	14	24	15	25
5	6	15	8	6	0	5	7	10	3
6	3	2	0	7	6	7	2	1	0
7	0	0	0	0	3	2	3	9	14
8	9	6	13	10	11	5	0	4	0
9	6	6	5	8	0	7	8	5	0
10	0	2	8	1	3	9	0	3	0
11	0	0	0	0	0	0	0	4	3
12	18	8	0	8	8	5	0	0	0
13	12	6	13	11	14	11	3	3	2
Total	100%	100%	100%	100%	100%	100%	100%	100%	100%
Number of Observations	33	48	40	73	36	43	41	118	36

studies. Young's dissertation [8] revealed no difference in labor effectiveness between the United States and Canada. He attributes the difference in productivity between the two countries to the larger volume and longer production runs in the United States. Our results show a 100 per cent median for the Canada/U.S. labor input ratio. The study, *Costs and Competition* [9], included questions on comparative labor effectiveness, but without any attempt at quantification. The percentage frequencies arrived at (p. 54) are shown below:

	When similar equipment is used, and similar product is manufactured, the labor time required per unit of output as as compared to the U.S. is:			
	Less	Same	More	Total
Canada	12%	78%	10%	100%
United Kingdom	—	71%	29%	100%
Common Market	2%	55%	42%	100%
Latin America	5%	52%	43%	100%
Australia	—	67%	33%	100%
All Other	—	33%	67%	100%
TOTAL	5%	61%	34%	100%

These figures point to the superiority of U.S. labor in all cases except Canada, but do not reveal by how much.

On the other hand, the study did attempt to quantify the over-all productivity ratios. It suggests (pp. 120-22) that the European/U.S. ratio lies between 50 and 80 per cent, implying a U.S./European ratio of 125-200 per cent. If we select a midpoint of 160 per cent, our results indicate that a third of the differential is due to the labor-effectiveness factor, while two-thirds are the result of differences in the physical facets of production. The same study suggests that the U.S./Latin American productivity ratio is between 150 and 300 per cent. Again, selecting a midpoint of 225 per cent, our results indicate that over a third of the differential is due to labor effectiveness. Finally, for Australia, the study suggests a productivity ratio of 125-200 per cent. Our median labor-effectiveness ratio (not shown separately in Table 3) of somewhat over 115 accounts for about a quarter of the productivity differential.

V. *Concluding Comments*

Although the "Leontief paradox" cannot be explained by reference to the relative superiority of U.S. labor, there are recent indications that his results might have been due to the peculiar composition of U.S. trade in 1947. First, W. Salant and B. Vaccara's study [7, pp. 47, 113] of 72 manufacturing industries shows that the median employment required to produce domestically $1 million of competitive imports in 1953-54 was 114 workers, while the median for $1 million of exports was 107 workers. But these figures relate only to the selected industries and do not cover all of U.S. foreign trade. More comprehensive as well as more recent coverage is offered in two pamphlets

published by the Bureau of Labor Statistics [10]. They cover direct and indirect labor requirements, as well as employment attributable to the replacement of plant and equipment consumed, and yield the following figures for 1960:

	Exports	"Competitive" Imports
Total Value of which:	$22,055.3 m	$6,808.6 m
Farm	$ 3,247.6 m	$ 241.5 m
No-farm	$18,807.7 m	$6,567.1 m
Required employment	3,081,700 workers	1,072,900 workers
Employment requirement for $1 m. of trade (man-year)	139.726	157.620

Although no data are available for capital requirements, making it impossible to establish relative factor ratios, the above figures contrast sharply with those obtained by Leontief. They suggest that Leontief's astonishing finding might have been the result of an unusual year.

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Appendix

Foreign Operations Compared to U.S.

(If you operate more than one foreign plant, please fill out a separate questionnaire for each plant.)

(A) Where similar equipment and organization are used:

(VI) The labor time required per unit of output tends to be:

_______________ Comparable in the two countries.

_______________ Greater in the foreign country than in the U.S.

_______________ Approximately by how much?

_______________ Greater in the U.S. than in the foreign country?

_______________ Approximately by how much?

Products or product lines compared _______________

____

Foreign country _______________

(VII) What characteristics of the labor force are responsible for the difference?

(B) Do you believe that this comparison applies only to the comparable productive processes in which your firm is engaged, or to the labor force in general?

(I) If restricted to your operations, what are the reasons for the difference?

(II) How would you rate the general labor force in the foreign country compared to that of the U.S.?

(III) Which is more effective, by how much, and for what reasons?

(C) *Additional comments:* (Please use back of page if necessary.)

Uncertainty and the Welfare Economics of Medical Care: Comment

In his "Uncertainty and the Welfare Economics of Medical Care" [3], Kenneth J. Arrow covered a lot of controversial ground. We take issue here with those parts of his paper that relate to health insurance.[1] Our concern is with uncertainty as measured by the degree of predictability of expenditures for medical care and not with uncertainty that may be associated with the effects of treatment. Like Arrow's our analysis and conclusions focus on medical care, but are generally applicable to all commodities involving such uncertainty.

[1] For a treatment of Arrow's contention [3, pp. 948-58] that medical care has characteristics which distinguish it from the usual commodity of economic textbooks, see [9].

I

Arrow's argument is intricate and not easy to summarize but we believe that the essence of his views can be represented as follows:

> . . . for present purposes, it will be sufficient to identify nonmarketability [of risk-bearing] with the observed absence of markets.
> . . . the variety of possible risks in the world is really staggering.
> . . . a great many risks are not covered, and indeed the markets for the service of risk coverage are poorly developed or nonexistent.
> The welfare case for insurance policies of all kinds is overwhelming. It follows that the government should undertake insurance in those cases where this market, for whatever reason, has failed to emerge [3, pp. 945 and 961].

The theoretical basis needs only brief indication. Assume "costless" insurance, that is, insurance premiums equal to an actuarially fair value, with no costs incurred by buyer or seller in obtaining (supplying) insurance or in settling claims.[2] Assume further that individuals are risk averters. Then, as Arrow shows, Paretian optimality requires insurance policies against all risks, no matter how small the potential loss (for losses > 0) nor how small or large the probability of occurrence (for $0 < p < 1$). An observed absence of insurance policies would be a necessary and sufficient condition of market failure. Welfare would be improved if government undertook insurance, assuming zero governmental costs.

That much, ideally, is clear. But in practice insurance is not costless: sellers incur administrative, selling, and other expenses; buyers incur costs of time and trouble and expense for advice. Arrow makes only passing mention of buyer's costs, and seller's costs play no more than a minor role in his argument [3, pp. 946, 960, 963]. Our main purpose is to demonstrate that, when buyer's and seller's costs are taken systematically into account, *absence* of insurance policies for certain risks may be a requirement of optimality. Thus, an observed absence of policies in the presence of risk cannot be taken as a sufficient condition of market failure and, by itself, provides no guide for public policy. We then test some empirical implications of our model and, finally, take explicit account of *time* (ignored by Arrow) for new types of health insurance to develop and for markets to grow.

II

Absence of insurance policies could, of course, be explained in terms of increasing marginal utility of income,[3] but an individual with increasing marginal utility of income everywhere would not insure any losses. There are undoubtedly individuals who by choice purchase no insurance for any medical-care costs, but our concern here is with individuals who insure costs of some

[2] In common usage, actuarial cost is referred to as "fair" insurance, and any premium greater than actuarial cost as "unfair." From an economic standpoint, actuarially "fair" insurance is more appropriately termed "costless," with any premium in excess of actuarial cost regarded as the price paid for the service of insurance.

[3] The proof of this assertion is demonstrated in [7, pp. 279-304].

items of medical care but not others and, in such circumstances, with the characteristics of insured and uninsured costs.

Suppose a total utility function with a segment convex from above (diminishing marginal utility of income) followed by a segment concave from above (increasing marginal utility of income). Then, if the individual's current income places him in the concave segment of the utility function, he would be unwilling to insure some types of medical expense, but might be willing to insure other types. In this case, small losses would not be insured, but large losses might be [7, n. 31].

However, absence of insurance policies covering certain costs of medical care can be explained in terms of diminishing marginal utility of income everywhere. Assuming no buyer's costs, a risk-averting individual will always be willing to pay the net premium (actuarial cost) for insurance against any loss. The price he is willing to pay for the *service* of insurance, i.e., the amount in excess of net premium, depends upon the magnitude of the potential loss (X) and the probability (p) of the occurrence of the event as a consequence of which the loss is incurred. For any particular X, the maximum price he will pay for insurance service (V) is equal to the difference between the actuarial income Y^* and that certain income (Y') having utility equal to the expected utility (U^*) of Y^*. In terms of Figure 1,

$$V = Y^* - Y'$$

where

$$Y^* = p(Y_1) + (1 - p)Y_2,$$

$$Y_1 = Y_2 - X,$$

and Y_2 is the individual's income if loss X does not occur. If p can take any value from zero to one, as p is increased from zero, V rises from zero to a maximum at some $0 < p < 1$, then declines again, falling to zero as p reaches its upper limit of one. If we assume for simplicity that the cost of insurance service (C) varies proportionally with the net premium[4] (where net premium is pX and the proportion is k), the smaller the probability of a particular X, the greater will be the likelihood that V exceeds C, and the more likely will it be that insurance policies covering the risk are observed. (See Figure 2, where, for analytical purposes, V has been related to pX rather than to U^*.[5])

For a given utility function and initial income level (Y_2), a family of V curves for X's of different magnitudes can be generated by the technique used in Figure 1. Each such curve will have positive values for $0 < pX < X$, and a value of zero where pX is equal to zero or X. The maximum height of each curve will vary directly with X. Two such curves are shown in Figure 3, from which it is apparent that, among risks involving different probabilities of events as a consequence of which losses of different magnitudes will be in-

[4] This assumption is also made by Arrow [3, p. 960]. In fact, proportionality is not crucial to the argument. For our results to hold generally, we need only assume that the cost function has a nonnegative second derivative (which would seem to be a reasonable expectation). We are indebted to G. S. Becker for pointing this out to us.

[5] U^* varies from $U(Y_2)$ to $U(Y_1)$ as pX varies from zero to X.

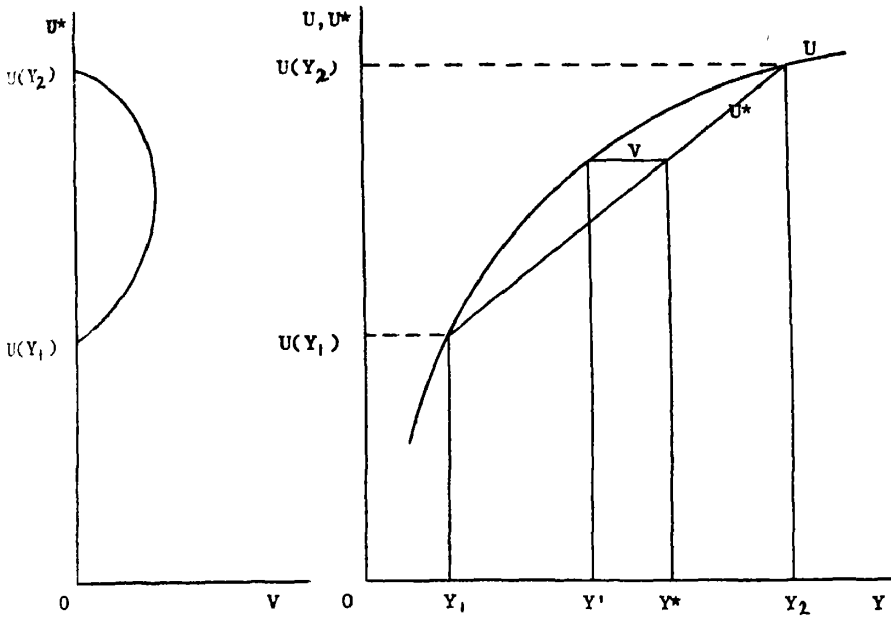



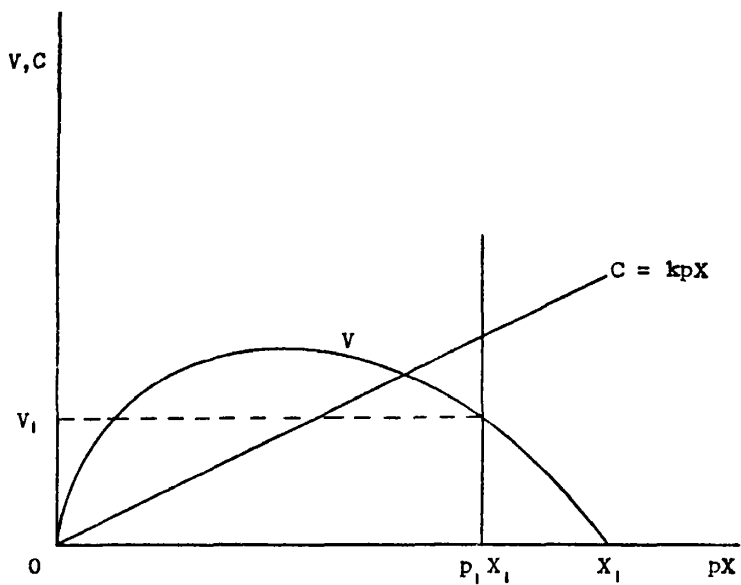
FIGURE 1

curred, the larger the X and the smaller the p, the more likely will it be that V exceeds C. In particular, Figure 3 shows that if one of two risks with the same actuarial value (OA) and, by assumption, the same cost of insurance service, is "worth" insuring but the other is not, the risk "worth" insuring will be the larger risk with the smaller probability.

Similarly, if C is proportional to p alone, larger losses and losses with smaller probabilities are more likely to be worth insuring than smaller losses and losses with larger probabilities. This situation is illustrated in Figure 4, where it is assumed that $C = k'p$.

Thus, if we knew no more than that insurance service was not costless, but had a cost which was proportional to either actuarial cost or probability, we would predict that some losses might not be insured and, if so, that these losses would be smaller and/or have greater probabilities than the losses insured.

But suppose insurance is costless on the supply side (i.e., can be purchased at actuarial cost) and is available for all losses. The existence of buyer's costs may still result in a rational rejection of insurance policies for some medical expenses. Specifically, the transactions cost to the individual of completing and filing applications and claims forms, paying premiums, keeping records, etc., as well as possible costs of obtaining information, may be of sufficient magnitude to make insurance policies against certain losses not worthwhile. In this case, we would also expect the insured losses to be larger and/or to involve smaller probabilities than the uninsured losses.

We have followed conventional usage in referring to these losses as "un-

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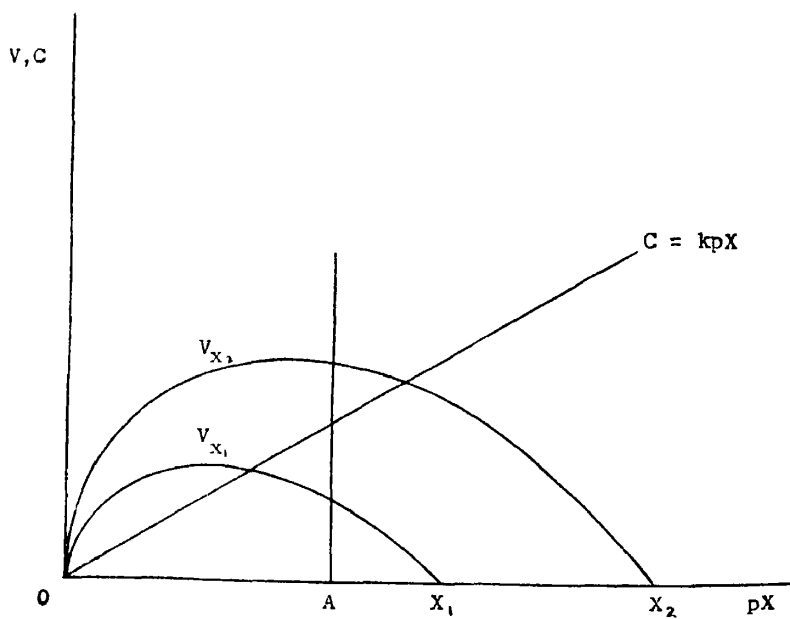
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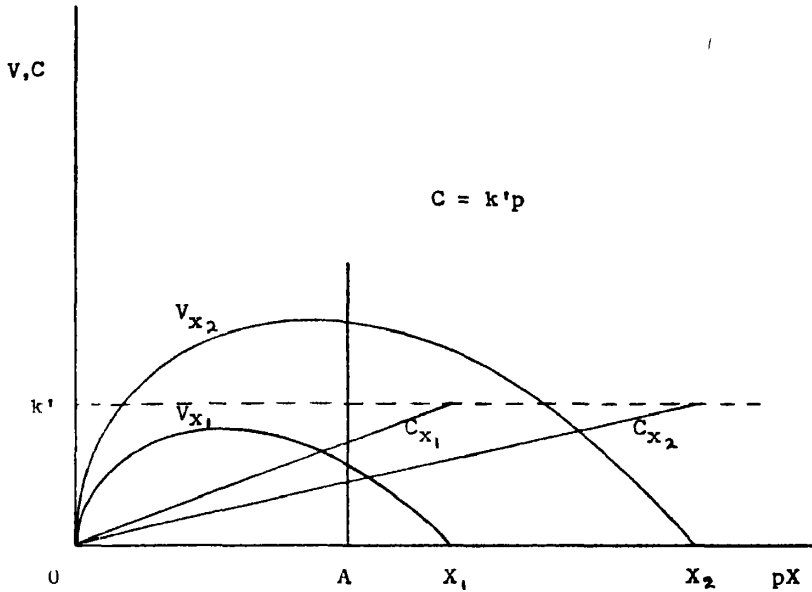
FIGURE 4

insured," but they would be more properly regarded as "self-insured,"[6] with the individual paying an implicit price equal to the value to him of insurance service for a particular loss. In Figure 2, this price is V_1. Other things equal, this would become an explicit price only if the costs of insurance service were to decline to V_1 or lower, i.e., $k \leq V_1/p_1X_1$. To compel an individual to purchase an insurance *policy* if its cost exceeds V_1 would reduce welfare: relevant costs exceed relevant utilities.

Our point of view can be put another way. "Households" are typically regarded as consumption units and linked with "firms" only through factor markets. But they are also important production units in their own right, intimately linked with firms through product markets, and investing resources that generate utility streams. They produce a wide and impressive array of services that compete with services produced in markets, conventionally regarded: hotel, restaurant, laundry, domestic service, barber and beauty salon, decorating, mechanic, schooling, medical care, gardening, banking, insurance, and so on. Households produce these services up to that point where there are mutual gains from trade at the margin with market or extra-household institutions. The boundaries between household and market activity can shift sharply. For example, over the past few decades domestic service has shifted from markets to households and laundry services have shifted the other way. Looked at generally, what determines the division of labor between households and markets is the normal adjustment of costs and benefits at the margin. Insurance against risk is no exception: where the purchase of insurance in

[6] This term is ambiguous but is meant here to imply no more than the deliberate assumption of one's own risks. See [12, p. 47, n. 4].

markets involves costs in excess of actuarial value, we should expect a portion of total insurance services to be produced by households themselves. There are compelling reasons—like the measuring rod of money—for restricting economic analysis and measurement to market activities. But an activity does not cease to be "economic" merely because it takes place in households as well as, or rather than, in markets.[7] And there can be no doubt that household production is quantitatively significant [5] [14, Appendix II].

The empirical implications of our analysis can be summarized as follows. If we classify losses and probabilities arbitrarily as small, intermediate, and large, we would predict that insurance involving any costs in excess of actuarial value is most likely to be observed for large losses with small probabilities and least likely to be observed for small losses with large probabilities. In intermediate ranges, insurance is more likely to be observed for larger losses than for smaller, and for losses with smaller probabilities than for losses with larger probabilities.

III

From the cost-price relationships developed in Section II, it is clear that the greater the X, given p, and the smaller the p, given X, the more likely will it be that $(V\text{-}C)$ is nonnegative. If this difference is negative, the loss will not be insured, and for nonnegative differences, the demand for insurance will be greater, the greater the difference.

If we could measure this difference, we could determine which losses were not worth insuring, and the relative strength of insurance demand among losses worth insuring. We cannot do this directly, but we suggest here a method of estimating relative magnitudes of this difference for various losses. The difference (D) between a particular X and its actuarial value, which is equal to $(1\text{-}p)X$, varies inversely with p and directly with X. We have seen that the likelihood of a nonnegative difference between V and C varies in the same way. Thus, the larger the D, the more likely will it be that $(V\text{-}C)$ is nonnegative. Also, if D is substantially greater for one loss than for another it is likely that $(V\text{-}C)$ is greater for the loss with the larger D.[8]

The ideal dichotomy would be between "routine" and "extraordinary" medical-care expense. Routine expense is highly predictable and/or small in relation to individual or family resources and can therefore be budgeted in much the same way as grocery bills. Extraordinary expense is both highly unpredictable and significantly large in relation to resources and is thus well suited to insurance. In terms of our analysis, routine expenses are those for which $C > V$, where C includes both buyer's and sellers's costs. The available data do not enable this distinction to be made but estimates of D for selected *items* of medical-care expense are shown in Table 1. If some types of medical-care expense are not worth insuring, we should expect the values of D to be lower

[7] For a rare and excellent treatment, see [13]. A household resembles a firm also, in that, within it, market transactions are eliminated. The fundamental reason is the same, namely, that the cost of using the price mechanism outweighs the benefits. See [6].

[8] This will be true unless the probability of this loss is very large or very small, relative to the probability of the loss for which the value of D is smaller.

for these expenses than for medical expenses insured. We would also expect that, in general, insurance covering various types of medical expense would be more prevalent the greater the value of D.

In Table 1, column 3 gives the mean expenditure per type of service for all individuals in the sample, and each mean is the actuarial value of insurance for the types of service shown in column 2. In column 4 are the mean expenditures of those individuals incurring expense or the expected total loss. Column 5 is the difference between the two and gives the expected loss in excess of actuarial value, which we are calling D.

Table 1—Estimation of "D" for Selected Types of Medical-Care Expenditures

(1)	(2)	(3)	(4)	(5)
		Mean Gross Expenditures, 1957–58		
Rank	Type of Service	per individual	per individual with expenditures	D
(1)	Hospital Care	19	176	157
	Physician Charges:			
(2)	Surgery	6	127	121
(3)	Other in Hospital	3	65	62
(4)	Dental Care	14	38	24
(5)	Prescribed Medicines	13	33	20
	Physician Charges:			
(6)	House Call	2	19	17
(7)	Office Visit	16	31	15
(8)	Nonprescribed Medicines	6	11	5

Source: Derived from data in [1].
Note: Obstetrical care has been excluded from these data and throughout the paper.

The types of service shown in Table 1 have been ranked in order of decreasing values of D, and we should expect the prevalence of insurance for the losses associated with the various types of service to vary in the same way. The lower the position of a type of service in the table, the greater the likelihood that the costs of insurance service exceed the value of the service and the more likely then that we will observe absence of insurance policies. With complete equality in income distribution and identical utility functions, there would be either complete insurance or zero insurance. With dispersion in income distribution and/or variation in utility functions, we would expect to observe individuals purchasing different amounts (including zero) of insurance. But the prevalence of insurance should still be generally consistent with the rank order of D.

The data in Table 2 are consistent with our expectations, considering that most of the benefits for "other" physicians' services are for in-hospital visits other than for surgery, and that most of the benefits for medicines are for prescriptions.

Of the services in Table 1, dental care, medicines, and physicians' services

other than in-hospital would appear, in the aggregate, to be marginal or not worth insuring. This should not be surprising in view of the values of D for these services in comparison to those at the top of the list. For example, it will be seen that surgery and nonprescribed medicines have identical actuarial values, i.e., \$6, but, due to widely differing probabilities of occurrence, a \$6 premium would buy a mean \$121 of protection for surgery and only \$5 for nonprescribed medicines. We do not know the demand price for the service of insurance but, intuitively, if insurance costs are added to the net premium, we would expect surgical expenses to be heavily insured and nonprescribed medicines hardly at all.

TABLE 2—PERCENTAGE OF PRIVATE EXPENDITURES FOR MEDICAL CARE[a] COVERED BY INSURANCE BENEFITS, BY TYPE OF SERVICE, 1952–53 AND 1957–58

Type of Service	Per Cent of Expenditures Covered	
	1952–53	1957–58
Hospital Care	50	58
Physician Charges:		
Surgery	38	48
Other	4	7
Prescriptions and Other Medicines	0[b]	1
Other Medical Goods and Service	1	1
Dentists	0[b]	0[b]

[a] Excluding costs of insurance service.
[b] Less than ½ of 1 per cent.
Source: [1, Table 69, p. 73].

This conclusion is reinforced by the fact that broad groupings of medical expense are often jointly incurred. Thus hospitalization is associated with surgery and in-hospital physician care, and out-of-hospital physician visits are associated with prescriptions. We can take account of this in the following way. Expenditures per individual for all individuals can be aggregated for various types of service, since their common denominator is the total number of individuals in the sample. However, expenditures per individual with expenditures for various types of service cannot be so aggregated because their denominators differ. The simplest way of approximating the expenditure per individual having expenditures for a group of services normally occurring in combination is to divide the aggregated expenditure per individual for all individuals for the combined group of services by the proportion of individuals having expenditures for the service with which the other services in the group are usually combined.

Thus, in Table 1, the combined expenditures for hospitalization (items 1 through 3) in column 3 are \$28, which, divided by the proportion of individuals having expenditures for hospital care (\$19/\$176 = .108), yield \$259 as the mean gross expenditure per individual with expenditures. Here $D = \$259 - \$28 = \$231$.

Using the same procedure, mean gross expenditure per individual with ex-

TABLE 3

Type of Service	Mean Gross Expenditure per Individual		
	All Individuals	Individuals with Expenditures	D
In-hospital Care Hospital care, in-patient In-hospital surgery Physician visits in hospital	$27	$360	$333
Physician's Care out of Hospital Physician visits, home and office Hospital outpatient visits Nonhospitalized surgery Prescriptions	32	57	25
Dental Care	14	38	24
Nonprescribed Medicines	6	11	5

penditures for out-of-hospital care by physicians (items 5, 6, and 7 in Table 1) is found to be $55, and $D = \$55 - 31 = \$24$. Thus although the actuarial value of insurance for in-hospital expense ($28) is nearly the same as that for out-of-hospital expense ($31), the likelihood that (V-C) is nonnegative is substantially greater for in-hospital than for out-of-hospital expense (refer to Figure 3).

Data upon which the results published in [1] were based make possible a more accurate allocation of medical-care costs to the in-hospital and out-of-hospital categories than can be accomplished with the published results.[9] When costs of nonhospitalized surgery (typically minor surgery performed in physicians' offices) and hospital outpatient treatment are included in the out-of-hospital category, even more striking results are obtained, as shown in Table 3.

It now appears that in-hospital costs constitute one class of losses and out-of-hospital costs another and much lower class of losses. The crucial difference between them appears to be the degree of predictability, which is far higher for the latter than for the former.[10] Yet we know that extraordinary medical expense is not necessarily associated with hospitalization and that at least some hospital admissions involve no more than routine expense.[11] However, the distribution of in-hospital costs is heavily weighted with extraordinary expense and that of out-of-hospital costs with routine expense. Consequently, the categories of out-of-hospital and in-hospital costs are proxies for routine and extraordinary expense.

[9] The data here referred to are in the files of the Health Information Foundation.

[10] Arrow adduces some empirical evidence to make the same point but draws no conclusions from it [3, p. 963].

[11] For example, the very purpose of major medical insurance is to meet extraordinary expense regardless of whether medical care involves hospitalization.

The most recent data show that voluntary health insurance meets a substantial part of the costs that our analysis indicates people would want most to insure. In 1962, insurance benefits met 72 per cent of all private consumer expenditures for hospital care in the United States and perhaps as much as two-thirds of expenditures for physicians' services in-hospital.[12] By contrast, coverage for all out-of-hospital expenditures is low—perhaps 5 per cent of the cost of physicians' services and only 2 per cent of all other items are met by insurance benefits.[13] If we could eliminate routine expenditures from the totals, the percentage coverage of in-hospital costs would increase somewhat while that of out-of-hospital costs would rise markedly. Thus, our two broad groupings, which lump together routine and extraordinary expense, result in an underestimate of the extent to which voluntary insurance meets medical expenses that are worthwhile insuring.

The observed discrepancy in the extent of over-all coverage for these two broad groups of service, particularly the low value for out-of-hospital service, is frequently used as evidence of the inadequacy of insurance, whereas, given the existence of buyer's and seller's costs, the difference in the relative importance of extraordinary expense in the two groups should lead us to expect a substantial difference in the observed extent of over-all coverage. Further, the discrepancy is misused by taking aggregate benefits for the two groups as a percentage of aggregate expenditures for medical care—a procedure that implies that each type of service is of equal insurance value to the consumer. At best this is meaningless and can be seriously misleading. Yet this weighted mean is used by Arrow as an indication of the present shortcomings of the voluntary health insurance mechanism [3, p. 964].[14]

Our conclusions would be further strengthened by four refinements:

1. We have treated as X the mean gross expenditure per individual with expenditures for each type of service. In fact, this mean is the expected value of X if the event at risk occurs, and there exists a dispersion of possible X values around this mean. Intuitively, we may expect the dispersion of the X's around their means to vary in general directly with the magnitudes of the means themselves. For example, the variance of expenditures for hospital care or surgery is surely greater than the variance of expenditures for nonprescribed medicines or office visits to physicians. Thus D is the expected value of the distribution of $(1\text{-}p)X$ for each type of service and transformation of the various D's into upper-limit values, based upon probable magnitudes of the variances for the various types of services, would tend to *increase* the

[12] Hospital-care coverage from [18, Table 11, p. 11]. Estimates of insurance benefits for in-hospital physician services are not published separately. Our figure of two-thirds coverage has been inferred from [2] [18] [16].

[13] Sources as for footnote 12.

[14] Since total out-of-hospital costs are roughly double total in-hospital costs, this weighted mean coverage is equal to $\frac{1}{3}(a) + \frac{2}{3}(b)$, where (a) and (b) are respectively the percentages of in-hospital and out-of-hospital expenses covered by insurance. These weights would be meaningful only in the extreme case of identical predictability of both groups of expense.

likelihood that (V-C) is nonnegative, more for services with higher D values in Table 1 than for services with lower D values.

2. We have assumed implicitly that the whole population is homogeneous with respect to risk. Empirically, we know this is not so. What we can now do is standardize for X and allow p to vary as between individuals and groups. The value of D then varies inversely with p. It is intuitively obvious that differentiation with respect to risk reduces the prevalence of insurance for all types of risk and, more important for our purposes, that the proportionate impact is greater, the smaller the D appropriate to a homogeneous population. The smaller this D, the more likely is (V-C) to be negative as p increases. Specifically, in terms of Table 3, the assumption of nonhomogeneity is far more likely to make insurance not worthwhile for out-of-hospital physician visits, etc., with a D of $25 than for in-hospital expenses, with a D of $333.

3. The expenditure figures used are for individuals, whereas the relevant decision-making unit is usually the family. Using the family as a base raises the predictability of all groups of medical expense and particularly out-of-hospital costs.[15] These are seen to be even more heavily weighted with smallish losses having high probabilities, which we would expect households to accommodate mainly by budgeting and the bearing of their own risks.[16] In-hospital expenses remain heavily weighted with large losses having low probabilities and here we would expect households to cover extensively through the purchase of insurance policies.

4. We have assumed implicitly that the only rational response to risk is insurance, either purchased in markets or produced in households. But the reduction of hazards and/or losses is an alternative response [11, pp. 9-11], and cost ratios may be such as to make it a preferred response. Periodic medical examinations, for example, may be regarded as a means of reducing both hazards and losses and thus of cutting insurance costs. Theory would predict that physical checkups would be more prevalent in a voluntary than in a compulsory health insurance system. In fact, checkups are rare under the

[15] See [2, Tables A-17, A-20, A-21, A-28, A-29, A-38, A-53, A-54]. These data show that the probability of incurring hospital-care expense rose from 0.09 for the individual to 0.26 for the family, and the probability of incurring physician expense from 0.44 to 0.75. This last figure includes in-hospital physician services. If these could be separated, the probability of incurring out-of-hospital physician expense would certainly exceed 0.75 and for most families would be close to one.

[16] Given perfect capital markets, families that had not managed to accumulate sufficient funds in a time period would borrow against future income. Imperfect capital markets would encourage prepayment institutions, whose members would borrow from one another and enjoy easier access to capital markets. But here the basic problem is imperfect capital markets, not imperfect response to uncertainty. And postpayment is not unknown [2, Table A-60]. Theory would predict that postpayment would be more prevalent for expenses of dependents than primary earners and that, conversely, insurance coverage of primary earners would be greater than that of dependents. In fact, this seems to be true [2, Tables A-62, A-64].

British National Health Service, while in the United States they are the most important single cause of consulting a physician.[17]

IV

It is not our purpose to review the growth of health insurance in detail, but, from Table 4, it is clear that judgments based on observations at a moment in time quickly become obsolete. Thus, in 1948 it was authoritatively predicted that voluntary insurance would leave something like half the total population uncovered [15, p. 87], and the policy prescription derived was

TABLE 4

(A) Health Insurance Coverage, 1948 and 1963

Type of Coverage	No. Covered (Mill.)		Percentage of Population Covered[a]	
	1948	1963	1948	1963
Hospital Expense	61.0	145.0	42	78
Surgical Expense	34.1	135.0	23	72
Regular Medical Expense	12.9	101.0	9	54
Major Medical Expense	—	41.5	—	22

(B) Health Insurance Benefits, 1948 and 1962

Type of Service	$ Billions		As Percentage of Private Consumer Expenditures For Type of Service Shown	
	1948	1962	1948	1962
Hospital Care	0.46	4.40	26.9	72.1
Phsyicians' Services	0.15	2.11	6.1	36.2
Total	0.61	6.51		

[a] Civilian population as of July 1.
Sources: (A) Health Insurance Institute; (B) *Social Security Bulletin*, December 1963.

that of compulsory health insurance. Few would deny that events have been unkind to that prediction.

Again, on the basis of 1958 data, Arrow shows that "over half" of all hospital expenses were met by insurance [3, p. 964]. Only four years later, in 1962, that proportion had risen to around 70 per cent. Arrow may yet feel that "the insurance mechanism is still very far from achieving the full coverage of which it is capable" [3, p. 964]. Our comments are threefold: (a) however "full coverage" is defined, the insurance mechanism is significantly nearer to achieving it; (b) Arrow's neat demonstration that a combination of deductible and coinsurance is Pareto optimal means that "full coverage"

[17] For references, see [10].

will be observably less than 100 per cent even for highly insurable items, and we would expect this percentage to fall as income rises; and (c) from our analysis, we would not expect families freely to purchase insurance coverage for routine expenses, which bulk large in relation to total expenditures for medical care.[18] Developments since 1948 that have raised the proportion of in-hospital costs covered by insurance from under one-quarter to over two-thirds and for out-of-hospital costs by very much less are consistent with our expectations.

Thirdly, product innovation takes time. Major medical insurance, which has transformed the potenitalities for dealing with catastrophic medical costs, was unknown 15 years ago, but now covers over a fifth of the population and is growing rapidly. An application in 1948 of the Arrow criterion of non-marketability, i.e., observed absence of markets, would have generated the conclusions of market failure and the need for governmental intervention.

Finally, many of the present coverage problems of health insurance reflect the fact that the health insurance industry is relatively new and did not get off the ground until the 'forties. Limited coverage of certain population sub-groups, such as the aged,[19] is a transitory phenomenon. The following perspective seems plausible: given widespread and expanding coverage of the present population, inclusion of infants in family coverage and continued extension of group insurance coverage to retirees, it is conceivable that within a generation almost all Americans will be covered throughout their lives by voluntary health insurance providing benefits meeting a substantial proportion of the cost of extraordinary medical care expense. Whatever role government is to play in this transition, it would at least seem inappropriate to create permanent institutions to deal with what is essentially a temporary problem.

V

When account is taken of costs, predictability, and time, much that is puzzling about voluntary health insurance falls into place. We hope to present a more detailed analysis in a later paper but, at this level of generality, the health insurance market is working as theory would predict. The relevant criticism of the market would seem to be not that it is failing, but that its success is incompatible with the attainment of some other objectives.

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[18] The judgment of an eminent authority is that "small losses constitute, in the aggregate, the bulk of the financial loss of the entire nation occasioned by accident or illness. In other words, the 'bulk of the financial loss' is diffused over millions of families in small amounts, so that the blow falling upon each family is very light" [8, p. 10].

[19] Nevertheless, at the end of 1962, 60 per cent of the aged were protected by some form of health insurance, compared with 26 per cent ten years before [17]. Thus, Arrow's view that "the aged are almost completely uncovered" cannot be taken as a serious appraisal of the evidence [3, p. 964].

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Uncertainty and the Welfare Economics of Medical Care: Reply (The Implications of Transaction Costs and Adjustment Lags)

The major part of the most interesting comment by Professors Lees and Rice is an essay in descriptive theory, a demonstration, theoretical and empirical, that for a given loading[1] the demand for insurance will rise with the unpredictability of the risk at hazard. Though the concept of unpredictability is more difficult to explicate precisely than might be inferred from their ex-

[1] Defined as the excess of actual premium over the actuarially fair premium, divided by the latter; as Lees and Rice state [2, n. 2], the loading can properly be regarded as the price of insurance.

position, it is clear that their proposition is broadly true, and will be accepted for the purposes of this discussion; as they make clear, it is certainly consistent with, and to some extent implied by, my paper [1].

The issue between us concerns the normative implications of this proposition. As against a presumption on my part that the failure of health insurance to reach a virtually complete coverage is an indication of nonoptimality, they present two significant arguments: (1) because of the presence of sellers' and buyers' costs, optimal allocation would call for less than universal coverage; (2) in any case, the coverage is in fact increasing rapidly, so that the welfare losses are in fact transitory, an inevitable lag in response to product innovations. I will take up these arguments in turn.

I

In stressing the importance of transaction costs, the authors have performed a great service by stressing a difficult and neglected area in welfare (and for that matter in descriptive) economics. It is somewhat surprising, however, that the explicit recognition of the costs of private marketing should be regarded as an argument *against* alternative modes of resource allocation.

What is clearly needed is a deeper analysis of the causes of transaction costs. It is not only their existence but their escapability under alternative institutional arrangements that is crucial to the normative discussion. Lees and Rice's discussion of buyers' costs [2, Sec. I] brings out the issue clearly; the costs are those "of completing and filing applications and claims, paying premiums, keeping records, etc., as well as possible costs of obtaining information." Unless one knew the context, he might be forgiven for supposing this was an argument for the British national health system, in which all these costs disappear.

Why should a household engage in the production of a commodity which is also available from a firm specializing in its production? The household might, of course, be simply more efficient in production; but for risk-averters, this reason is hardly applicable to the present case, since an insurance carrier, which pools many risks, is necessarily more efficient than any individual household bearing only its own risks. The alternative explanation is that there is a gap between the price received by the producer and that paid by the household for the product, with the marginal cost of production to the household lying somewhere in between. It is this gap which constitutes the transaction costs (sum of seller's and buyer's costs), and which must be studied more closely.

The transaction cost may represent a real cost independent of the method of market organization, for example, transportation cost. The washing of the family dishes is likely to be specialized to the household; a professional dishwasher may well be more efficient, but his superior efficiency is outweighed by the cost of transporting either him to the dishes or the dishes to him. Alternatively, transaction costs may inhere in a particular mode of economic organization, and may be avoided by switching to a different system. In the present case, doubtless some part of the administrative costs of a health insurance system are unavoidable under any mode of payment and ownership.

But a system of reimbursement for the costs of specific treatments to specific individuals requires a great deal more record-keeping than the British system of unlimited coverage of the patient and *per caput* payment to physicians.

Quantitatively even more significant are selling costs. In this context, it is noteworthy that Lees and Rice make no reference to the observation [1, p. 963] that expenses constitute more than 50 per cent of total premium income for individual policies, and less than 10 per cent for group policies. Here is a most remarkable direct measure of the variation of transaction costs attributable to changes in the method of payment.

The authors may object to my equating group policies with governmentally operated health insurance. They could properly argue that group policies are in fact the optimum response of the market to the presence of increasing returns in selling insurance. But I think the force of this point is seriously weakened by two considerations. The first simply meets the argument on its own level; since it is clear that the economies of scale to group policies can be at least equaled by a universal system, the presence of any individual policies points to some degree of nonoptimality in the system.

The second leads into deeper problems and perhaps to a clarification of the classical welfare economics case for government intervention. The very existence of group contracts represents a restriction on the freedom of the market; the individual employee (if the group is composed of the employees of a firm) is essentially faced with a monopoly. Even on a larger scale, the Blue Cross-Blue Shield network is by no stretch of the imagination an example of a competitive market in health insurance. On the other hand, it would clearly be incorrect to regard it as a profit-maximizing monopoly. What has happened is that a voluntary association has essentially played the role of a surrogate government. Just as urged by classical welfare economics, the failure of the market has been met by the development of an organization which departs significantly from the competitive system; but what has not usually been recognized is that other organizations than the state may fill the optimality gap. (This point, as applied to the medical profession itself rather than to health insurance, is, after all, the main thesis of my paper.)

These remarks may be clarified by considering an analogous problem. Suppose the provision of roads is left strictly to private enterprise. Consider in particular a system of local roads, each having many intersections with other roads in the system. Since each stretch of road between intersections is a separate service, the road providers might logically think of charging a separate price for each. But there is a transaction cost; the collection of tolls requires a man with a booth, or at least a mechanical device of some sort. To earn normal profits, the price charged to the consumer must exceed that received by the seller. It can happen that the individual consumer will find it cheaper to build his own roads across his property, especially since he also saves the buyer's cost of delay at the toll booths. Just as in Lees and Rice's comments, this is a rational response on the part of individuals, but it does not follow in any way that it is a socially optimal outcome (in the usual Pareto sense). The road providers may seek to economize on toll collection costs by having the booths more widely separated, at the expense of some

inefficiencies in allocation within each area. But this solution is possible only if the road-provision firms each take in a larger territory (this is analogous to the economies of group insurance contracts and the resulting diminution of choice by the individual insured). It may finally be realized that considerable economy can be effected by charging each car according to its mileage (measured, perhaps, by gasoline purchases or tire wear), but the economy can only be achieved by complete financial unification of the road system. The operating institution could be an unregulated private monopoly, but the resulting allocative inefficiency and power concentration may be felt to be intolerable; it could be a regulated monopoly; it could be a voluntary association of road users; or it could be the government. The coercive powers of government permit certain economies in the collection process, which might weigh strongly in the final choice. But in any case the existence of transaction costs totally or partially eliminable by consolidation of firms works against the preservation of the usual competitive market.

II

The implications of Lees and Rice's second argument, that the coverage of health insurance programs is in fact increasing and may therefore be presumed to be approaching an optimum not yet attained, are to some extent contradictory to the first. It is certainly agreed by them that at least in 1958 (if not today) coverage was suboptimal. *A fortiori*, from Lees and Rice's figures, there was a large welfare loss in 1948. It follows that the institution of universal health insurance in 1948 would have led to a considerable increase in welfare over the subsequent decade, as compared with the alternative actually followed. Given some discounting of the future, acceleration of insurance diffusion through government intervention would appear justified on welfare grounds.[2] To paraphrase the authors' last sentence, the relevant criticism of government health insurance would seem to be not that it would not accomplish its purposes, but that its success is incompatible with the attainment of some other objectives.

In any case, policy determination ought not to be replaced by trend extrapolations. The present coverage, more or less satisfactory as it may be, was achieved not as the spontaneous resultant of many individual self-seeking decisions in the classical picture of the competitive market, but by the deliberate large-scale institutional innovations of the Blue Cross and Blue Shield plans, motivated, at least in part, by the desire to head off government insurance. One might indeed expect, on the basis of the Lees-Rice analysis, that the asymptotic coverage will vary among different elements of the population according to the administrative costs of arranging for the insurance; and the group arrangements needed for economical administration under the existing system must surely militate against those elements, such as migratory workers or the aged, less capable of being formed into administratively convenient groups. Clearly, further innovation is desirable in the provision of

[2] T. Marschak [3] has given a theoretical analysis of adjustment lags as computing time and studied the effects on the efficiency comparison of centralized and decentralized systems.

health insurance, and I see no convincing argument that, in the absence of alternatives, it is undesirable or unnecessary for it to take the form of an increased role for the government.

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Fluctuations in Economic Activity: Planned and Free-Market Economies, 1950-60: Comment

The communication by George J. Staller in the June 1964 issue of the *American Economic Review* discussing the effects of planning on economic stability has led me to conclude that there is still much confusion about which economies are planned and which are not. Staller defined planned economies as those "whose means of production are owned predominantly by the state and whose economic activities are directed pursuant to an over-all plan" [2, p. 386]. He found that planned economies have roughly twice as great a mean fluctuation as those in free-market economies and concluded that "the planned economies have some considerable way to go before they can substantiate the claim to superiority with respect to fluctuations (in total output)" [2, p. 392]. This conclusion, it appears to me, depends upon how the countries under discussion are classified—Staller's classification is more political than economic, i.e., Communist and non-Communist rather than planned and free-market.

On the basis of the ownership criterion in his definition, Yugoslavia is a planned economy, but on the basis of the central-planning criterion, it may possibly be classified as a free-market economy, since it has indirect controls and makes widespread use of the market as a means of allocating resources. France, on the other hand, considered by Staller to be a free-market economy, may in some respects be considered a planned economy for it makes extensive use of "indicative" or "active" planning [1, pp. 46-57]. Given such apparent hybrid cases, doubts necessarily arise regarding Staller's system of classification and therefore his conclusions.

Upon the suggestion of Abba Lerner, and recognizing the improbability of developing an objective index number of the degree of planning, I sent a somewhat crude questionnaire to 38 economists whom I considered authorities, requesting that they rank the countries used by Staller. Most of these economists were listed in the 1964 Handbook of the American Economic Associa-

tion as specialists in the area of "Economic Systems; Planning and Reform," and the others, who were not listed in the Handbook, have published extensively in this area. The heading of the questionnaire read: "Defining the term 'central planning' as the allocation of resources by some non-market institution, please rank the countries listed below with a number from 1 to 4." Number 1 signified "no central planning"; number 2, "low degree of central planning"; number 3, "moderate degree of central planning"; and number 4, "high degree of central planning." Fractional values between the four numbers were not used. I received 22 replies.

The results of my inquiry are summarized in Table 1. They can be interpreted as follows: (1) There is general agreement that the seven countries from Poland to the Soviet Union have a "high degree of central planning." (2) Since Yugoslavia scored 3.22—closer to three than to four—it is classified in the "moderate degree of central planning" group. Because of its midway position between Poland and France, I did two sets of calculations, putting it alternately in the "high" and the "moderate" groups. (3) There is general

TABLE 1—DEGREE OF CENTRAL PLANNING FOR VARIOUS COUNTRIES

Country	Mean*
I. *No Central Planning to Low Degree of Central Planning*	
Ireland	1.25
Canada	1.37
United States	1.55
Germany (F. R.)	1.59
Greece	1.67
Portugal	1.75
II. *Low Degree of Central Planning*	
Austria	1.80
Luxembourg	1.82
Turkey	1.88
Denmark	1.89
Belgium	1.90
Iceland	1.93
Italy	2.11
United Kingdom	2.13
III. *Moderate Degree of Central Planning*	
Sweden	2.55
Netherlands	2.57
Norway	2.57
France	2.59
Yugoslavia	3.22
IV. *High Degree of Central Planning*	
Poland	3.86
Rumania	3.95
Hungary	3.95
Czechoslovakia	3.95
Bulgaria	4.00
East Germany	4.00
Soviet Union	4.00

* The sum of all the scores divided by the number of votes.

agreement that Communism does not preclude extensive utilization of the free market (Yugoslavia), and non-Communism does not preclude central planning (countries in the "moderate" group).

The mean fluctuations in output for the four groups are shown in Table 2. These results differ from those of Staller because the classification of data is different. The results depend partly on the classification of Yugoslavia and whether we use output adjusted or unadjusted for what Staller called domestic "material" output. Staller made two comparisons between planned and free-market economies: one with the free-market economies' total output including a service component and another with the free-market output excluding a service component. This he felt was necessary because Communist countries' national income refers to income originating in material (nonservice) production exclusive of depreciation, while national income for the non-Communist countries refers to domestic product at factor cost. The Communist accounts exclude services unless they are rendered to enterprises which produce "material" output. Thus the value of the services of the school teacher would ordinarily be excluded, but if the teacher were an instructor for a firm producing automobiles, the value of the services of the teacher would be included in national income, being reflected in the value of the output of automobiles. The non-Communist countries usually include all marketable services. Since there are no data on output at factor cost (including services) for Communist countries, it was assumed in Table 2 that mean fluctuations for output at factor cost coincided with fluctuations in domestic "material" output for Communist countries.

When Yugoslavia is in Group III, and we make no adjustment for "material" output, the ranking (going from the lowest to the highest fluctuations) is I, II, III, IV. This appears to show that the lower the degree of central planning, the lower the degree of fluctuation. However, with Yugoslavia in

TABLE 2—MEAN FLUCTUATIONS IN OUTPUT FOR VARIOUS GROUPS OF COUNTRIES, 1950–60[a]
(per cent)

Group	Yugoslavia in Group III[b]		Yugoslavia in Group IV[b]	
	Method 1	Method 2	Method 1	Method 2
I. None to Low Degree of Central Planning	3.1 (3.9)	2.8 (3.8)	3.1 (3.9)	2.8 (3.8)
II. Low Degree of Central Planning	3.3 (3.8)	3.0 (3.5)	3.3 (3.8)	3.0 (3.5)
III. Moderate Degree of Central Planning	4.2 (6.4)	3.8 (5.8)	2.5 (3.7)	2.5 (3.2)
IV. High Degree of Central Planning	5.8 (5.8)	4.9 (4.9)	6.5 (6.5)	5.7 (5.7)

[a] I have taken Staller's data on individual countries and classified them according to the results in Table 1.

[b] The number in parentheses refers to output adjusted for domestic "material" output. The number without parentheses refers to output without adjustment for domestic "material" output.

Group III, but adjusting the free-market economies' data for domestic "material" output, the ranking is II, I, IV, and III.

If we include Yugoslavia in Group IV (as Staller does) and do not adjust for "material" output, the ranking is III, I, II, IV. If we include Yugoslavia in Group IV and adjust for domestic "material" output, the ranking is III, II, I, IV.

The above rankings give evidence, though far less conclusive than indicated by Staller, that the "high degree of central planning" economies have experienced more fluctuation than the other economies. Group IV is in last place three out of four times in comparing domestic "material" output fluctuations. However, for the countries in Groups I, II, and III there is no correlation between the degree of central planning and the degree of fluctuations. If the data are plotted on a scatter diagram, the lack of correlation can easily be seen. This is also revealed by the fact that in the four tests comparing domestic "material" output Group I showed the least fluctuation in one test; Group II showed the least fluctuation in one test; and Group III showed the least fluctuation in two tests. Finally, these conclusions are insensitive to changes in the composition of countries in Group I and Group II. For example, if we shifted Austria, Luxembourg, and Turkey into Group I, mean fluctuations would be increased slightly for Group I and reduced slightly for Group II. The rankings would be unchanged.

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Fluctuations in Economic Activity: Planned and Free-Market Economies, 1950-60: Reply

The multidimensional character of such concepts as "free market" and "economic planning" and the classificational difficulties connected with them have been thoroughly discussed in the literature.[1] I divided the economic systems as they existed between 1950 and 1960 on the basis of two reasonably clear and unambiguous criteria. The definition of planned economies as those whose means of production are predominantly owned by the state and whose activities are directed pursuant to an over-all economic plan is an economic one, and not principally political. That it is also clear-cut is fully supported by Professor Myron H. Ross's own compilation: except for Yugoslavia (about which more below), the planned economies group, as I defined it, stands unambiguously apart from the rest.

[1] For a brief summary of lengthy discussion, see [4, pp. 55-76] and [2, pp. 171-226].

Professor Ross prefers a four-way classification based on subjective appraisal of 22 economists responding to a questionnaire whose instructions were insufficiently specific. What is the difference between "no," "low," and "moderate" degree of central planning? What time period does Ross refer to? This was not specified in his questionnaire. The *modus operandi* of each of the two economies Ross chose to discuss, Yugoslavia and France, was significantly different in the 'fifties compared to the fall of 1964, when the questionnaires reached the respondents. In 1955 Yugoslavia was just completing a program of reforms leading to freer markets[2] while France was extending its planning activities.

Professor Ross's handling of statistical results derived from his questionnaires is as puzzling as the questionnaire itself. He does not specify his method of using the scores to divide the countries into groups. Yugoslavia is classified in Group III because "it scored 3.22—closer to three than to four." By the same token, Group I should be composed of only Ireland and Canada, the only two countries whose scores are closer to one than to two (see Table 1). When equal-size intervals are used for classification, Group III ranges from 2.51 to 3.25; four countries cluster at the lower end of the interval (Sweden 2.55, Netherlands 2.57, Norway 2.57, and France 2.59), while Yugoslavia stands alone at the upper end (3.22). Little support can be found in Table 1 for Ross's claim that there is "general agreement" that Yugoslavia should be included in Group III with the other four countries.[3] The Table does show, however, that the scores representing the "degree of central planning" exhibit, except for Yugoslavia, only one important discontinuity: between France (2.59) and Poland (3.86). This points to the advisability of classifying the countries in Ross's Groups I, II, and III in one group, and the countries in Group IV in another group. Such classification is identical with the one I used in my paper.

Yugoslavia deserves further consideration. There is no doubt that the country moved away from the Soviet planning model during the first half of the 'fifties. But when the Yugoslavs themselves wrote about their "free Socialist market," they described a market which was freer than during the First Five Year Plan (1947-51), or freer than the markets in Soviet-bloc economies. During the 1950 to 1960 period, the Yugoslav government maintained a degree of control over the economy never exercised by the governments of the "free market" economies. As Vice-President Kardelj put it:

> Our free market is a conditional term . . . it must be understood that the free formation of prices under our conditions means forming them on the basis of supply and demand within the general proportions of the plan [6, pp. 147-48].

[2] The Tito-Kardelj economic reforms took place during the first half of the 'fifties [1, pp. 190-91] [8, p. 558]. Their "full impact" on growth, and thus variations in output, was felt in the second half of the decade. [7, p. 575].

[3] If Professor Ross specified an earlier time period, at least one more respondent would undoubtedly have put Yugoslavia in the fourth group, pushing its score over 3.25.

A report submitted in 1957 to the Congress of Workers' Councils stated

> . . . freedom for each undertaking on the market in no way means that it can act in isolation or arbitrarily, nor does society waive its right to direct production according to a plan in order to meet such needs as have arisen. Freedom for the undertaking consists in the statutory definition of its rights as regards production, trade, and distribution. . . . But this market is subject to conscientious social direction [5, p. 36].[4]

Thus the Yugoslav economy should be looked upon as either *sui generis* and left out of consideration or classified in the planned group. But no matter how we classify Yugoslavia, Ross's Table 2 does not present "far less conclusive support" of my findings.[5] The fluctuations means of his Group IV, with or without Yugoslavia, turn out in all eight "tests" markedly higher than the means of other groups.[6] The question of stability *within* Groups I to III (my free-market group) present a separate problem which, perhaps, should be investigated. If one accepted Ross's approach, the results of his questionnaire would suggest that "for the countries in Groups I, II, and III, there is no correlation between the degree of central planning and the degree of fluctuation"; I never said there was. Professor Ross's findings do not modify in any way the conclusions of my paper.

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[4] In contrast, the French economic planning (with the high score in Ross's inquiry) was characterized by Professor Jacques H. Drèze, the one reference given by Ross, as ". . . a summary description of the (market) economy projected four years into the future and accompanied by a set of commentaries and recommendations. As such, it may be regarded as a giant marketing study at national level. . . . It is thus *hoped* that the projection, to the extent that it *forecasts and influences* their [many decision-makers'] decisions, brings about enough consistency among decentralized plans and consensus among decentralized objectives to become substantially *self-realizing*" [3, p. 46, author's italics]. See also [9, pp. 263, 269] [10, pp. 337, 341, 343].

[5] Ross attributes to me the position that the planned economies "have experienced *more* fluctuations than the other economies." My conclusion reads ". . . the planned economies . . . were subject to fluctuations in economic activity *equal to or greater* than those experienced by free-market economies. . . ."

[6] The mean fluctuation of Group III in Table 2 with Yugoslavia included (6.4 for Method 1 and 5.8 for Method 2) appears to be higher than of Group IV for adjusted "material" output (5.8 and 4.9, respectively). The explanation lies in the change in composition of Group III. No data on fluctuations of adjusted "material" output are available for Norway and Sweden. Thus Ross's Group III consists of only three countries: France, Netherlands, and Yugoslavia. If we make any reasonable estimate of fluctuations in the adjusted "material" outputs of Norway and Sweden (based on the known fluctuations of unadjusted outputs), the means for Group III drop below the corresponding means in Group IV (they are about 4.8 for Method 1 and 4.4 for Method 2).

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The Optimum Foreign Exchange Market: Comment

In a recent article in this Review,[1] Jerome L. Stein presents a theory of the optimum foreign exchange market to demonstrate "how social welfare criteria can be used to evaluate the desirability of alternative foreign exchange markets." Arguing on the basis of a simple model, he is able to obtain conclusions of far-reaching significance. For an economy with idle resources, he concludes, a free exchange market is more efficient than a stabilized exchange market if the balance of payments tends towards deficits in periods of output contraction and surpluses in periods of output expansion; on the other hand, a stabilized exchange market is more efficient than a free exchange market, if the balance of payments tends towards surpluses in periods of output contraction and deficits in periods of output expansion. For a full-employment export economy with fluctuating productivity, a foreign exchange market stabilized at the "equilibrium rate" is found to be more efficient than a free exchange market. Thus, it appears that the long and arduous controversy over the relative desirability of freely flexible and fixed exchange rates has found a startlingly simple solution: depending upon the "structure" of the economy in question, Stein's theory would permit an unequivocal choice between the two types of exchange-rate systems.

Unfortunately, the promising prospect turns out to be a mere mirage. In this note, I intend to show (1) that the theory is based on a narrow and uninteresting view of the function of the foreign exchange market and con-

[1] Jerome L. Stein, "The Optimum Foreign Exchange Market," *Am. Econ. Rev.*, June 1963, *53*, 384-402.

siders none of the major issues in the current controversy over the relative desirability of freely flexible and fixed exchange rates; (2) that, even for the special problem with which the theory is concerned, the model presented is too restrictive to be of much use as an aid to analysis; and (3) that, given the specific forms of the functional relationships postulated in the model, the conclusions derived by Stein can all be readily obtained by the application of a simple theorem in elementary economics, without ever having to resort to the sophisticated mathematical and graphical tools he employs. In short, Stein's theory of the optimum foreign exchange market is disappointing both in its content and in its derivation.

My comments will follow the dichotomy Stein uses in presenting his theory: a "general model" to set forth his approach to the problem, and a "specific model" to derive his conclusions. Section I below will deal with his general model, Section II his specific model.

I. *Comments on Stein's "General Model"*

Stein takes it for granted that the problem of the optimum foreign exchange market is "to find the time path of absorption which will maximize social utility, subject to a constraint," and characterizes it as "an Irving Fisher problem applied to international finance." Immediately, he lays out the standard tools of optimization theories—a utility function, a set of structural equations, a constraint to maximization—and starts to solve for the necessary conditions for a maximum. From the latter, he derives the characteristic of the "optimum foreign exchange market" as a functional relationship defining the "optimum" responses in the exchange rate to given fluctuations in the nation's real output, which is then used as the criterion for evaluating the relative desirability of fixed and freely flexible exchange rates. While impressed by the rigor and ingenuity with which the conclusions are derived, the reader may be harassed by a nibbling doubt over the soundness of the very basis of the theory: Why should the problem of the optimum foreign exchange market be viewed as "an Irving Fisher problem applied to international finance"? Is it the function of the foreign exchange market to facilitate *intertemporal,* rather than *international,* resource allocation? What about some other considerations which might also be relevant to the evaluation of the relative efficiency of alternative exchange-rate systems? He will look in vain in Stein's article for an answer to these fundamental questions.

In fact, by assuming that the only function of the foreign exchange market is to facilitate intertemporal resource allocation, the theory leaves aside all the major issues in the still-continuing controversy over the relative desirability of fixed and flexible exchange rates. For instance, the theory is mum about objectives of national economic policy other than the "optimum time path of absorption," nor is there any discussion of the constraints on the choice of economic policies imposed by either type of exchange-rate system. The theory neglects the current discussion on the relative effectiveness of economic policies under the two types of exchange-rate system in the pursuit of given policy objectives. By assuming a constant over-all price index, it by-passes the problem of domestic price stability in relation to exchange stability. No considera-

tion is given to the costs of international trade and investment under the two types of exchange-rate systems. And, since it assumes no international lending and borrowing, the crucial question of the behavior of international capital movement under the alternative exchange-rate systems is also ignored.

One must concede, however, that a theory may be perfectly meaningful even though it explores only one particular aspect of a complex problem. To the extent that it succeeds in throwing a new light on that aspect, it enhances our understanding of the problem. Perhaps intertemporal resource allocation is an aspect that has not received sufficient attention from the participants to the controversy. Let us then start from Stein's own ground and examine whether his model is appropriate or adequate for analyzing that problem.

First, it may be observed that, whereas the "time path of absorption" suggests an essentially dynamic problem, the theory that Stein has proposed to tackle it is a purely static one. Despite the explicit recognition of n discrete time periods, it assumes that the variables pertaining to one period are functionally related only to the variables in the same period. The only thread that strings the n time periods together is the balance-of-payments constraint, requiring that the payments deficit (or surplus) in one period be exactly offset by payments surpluses (or deficits) in other periods. Besides that, the insulation between time periods is complete. The model recognizes n time periods, but the analysis is timeless.

There is, of course, nothing wrong with using a static model to analyze an allocation problem involving different periods over time. Irving Fisher's theory of interest is static, yet eminently useful in analyzing the motivations behind the individual demand and supply of capital. However, there is a vital difference between the Fisherian theory of interest and Stein's theory of the optimum foreign exchange market: the viewpoint of analysis has shifted from that of individual decision-makers to that of an analyst on behalf of the society as a whole. For an individual saver, Fisher is on solid ground to assume a given income stream, unaffected by the individual's own decision regarding its disposition or by the borrower's decision to invest in the production process. On the other hand, in a macroeconomic analysis, especially in a situation where there are idle resources, it is no longer warranted to assume that the time shape of the output stream is given, that decisions to raise or lower the level of present absorption or to change its composition between consumption or investment will have no effect on the course of the output stream in future periods. In making these assumptions Stein has deprived the problem of its essential macrodynamic character.

Secondly, the *ceteris paribus* assumption of a given output stream also reflects a piecemeal approach to the formulation of economic policies, unsuitable in a normative theory of decision-making. To assume a given time path of output is to say that all the monetary, fiscal, and commercial policies have somehow been determined outside of the theory, leaving the exchange rate as the only policy instrument for adjusting absorptions to their optimum levels. However, compared with other policy instruments, the exchange rate may not be the most efficient one for adjusting the level of national absorption; moreover, it is likely that a consideration of the interactions between exchange-rate policy

and other policy measures may vitiate a judgment on the relative desirability of alternative exchange markets based on a consideration of the exchange-rate policy alone.

For instance, for a "Keynesian conflict economy," a flexible exchange rate is found to be more efficient than a fixed exchange rate, because the former tends to stabilize the level of absorption, thus more closely fulfilling the necessary condition for an optimum than the latter. However, in a broader framework of analysis, it is quite possible that the same objective may be more efficiently attained by a combination of monetary and fiscal measures which will stabilize the level of absorption without the countervailing "terms-of-trade effect" of exchange-rate fluctuations. Furthermore, it is also likely that the countercyclical fiscal and monetary policy may be more effective under a fixed exchange-rate system than under a flexible exchange-rate system, when, for instance, the effect of exchange-rate fluctuation on domestic price stability is also taken into consideration. In that case, Stein's conclusion, based on a partial theory of economic policy, will have to be reversed.[2]

Thirdly, the balance-of-payments constraint postulated in the theory needs scrutiny. Stein explains that, since all foreign assets are assumed to consist of monetary gold yielding no interest, "balance of payments equilibrium" requires that there is no net accumulation or decumulation of gold over the entire time horizon. But, that is a *non sequitur*. Holding gold may yield no revenue to the nation; it does not follow that gold must be disposed of at any cost. That in his theory a disposal of gold may involve a cost to the nation is explicitly recognized by Stein himself. He shows (p. 390) that, since the exchange rate is the only policy instrument under consideration, a decumulation of previously accumulated gold must be effected through an exchange appreciation, which in the Keynesian case of underemployment will result in a reduction in output, absorption, and hence in total social utility. In this case, the nation would obviously be better off retaining the accumulated gold, and an exact fulfillment of the constraint would mean a failure to maximize national welfare. That is, the assumption of the payments constraint is in an important instance inconsistent with the fundamental postulate of utility maximization.[3]

My demur rests not with the rationale of the payments constraint, but with the restrictiveness of the structural model, which, by considering the exchange

[2] Stein is not unaware of the pitfalls of a truncated approach to rational decision-making. In a section on "the global maximum social utility," he considers the case in which both the real output and the exchange rate are subject to policy control. He shows convincingly that the optimum exchange rate and the optimum output must be simultaneously determined, and that the "global maximum" cannot be attained by arbitrarily fixing the exchange rate and adjusting the real output. By the same reasoning, it may be argued, adjusting the exchange rate to a given output would bring about the "global maximum" only if the given output happened to be the "optimum output."

[3] This may be constrasted to the budget constraint postulated in the Hicksian theory of consumer demand. The consumer in the Hicksian theory will maximize utility only by carrying his spending to the limit of the constraint, i.e., by spending his entire income. For, by assumption, spending involves no cost; it can only augment but never diminish total satisfaction. In contrast, as shown above, getting rid of gold reserve may be costly to the nation's welfare in Stein's theory.

rate as the only policy instrument, makes it difficult to reconcile the payments constraint with the objective of welfare maximization. The payments constraint is a meaningful postulate, calling attention to the often-neglected fact that no nation could indefinitely spend beyond what it can produce with its own resources. It should definitely be retained as an integral part of the theory. On the other hand, in order to preserve the internal consistency of the theory, I would suggest an expansion of the model to bring in policy instruments other than the exchange rate, such that a reduction in gold reserve will be shown as always resulting in an improvement, never a deterioration, in the national welfare. Thus expanded—perhaps also to take into account multiple policy objectives—the analysis will no doubt become more complex, and the conclusions less clear-cut, than what has been presented. However, my hunch is that the task should not be unmanageable. In any case, I cannot see any real alternative to it.

II. *Comments on Stein's "Specific Model"*

As indicated earlier, Stein uses his "general model" (which considers an n-period horizon and assumes no specific form for the functional relationships postulated in the model) to outline his approach to the problem of the optimum foreign exchange market, but relies on a "specific model" (assuming a two-period horizon and specific functional relationships) to derive his conclusions. At the core of the "specific model" is a social-utility function assumed to be of the following form:

$$u = \ln a_1 a_2,$$

which states that the index of social utility (u) is measured by the natural logarithm of the product of the real absorptions in periods one and two (a_1 and a_2).

It is easy to see that the function is characterized by a *unitary elasticity of substitution* between the absorptions in the two periods; that is to say, the society's time preference for absorption in the two periods is assumed to be such that the marginal rate of substitution is always equal to the ratio of the absorptions in the two periods. On the other hand, the structural equations (including the balance-of-payments constraint) imply that the "marginal rate of transformation" between the absorptions in the two periods is unity.[4] Given these characteristics of the social-utility function and the structural equations,

[4] Solution of the system of the structual equations gives $a_1 + a_2 = k(y_1^* + y_2^*)$, where a_1 and a_2 are the real absorptions, y_1^* and y_2^* the real outputs, in the two periods, and k a function of the parameters in the structural equations.

This reduced form of the structural equations also shows that, in attempting to simplify the "ideal price line" and plot it on two-dimensional diagrams, Stein has made an inadmissible assumption that the mean value of y_1^* and y_2^* be zero (p. 392). As the reduced form shows, the assumption implies that the sum of the absorptions in the two periods must also be zero, which clearly contradicts the logarithmic form of the postulated social-utility function. As will become clear in a moment, the assumption also makes it impossible to fulfill the necessary condition for a maximum, except when the absorptions in both periods are equal to zero.

an undergraduate student in economics should be able to conclude immediately that the optimum time path of absorption must be one of equal absorptions in the two periods,[5] and that the optimum foreign exchange market must be such that the exchange rate will automatically adjust to changes in output so as to equate the absorptions in the two periods. That, in essence, is the basis of the criterion used for evaluating the relative desirability of fixed and freely flexible exchange rates.

The rest follows readily. Since, by assumption, no adjustment in the time path of absorption takes place under a fixed exchange-rate system, the relative desirability of the fixed and the flexible exchange-rate systems depends entirely on how exchange fluctuations in the latter system affect the time path of absorption. According to the structural equations postulated in the model, an exchange depreciation increases and an exchange appreciation decreases absorption in a Keynesian economy dominated by the idle-resources effect, whereas the reverse is true in a full-employment economy. Now, the three "types" of economy considered by Stein are distinguished from one another by the way output fluctuations are associated with exchange fluctuations in a flexible exchange-rate system. The characteristic output-exchange rate relationships and the effect of exchange flexibility on the level of absorption may all be summarized in Table 1.

TABLE 1

Types of Economy	Exchange-Rate Fluctuation During Periods of		Effect of Exchange Flexibility on the Level of Absorption
	Output Expansion	Output Contraction	
(1) "Keynesian Conflict Economy"	Appreciation	Depreciation	Stabilization
(2) "Keynesian Compatible Economy"	Depreciation	Appreciation	Destabilization
(3) "Full-Employment Export Economy"	Appreciation	Depreciation	Destabilization

Since, from above, a freely flexible exchange rate is considered preferable to a fixed exchange rate if the effect of exchange flexibility is towards stabilization of the level of absorption, and less preferable if the effect is towards destabilization, the results summarized in this table clearly show that a flexible exchange rate is preferable to a fixed exchange rate in (1) and less preferable in (2) and (3). All the conclusions from Stein's theory are thus obtained without ever having to use the elaborate mathematical and graphical tools employed in his theory.

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[5] This is an exact literary interpretation of the necessary condition for an optimum—equation (3.1), p. 391—derived mathematically in Stein's article.

The Optimum Foreign Exchange Market: Reply

In this *Reply*, I shall show why I think that my article (a) conside major theoretical issues in the current exchange rate controversy an contains multiple policy objectives.

I. *The Current Controversies*

In my article, I assumed that each country selects its own monetar fiscal policy.[1] The question that I considered is whether it is better for a try to (a) stabilize its exchange rate at a level which will balance its ments over the entire period or (b) let the rate float around this equilil level.

On the other hand, the issues that have aroused passionate controvers as follows.[2]

(i) Is a fully autonomous monetary policy compatible with the m nance of a stabilized exchange rate? Are countries prepared to suborc their monetary and fiscal policies to the requirements of external balanc

(ii) Would the resistance to credit inflation be weakened under a sy of flexible exchange rates?

(iii) Would the risks of foreign trade be increased under a system of ble exchange rates?

Cheng questioned the usefulness of my paper because my analysis se remote from these issues.[3] I shall show that I explicitly discussed the the cal substances of (i) and (iii) and neglected (ii) because it is not prin an economic question.

(i) There is no theoretical reason why a country cannot pursue an pendent monetary-fiscal policy with a stabilized exchange market. The retical problem is to find a stabilized exchange rate which will produce ance-of-payments equilibrium over the entire period, given the monetary- policy. In my article, I showed how this rate can be determined. Meas from an arbitrary origin, my equilibrium exchange rate was zero. C missed the qualifying phrase and thought that the dollar price of foreig change was in fact zero. Hence, utility was not maximized. In point of the stabilized equilibrium price of foreign exchange is the positive price cl as the origin. It was only for geometric simplicity that I measured this from an arbitrary origin, and hereby produced a $\bar{p} = 0$. Whenever he a $\bar{p} = 0$ in a diagram or equation, he should read it as p equals a pos number.

Controversy arises over the question: are the monetary authorities a

[1] In IIC I relaxed this assumption and allowed both the exchange rate and the mon fiscal policy to be dependent variables. Cheng seems to agree (see his footnote 2) th approach is the general way to attack the problem.

[2] See F. Machlup, *Plans for Reform of the International Monetary System* (Princ International Finance Section, 1962), pp. 51-65.

[3] Since he is not too specific about which of the major issues in the controversy I lected, I assumed he would agree with Machlup's evaluation of what are the major I issues.

pable of finding this equilibrium rate as the free market is capable of reachin it? Although this is a fundamental question, it is an empirical rather than theoretical one. For this reason it was only briefly mentioned in my pape (pp. 398, 400).

(ii) When the monetary-fiscal policy is taken as an independent variable question (ii) becomes basically a political-psychological one rather than a economic question. Which situation is more likely to deter the Central Banker from "feeding the fires of inflation"—a declining exchange rate or declinin reserves? It is not clear that economic theory is able to answer (ii).

I assumed that the over-all price index is stable from period to period thereby satisfying an objective of price stability. Such an assumption is no crucial to the model. Postulate any price behavioral equation: the price inde could depend upon the level of output, the exchange rate, etc. Given th postulated price function, calculate the new stabilized equilibrium rate of ex change which produces balance-of-payments equilibrium over the entir period. We then proceed exactly as we did in Figures 2 and 3 to determin which exchange market minimizes social inefficiency. My method of analysi is quite general.

(iii) In a free exchange market, a fundamental reason for a fluctuating rat is the existence of a variable basic balance. The private sector as a whole can not escape the uncertainty about the future course of exchange rates. Althoug risks may be shifted from hedgers to speculators, the cost of hedging is als variable. If the market were stabilized at a given exchange rate, the socia uncertainty would not be eliminated. The Central Bank assumes the man risks involved in accumulating and decumulating foreign exchange reserves The private sector, under a stabilized exchange market, still bears the risk that devaluation will occur or that exchange controls will be imposed—if th Central Bank did not find the equilibrium exchange rate. Social uncertaint cannot be eliminated. The economic question is whether it is better for th private sector to be confronted with a variable exchange rate, and thereby b induced to vary the volume of imports and exports, or to be confronted wit a stabilized rate. My analysis was explicitly designed to answer this questior on the assumption that the Central Bank does stabilize at the equilibrium rate

For these reasons, I believe that my analysis is quite general and does con tain the major current economic controversies about the desirability of fre versus floating rates.

II. *International Resource Allocation*

Cheng asks: "Why should the problem of the optimum foreign exchang market be viewed as 'an Irving Fisher problem applied to international fi nance'? Is it the function of the foreign exchange market to facilitate *inter temporal*, rather than *international*, resource allocation? What about som other considerations which might also be relevant to the evaluation of th relative efficiency of alternative exchange-rate systems?"

The answer is that *international* economic efficiency is achieved if there is (1) equilibrium in the foreign exchange market, such that the supply pric and the demand price for foreign exchange are equal, and (2) universa

competitive equilibrium. Conditions (1) and (2) imply[4] that the margina rates of substitution in consumption and the marginal rate of transformatio in production are the same for any pair of commodities, regardless of wher they are produced or consumed. My requirement (A4) that, over the entir period, the foreign exchange market be in equilibrium is condition (1). It i a necessary condition for an optimal allocation of resources. If condition (2 is also met, my optimal foreign exchange market most certainly produces a optimal allocation of resources among countries. The marginal conditions mus apply in the long run, when the scale of operations and factor proportions ca be varied. Hence it is important to let our horizon be sufficiently long to allov all the adjustments—optimal factor proportions, optimal scale—to occu Choose the length of period 1 to period *n* in such a way that all the adjust ments are capable of occurring.

I took the degree of monopoly within countries as given. Hence my opti mum foreign exchange market will not necessarily produce an optimal inter national allocation of resources. But is it the function of the foreign exchang market to produce competitive conditions? One cannot ask the foreign ex change market to do more than it is capable of doing.

Finally, there are two technical points that can be resolved. (a) My analysi can be interpreted in a dynamic as well as in a static way without changin any results. Given the initial stock of capital, the labor force, and the tech nologies, the production possibility curve is determined. If we also take, a exogenously determined, the rate of investment in each period and the rate o growth of the labor force, the production possibility curves in each period ar determined. Now, the analysis is not static. (b) Balance-of-payments equilib rium requires that the accumulated value of surpluses and deficits over th entire period should be zero. Solely to avoid the interest complications in volved in this summation, I phrased this constraint in terms of gold reserve which bear no interest: The country should end the period with the sam value of gold that it started with. Cheng seems to have been troubled by thi innocuous constraint.

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Capital Controls and the U.S. Balance of Payments: Commen

In a recent article in this *Review* [2], Professor D. A. Snider advocate controls over long-term capital outflows as the most appropriate solution t the U.S. balance-of-payments problem. Snider's brief is both well prepare and persuasively presented, and his case is therefore difficult to resist. Neve theless, the paper suffers from a serious defect which, in my view, casts dout upon the usefulness of his conclusions and recommendations.

Snider opens his argument by describing the four criteria he will use i

[4] The proof is straightforward and, in the interest of conserving space, is left to tl reader.

judging the acceptability of alternative payments policies. Three of the criteria —effectiveness, employment, and efficiency—are economic in nature; the fourth—institutional—essentially noneconomic. He then lists the eight alternative payments policies that, in his opinion, "could conceivably be effective in reducing a payments deficit" [2, p. 348]. They are: deflation; devaluation; exchange-rate flexibility; import restrictions; export subsidies; reduced foreign military expenditure; reduced foreign aid; and capital controls. Considering each of the eight alternatives in turn, he concludes finally that "on the basis of the criteria I have proposed," capital controls would be the most "feasible and acceptable means of balance-of-payments adjustment" [2, p. 359].

Snider is evidently well aware of the political implications of the alternative solutions to the balance-of-payments problem. It is to take account of these implications that he includes among his standards for judgment the noneconomic institutional criterion, which refers to the "feasibility of a policy in the context of the contemporary world and the historical position of the United States in that world" [2, p. 348]. Moreover, it is on the basis of this criterion that Snider rejects such familiar proposals as devaluation and exchange-rate flexibility, arguing that these alternative payments policies would require too great a departure from established institutions and practices. Capital controls, on the other hand, would in his view be quite feasible, despite the historic commitment of the United States to the principle of freedom in international economic relations. For, he reasons, even more serious losses of economic freedom than that of the freedom to invest abroad without restriction could ensue if the balance-of-payments problem is not solved. "Therefore, measures that otherwise would be unacceptable may be acceptable, provided they would effectively contribute to its solution" [2, p. 355].

It is important to take note that Snider thus makes acceptability a function of effectiveness: capital controls would satisfy the institutional criterion only because they can be counted on to satisfy the effectiveness criterion. The defect of Snider's paper is that he apparently misunderstands—or forgets—what he himself intended by the latter. The effectiveness criterion, he writes early in the paper, "refers strictly to the balance-of-payments repercussions of a *measure*" [2, p. 347; italics supplied]. Yet later, in analyzing the potential effectiveness of capital controls, he discusses not the effectiveness of such *measures* in reducing capital outflows but rather the effectiveness of such a *reduction* in improving the U.S. balance of payments. With regard to the latter question he concludes that "the balance of payments of the United States could be substantially improved over the course of the next four or five years by reducing the outflow of private capital" [2, p. 353]. No doubt a majority of economists would agree with his conclusion. But that is hardly the whole issue. The former question remains: could controls effectively reduce capital outflows? On this subject Snider is silent; in fact, he declines to discuss it, explicitly omitting from his paper consideration of "the problems—*some of which may be formidable*— attending the implementation of such measures" [2, p. 346; italics supplied]. I submit that it is precisely the formidability of these problems which is here the central issue. For there is adequate reason

for supposing that ordinary controls would not be effective in reducing capital outflows, and that to make them effective would require such an extension of governmental authority as to jeopardize what acceptability there might be for such measures on institutional grounds.

Why would ordinary controls not be effective in reducing capital outflows? Direct controls, it should be remembered, operate by suspending the free market. They do not eliminate an excess demand for foreign exchange; they merely suppress it, by rationing, administrative allocation, or similar discretionary restrictions of foreign exchange transactions. So long as only such controls are relied upon to eliminate a payments deficit, the excess demand for foreign exchange will remain unsatisfied. Therefore, so long as the controls are less than absolute, outlets will be sought for that excess demand, in much the same manner—to evoke the classic simile—as water seeks its own level. Smart operators can be expected very soon to devise and attempt new methods for circumventing the government's authority, and as they do the effectiveness of the existing controls will steadily decline. That is the fatal weakness of Snider's case: the only way to maintain the effectiveness of capital controls would be to expand continually their scope. This is what we might call the Iron Law of Economic Controls: to be effective, controls must reproduce at a rate faster than that at which means are found for avoiding them. But since any multiplication of controls—even controls which were initially quite acceptable—would be bound to generate resistance in this country at a rapidly increasing rate, truly effective capital controls are not apt to be politically feasible. In other words, the two criteria in question could not be satisfied simultaneously: capital controls that might satisfy the institutional criterion probably would not satisfy the effectiveness criterion, while controls that might satisfy the effectiveness criterion almost certainly would not satisfy the institutional criterion.

The Iron Law of Economic Controls is well illustrated by the history of the so-called "interest equalization tax" (IET), first proposed on July 18, 1963, and signed into law on September 2, 1964. The IET is a special, temporary—it is supposed to expire at the end of 1965—excise tax on U.S. purchases from foreigners of (new or outstanding) equities or debt obligations of three or more years maturity originating in any industrial country (Canada excepted). It is legitimate to view the IET as a mild form of discretionary control over the volume of U.S. portfolio investments abroad, specifically in Western Europe and Japan. Moreover, the tax may be viewed as a form of control which, because of its retroactive character, took effect immediately from the day of its proposal. Was the IET effective in reducing total capital outflows? The answer to this question demonstrates the weakness of the case for capital controls.

There is no doubt that the IET effectively reduced U.S. net acquisitions of foreign shares and debt obligations affected by the tax. Particularly hard hit were European and Japanese net new issues in New York—the main intended victim of the proposal—which dropped from a rate above $300 million during the first six months of 1963 to only about $90 million in the second half and to an excess of redemptions over issues in the first half of 1964. Likewise, net

acquisitions of outstanding European and Japanese securities, which amounted to more than $75 million during the first half of 1963, shifted to a considerable repatriation of funds during the next twelve months. But there is grave doubt whether the total outflow of private funds was much reduced as a consequence. Indeed, the evidence suggests that new methods for circumventing the impact of the IET were devised in remarkably short order.

One almost immediate result of the IET proposal was the development in Western Europe of the practice of issuing new securities denominated in U.S. dollars rather than in local currencies. In the second half of 1963 almost $60 million was raised in this fashion by European and Japanese borrowers, and in the first half of 1964 almost $230 million more. Where did the funds come from? Not all of them could have come from European investors, since during this same period there was no noticeable decline in the volume of foreign issues in Western Europe denominated in local currencies—in fact, there was an increase in the first six months of 1964[1]—and there is no reason to suppose that in the summer of 1963 Europeans suddenly overcame their habitual suspicion of foreign long-term securities. Thus at least a portion of the dollar-denominated flotations must have been purchased by Americans. There certainly was an ample economic incentive for doing so, namely, higher yields than those available at home but no additional exchange risk. Furthermore, there certainly were ample channels for doing so without formally breaking the law. For example, we can be virtually certain that some portion was purchased with funds transferred nominally by individual American citizens or corporations to commercial banks in Europe (taking the form of an increase in recorded or unrecorded U.S. short-term capital outflows). Similarly, it is quite possible that another substantial portion of European dollar issues was absorbed by foreign branches or subsidiaries of American corporations using funds borrowed from head offices here (taking the form of an increase in recorded U.S. direct investments abroad). And it is probable that some European purchases were facilitated indirectly by additional placements of U.S.-owned dollars in Euro-dollar deposits.[2]

As serious as these outlets were, they were not so conspicuous as the exemption granted under the IET proposal to long-term bank loans to foreigners. Being natural substitutes for securities issues in New York, such loans rose sharply, from less than $150 million in the first half of 1963 to more than $400 million in the second half and more than $300 million in the first half of 1964. Indeed, the loophole was so conspicuous that before finally passing the IET, the Congress amended it to give the President discretionary standby

[1] Foreign issues in European markets denominated in local currencies (including European-unit-of-account issues) averaged roughly $150 million in each of the four halves of 1962 and 1963. In the first six months of 1964 the volume jumped to almost $225 million.

[2] To be sure, most of the dollars raised in Europe until now were probably owned outright by the European lenders. Some, however do seem to have been borrowed short-term in the Euro-dollar market for the purpose of lending long-term. Other things being equal, this would have exerted some upward pressure on Euro-dollar rates, with the implication of an increased outflow of short-term dollar funds from the United States. Einzig confirms the increasing use of short-term Euro-dollars in connection with European dollar issues. See [1, pp. 445, 446].

authority to impose the tax on such capital outflows as well. In short, even before the IET became law its effectiveness was called into question. This was the Iron Law operating with a vengeance. The sequence of events surely demonstrates that America's limited experiment with quasi controls has not been a success, and that to become successful the program would have to be broadened dramatically. But there is no reason to suppose that such an extension would be politically feasible. Thus Snider's case for capital controls is defective, since such a payments policy could not satisfy the institutional and effectiveness criteria simultaneously.

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Capital Controls and the U.S. Balance of Payments: Reply

In any policy discussion there are always two kinds of issue to be investigated: (a) whether a given policy is theoretically sound—i.e., assuming the policy is fully carried out, whether it can be expected to produce the effects desired, and (b) whether the policy is capable of being effectively carried out within the framework of given institutions and attitudes. A policy may be rejected on either of these grounds—because it is theoretically unsound, or because it is not practically feasible. Professor Cohen apparently accepts the theoretical arguments for capital controls which I have presented, but rejects the policy on the grounds of difficulty of implementation. The latter, he states, is "the central issue."

Since my paper explicitly excluded from its purview the question of specific measures of capital control and their implementation, Professor Cohen's comments are not really directed at the content of my paper. However, there was an implicit assumption in my paper that it is possible through various measures to reduce the outflow of capital, and that such measures need not be so extensive or so drastic as to render them institutionally unacceptable. If Cohen is correct, this assumption is invalid.

I would be among the first to admit the difficulties encountered in effectively controlling the international movement of capital. However, to conclude with Cohen that the problem is so formidable as forthwith to close the case against capital controls requires, in my judgment, a great deal more evidence than he presents. His so-called "Iron Law of Economic Controls" strikes me more as a nostalgic evocation of the world of Smith, Malthus, and Spencer rather than as a serious operating principle in mid-twentieth century.

The only putative evidence offered to support the thesis that capital controls will not work unless continuously expanded is experience with the Interest Equalization Tax. Apart from the point that this measure is hardly a fair sample of conceivable devices to reduce capital outflows, surely enough experience has not yet accumulated to permit a final judgment on its effectiveness, nor are the alleged circumventions cited by Cohen sufficiently documented to warrant the conclusion that it has already failed.

It would be well to remember that capital controls are not a new device, but one widely employed by many countries over extended periods. While the experience of some countries during certain periods illustrates the well-known difficulties of effective controls, the record of other countries (some with a state of economic development and institutional setting not far different from our own) supports the view that controls can be effective without being Draconian. The results to a large extent depend upon the state of the economy and the severity of the disequilibrium in the balance of payments.

The difficulty of effective enforcement of capital controls is greatest when the country involved is experiencing a massive flight of capital because of an economic, financial, or political crisis. "Fear" capital movements are not easily deterred, even when the escape from controls becomes very expensive. In the case of the United States, however, the outflow of capital is not significantly of a panic-flight variety. It is not, therefore, a question of capital seeking to escape under high pressure and at any cost. Even assuming with Cohen that attempts to escape from controls are inevitable, the increased cost (or diminished return) that circumstances inevitably entail could be expected to reduce the volume of capital outflow from the United States.

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BOOK REVIEWS

General Economics; Methodology

The Critical Decade—An Economic Policy for America and the Free World. By HENRY S. REUSS. New York: McGraw-Hill Book Co., 1964. Pp. xii, 227. $5.50.

"What I have to say is stated with some specificity, in order that public opinion may have the opportunity to agree or disagree." Thus does the author, a distinguished Member of Congress, explain in the preface the way he proposes to develop his views on an economic policy for America and the free world. It is a useful book—intelligible to the thoughtful lay audience, and at the same time one to be read with profit by the professional economist. The author has been a member for many years of that summit seminar in economic developments and policy, the Joint Economic Committee of the Congress, and the reader is the beneficiary of his active participation in the Committee's deliberations.

The book is concerned with what have come to be some central concerns of economic policy. First, how can we regain and sustain domestic full employment? Second, how can we reconcile the requirements of domestic economic policy with the problem of external equilibrium? Third, what is the role that this nation should play in the process of economic development of the less advanced countries?

On the latter question the author accepts what might be described as the orthodox contemporary view. Capital resources, essential for the poor nations to develop, must come heavily from outside. Progress to date has been slow because of the small extent of aid matched against the massive extent of needs. The needs of the developing nations will be $2-$6 billion in excess of probable resources forthcoming from the developed nations. "The problem for the advanced nations, then, is how to fill the $2-$6 billion gap between present programs and present needs" (p. 110). Six suggestions are then made, ranging from programs to involve more Americans in the aid effort (in order to broaden public support) to inducing other developed countries to contribute more.

Many readers, while not dissenting with the author's support of economic assistance, will find themselves a bit uncomfortable with the analysis here. Aid has hard political going for more fundamental reasons than lack of public awareness or taxpayers' grumbles. We do not yet have a well-defined theory of economic aid. The evidence increasingly indicates that development is considerably more than a matter of capital, that if these more dynamic factors are present, economic progress may be impressive whatever the aid program, and that our aid programs have on occasion abetted internal tendencies which lower the capabilities for growth.

Mr. Reuss examines the diagnoses of the domestic unemployment problem

emanating from those emphasizing deficiencies of aggregate demand and from those stressing structural imbalances. He concludes that the evidence lends support to those who attribute the abnormally high unemployment since 1958 to a deficiency of aggregate demand. The evidence he cites is persuasive. The blue-collar workers account for less of the total unemployment now than in the 1950's, the gap between actual and potential output (e.g., Mr. Knowles' work) has been unusually persistent, and U.S. unemployment has been high by international standards even when comparable definitions are used. He then recommends the vigorous use of tax reductions, expenditures on selected programs, and easy monetary and credit policies to get the aggregate demand for output back on target.

The author deals at various places in the book with issues central to reconciling the needs of domestic economic policy with our external imbalance, and these discussions are among the more provocative and rewarding parts of the volume. Many of his proposals are on most lists of remedies—exert pressure to keep down interest rates abroad, ask other countries to eliminate capital controls, deter long-term U.S. private capital outflows to developed countries, keep domestic prices stable, boost the domestic growth rate.

It might have been better to have had another recommendation or two come between pressing "other countries to eliminate capital controls" and reducing U.S. "long-term private capital outflow to developed countries" (pp. 83-84). If we do the latter, our efforts on the former, one suspects, would lack something in persuasiveness. Other countries are not apt to respond to our urging about giving up capital controls if we are moving to reduce outflows on a selective basis (which clearly implies more extensive capital controls). The author is on stronger grounds in suggesting that a more vigorous domestic economy might improve our external disequilibrium. Though this has not been the conventional view, it is coming to be accepted that a more innovative and more profitable domestic economy might ultimately improve its penetration in the world export market. (It is the sluggish economies—U.S., U.K., Canadian—that have lost penetration in the last decade.) And a stronger, more profitable domestic economy should certainly improve our balance on capital account.

On trade policy the author says "with some specificity" things that may startle a few readers. He clearly does not find the Common Market to be an enchanting development. He says quite forthrightly that its emergence has been inimical to U.S. interests, it may become even more so, and he finds the disinclination of policy makers to be realistic about this a bit annoying Reuss frowns at the EEC partly because he favors a more multilateralized trading world. For that reason he would favor beefing up the OECD so that it becomes, in effect, an organization for cooperation among all developed nations. There can, of course, be honest differences about whether the EEC represents one step in a step-at-a-time approach toward a world of generally liberalized trade barriers, but Reuss performs a service in articulating his skepticism about whether it is a step in the wrong or the right direction.

In one area Reuss allows an almost emotional approach to depreciate somewhat a generally well-argued problem-centered book. This is in the area of

monetary policy. It comes through rather clearly that the Federal Reserve and central bankers generally are his *bête noire,* and that William McChesney Martin, Jr., is pretty nearly the king of (these) beasts. Federal Reserve policy certainly has its less happy chapters. The continued tight-money policy in 1957 after it was clear that the economy was moving toward a recession, indeed after it was clear that we were in a recession, was not central banking at its finest. And the excessively tight policy in 1959, coming on the heels of the 1957 episode, is also not easily excused. On the other hand, their performance in terms of strategy and timing during the 1953-54 recession made a major contribution to stability, and it is to their credit that they backed away from a tight policy early in 1960 well before the subsequent decline began.

It is precisely because Federal Reserve policy in certain periods deserves and needs discriminating criticism that the author's indiscriminate vendetta lacks force and persuasiveness. On page 79 we read about the Federal Reserve's "doctrinaire policy of purchasing only short-term Treasury bills. . . ." This policy may have been wise or unwise at the time. Federal Reserve officials were very careful to state fully and in a closely argued manner their case (e.g., papers by Riefler and Young and Yaeger). It is incumbent on those who think that the policy was wrong to produce an equally well-argued analysis, and this is not to be found in the book. In 1963 Martin was "not satisfied with a drastic curtailment of free reserves and an increase in the rediscount rate," and "started a campaign to persuade lenders not to be so willing to lend" (p. 164). This is the interpretation of Martin's concern about deterioration of credit quality. Now Martin may have been wrong about whether credit quality was deteriorating, but the Federal Reserve Chairman is certainly not wrong to be concerned about the problem. Moreover, the word "drastic" seems a little strong if it is meant to describe the magnitude of the 1963 shift in monetary policy. The fact is that the money supply, properly defined to include time deposits, increased 7.6 per cent in 1963, and at the annual rate of 8.0 per cent in the first three quarters of 1964. Or again, "the propensity of Chairman Martin to regard tight money as the proper condition to which the rest of the economy must somehow adjust. . . ." These outbursts do less damage to Chairman Martin than to *The Critical Decade.*

The book also does not prove to be very helpful on the problem of trying to regain reasonably full employment without setting off another significant price-cost rise. The price stability of recent years tells us little. We have always assumed that we could keep the price level reasonably stable if we would accept enough unemployment. Price-wage justification procedures are advocated as a way to deal with cost-price pressures as aggregate demand strengthens. The full implications of this proposal are not explored, however, and there is little consideration of criticisms that have been made of such an approach to controlling administered inflation. This is a pity. If our present progress toward full employment runs aground, the shoals are apt to be a renewed tendency for our cost-price level to give way.

This, however, is generally a good book. It is a useful addition to our understanding of the challenges facing economic policy in the coming years. And it is to be hoped that this lucid and articulate exploration of policy will en-

courage some of the author's colleagues, on both sides of the House, to take their pens in hand. Both policy-thinking and policy-making would be the better.

PAUL W. MCCRACKEN

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Industrialization and Democracy—Economic Necessities and Political Possibilities. By KARL DE SCHWEINITZ, JR. New York: Free Press of Glencoe, 1964. Pp. vii, 309. $6.95.

This book is styled by the author "an essay in political economy," and it does indeed lie in that dangerous territory where economics and political science meet. Its central problem is the relation of democratic institutions to industrialization and economic growth. Its central argument is that whereas industrialization and democracy were closely connected in the nineteenth century, the rise of the socialist states in the twentieth century has broken this connection. This thesis is argued in the first four chapters. Then in the second part of the book, entitled "The Evidence," we have a chapter on the British economy, which argues that here circumstances were such that economic development assisted the evolution of democratic institutions. Then follows a chapter on the United States, which maintains that in this case democratic institutions really preceded economic development and assisted it. The next chapter, on Germany, suggests that the link between industrialization and democracy was beginning to break and that actually industrialization in Germany fostered an unstable democracy. A chapter on Russia and totalitarian industrialization argues that here the break is complete between industrialization and democracy. There is a gloomy chapter on the prospects for democracy in the underdeveloped economies, followed by a conclusion and summary.

This is a book with a single quite important idea, but it could easily have been condensed into a single paper. The evidence presented is all from secondary sources. Furthermore, the work is marred in the eyes of this reviewer by a failure to appreciate the real significance of the long-continued technological development through which we are still passing. Thus the author argues that in Britain "the development of the British economy and political community after 1850 lies outside our purview since the industrial revolution drew to a close toward the middle of the century" (p. 126). In point of fact it was not until after 1850 that the real impact of science on technology was felt in the development of the chemical, electrical, and eventually nuclear industries. All we had before 1850 was a little acceleration of the rising folk-technology of the Middle Ages. Similarly, it seems to me, the author fails to appreciate the real significance of political developments. Thus he says "the emergence of democratic political institutions represented a victory of society over the state" (p. 270). Nothing, it seems to me, could be further from the truth. The state organization in most countries increased substantially during the whole period under review. To my mind, also, he underestimates the role of nationalism and the rise of what might be called the "integrative function" of the state.

These considerations throw some doubt on the validity of the central prop-

osition of this book, even though it does represent a courageous attempt to simplify the cross-currents of history. Perhaps, however, the simplification is too great, although the problem is real, and the book points up the need for a more extended and serious study of the problem which is less dependent on armchair philosophy and casual empiricism. The author therefore is at least to be congratulated on opening up an important subject for discussion.

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The U.S. Economy in the 1950's—An Economic History. By HAROLD G. VATTER. New York: W. W. Norton & Co., 1963. Pp. xii, 308. $5.00.

Professor Vatter's book, reviewing the behavior of the U.S. economy during the 1950's, and designed primarily for use in college courses in U.S. economic history and the principles of economics, packs a great deal of solid information in relatively small compass. Recognizing that a meaningful historical account of any period must be theoretically oriented, the author marshalls his statistical and descriptive materials so as to focus on the relative influence of the major spending streams on the short-run economic fluctuations and on the longer-run rate of economic growth.

In selecting this particular decade for study, the author believes that it furnishes the most advantageous analytical base from which to view U.S. economic development, as well as to interpret the behavior of the economy in the 1960's and to deal with the policy questions that are likely to arise in that decade. By the 1950's it was abundantly clear that the United States had truly become the affluent society and that poverty could be eliminated in the foreseeable future; that mass unemployment was to be a thing of the past; that with the chronic cold war and the inauguration of the space age large governmental budgets were to be permanently with us; that inflationary pressures were likely to continue to have the upper hand, that the population spurt and urban explosion of the 1950's would have considerable impact throughout the whole economy in the 1960's; and that the United States would be increasingly sensitive to events in the world economy.

The book is divided into ten chapters. The first two are introductory—Chapter 1 summarizes the major distinguishing economic characteristics of the 1950's, and Chapter 2 describes the institutional setting of the "mixed economy"—an economy in which policy issues get formulated mainly in terms of little more or a little less of government intervention. The next three review the short-run economic fluctuations of the 1950's. These are followed by chapters concerned with developments in the business sector, the capital markets, labor and agriculture, America's role in the world economy, and a final chapter on investment, growth, and public policy.

While here and there one may take issue with the statistical measures employed or with the interpretation of the data, the author has generally made judicious use of his materials.

ASHER ACHINSTEIN

The Library of Congress

Macrodynamic Economics—Growth, Employment and Prices. By HOWARD J. SHERMAN. New York: Appleton-Century-Crofts, 1964. Pp. xvii, 257. $5.00.

In this text it is the author's objective to combine the principles of growth, business cycles, income determination, and monetary and fiscal policy into a "theory of the whole." It is his intention that this text be used in an undergraduate survey course of the above fields. In a short space he hopes to provide a broad, systematically developed, and internally consistent survey of all of these areas of study. Few new substantive contributions to any of the areas of study or to its synthesis are claimed or accomplished.

The author begins his development of "macrodynamic economics" with a description of the institutional characteristics of a monetary economy. He then introduces a static income determination model which he proceeds to "dynamize" by explicitly stipulating capital-output and capital-labor ratios and factor growth rates. He notes, in passing, some of the possible effects of changes in technology and/or changes in factor prices. Having posed an explanation of secular or long-run movements of the economy, the author then considers explanations of short-run cyclical disturbances. In this respect he particularly emphasizes the underconsumption and overinvestment theories of the cycle. A host of possible factors affecting cyclical disturbances are introduced and considered. These factors include the determination of expectations, the acceleration principle, the problem of inventory management, changes in monetary conditions, the timing of innovations, weather conditions, strikes, colonialism, decolonization, and wars. The author attempts to modify these results by considering some of the realities of imperfect competition. In this regard he emphasizes the observed cyclical and long-run patterns of profit and investment rates as related to the size and concentration of industrial corporations. He ends the text with a discussion of the international aspects and policy implications of what preceded.

Contrary to the opinion of the author, I do not think this text represents a useful survey of "macrodynamic economics" for the beginning student who has acquired no previous knowledge of economic analysis. The author discusses many economic concepts—balanced growth, diminishing productivity, cobweb theorems, etc.—without provision of adequate definitions. In many cases the meaning of these concepts might have been enhanced by graphs. The text has a notable dearth of illustrative graphs. In order to facilitate a reading of the text by beginning students, the author has placed all algebraic derivations in 31 pages of appendices. In my opinion these derivations are not of sufficient complexity to warrant their separation from the text.

More serious criticisms of the text, however, center on statements which I think tend to be misleading or inappropriate. For example, the author states (p. 62) that Keynes's main contribution was the demolition of Say's Law, and that most of Keynes's contributions were contained in the work of earlier economists, such as Malthus and Marx. Notwithstanding the kernels of truth contained in these statements, without elaboration they seem to be inappropriate. In another place (p. 8), he states that the perpetual equation of aggregate demand and supply denies the possibility of cycles in general busi-

ness activity. For some unexplained reason aggregate demand and supply cannot simultaneously increase or decrease. It seems to me that these misleading and otherwise inappropriate aspects of the text are the result of the author's effort to explain too much in too little space.

In teaching a course that surveys all the areas touched upon in this book, it is difficult, of course, to find a single text that encompasses all of these areas of study. However, I should think that an instructor might more wisely select an appropriate set of paperbacks rather than this text.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Investment Decisions, Economic Forecasting, and Public Policy. By JOHN R. MEYER AND ROBERT R. GLAUBER. Boston: Graduate School of Business, Harvard University, 1964. Pp. xix, 280. $6.00.

Capital Stock Growth: A Micro-Econometric Approach. By EDWIN KUH. Amsterdam: North-Holland Publishing Co., 1963. Pp. xiv, 341. $9.80.

The publication of these two books is of more than usual interest, since each is an extension of an earlier study by John Meyer and Edwin Kuh.[1] Despite its substantial contribution to the analysis of investment behavior, this earlier work left two major questions unanswered. First, would the "accelerator-residual funds" theory of investment, proposed by Meyer and Kuh to explain their empirical findings with data for 1946-50, survive tests performed with independent data? Second, would the accelerator-residual funds hypothesis, which might provide a good explanation of cross-section variation in investment expenditure, prove useful in the analysis of investment behavior over time? The two books under review, although not strictly confined to these questions, attempt to provide answers to them.

Briefly, the Meyer and Kuh theory assumes that the cost of external funds is substantially greater than that of internal funds. In the long run the capital stock will grow in such a way as to minimize cost of production, and the supply of internal funds may be adjusted to strike a balance between the conflicting objectives of high dividends and low-cost internal finance for investment. In the short run, however, because of the desire for stable dividend payments, the supply of internal funds may be considered a residual that acts as a constraint on investment. During a period of high demand relative to existing capacity, the large number of profitable investment opportunities for expansion may induce firms to pay the cost of external finance. In this case, investment expenditure may be in large part a function of accelerator variables. During a period of excess capacity, investment expenditure is likely to

[1] John R. Meyer and Edwin Kuh, *The Investment Decision: An Empirical Study* (Cambridge, Mass. 1957).

be for the purposes of cost reduction or expansion in anticipation of future increases in demand, timed to take advantage of the availability of low-cost internal funds. In this case, the constraint will be effective, and the supply of residual funds will be the primary determinant of investment.

The book by Meyer and Robert Glauber divides neatly into three parts. The first part is a cross-section study designed to test the residual funds hypothesis with data for 1951-54. Both the sample of firms and the statistical procedures are near duplicates of those used by Meyer and Kuh. Three sets of regressions are estimated: those that have both accelerator and residual funds variables, those that have only accelerator variables, and those that have only residual funds variables. If the authors' theory is correct then the accelerator variables and models will perform better in 1951 and 1952 (years of full-capacity utilization), while the residual funds variables and models will perform better in 1954 (a year of excess capacity) and, perhaps, in 1953 (a turning-point year).

Unfortunatey, the results are ambiguous in at least two respects. First, the procedures used do not really constitute a well-defined hypothesis test, since no basis is provided for accepting or rejecting the hypothesis. The results for 1951 and 1953 seem roughly in accord with the hypothesis, while the results for 1952 and 1954 do not. Although the authors judge the results to be a "mild confirmation" of the hypothesis, the reader is really left to guess for himself the meaning of these results. Second, an unnecessary amount of preliminary experimentation with the data is performed to help decide which variable definitions and functional forms should be used. Since such experimentation inevitably biases the results, it could preferably have been done with the Meyer and Kuh sample or replaced with more careful theoretical work.

The second part of Meyer and Glauber is a time-series study using quarterly data, primarily aggregate manufacturing, for 1949-III through 1958-IV. A recurring theme throughout this section is that the collinearity of time-series data substantially narrows the opportunity for meaningful statistical inference. But this valid concern with an annoying property of a great deal of time-series data leads the authors to employ some questionable procedures. For example, the interesting result that the residual funds variable becomes insignificant whenever capacity utilization and stock prices appear in the same equation is considered an unfortunate and an unacceptable consequence of multicollinearity. In order to restore the significance of the residual funds variable, two separate steps are taken. First, the stock price variable is replaced by its first difference (without any theoretical explanation or justification); and, second, the coefficient of the residual funds variable is constrained to be positive. The authors contend that the resulting equations, despite lower R^2's, have higher "structural quality" (i.e., support the hypothesis being tested?) and are, therefore, to be preferred.

A more direct test of the accelerator-residual funds theory is made with two subsets of the sample: one consisting of data for quarters with full-capacity utilization and the other consisting of data for quarters with excess capacity. An accelerator model is estimated for the upswing sample and a re-

sidual funds model for the downswing sample. Both models fit the data well, lending support to the authors' hypothesis. It is unfortunate, however, that both models, and, in addition, models incorporating absolute stock price, accelerator, and residual funds variables, were not fitted to both the upswing and downswing samples, so that a complete test of the hypothesis could be made.

The final and most interesting part of Meyer and Glauber is an analysis of the use of time-series models in forecasting investment expenditure. Several models that were used in the time-series study were re-estimated by methods other than simple least squares (e.g., minimization of sum of absolute errors) and tested for their ability to predict investment expenditure for seven quarters (1959-I through 1960-III) following the sample period. It is interesting that models estimated by least squares did not generally do better than those estimated by other techniques, whether in terms of R^2, Theil's U statistic, or other criteria. It might be noted that unconstrained models, including the absolute stock price and capacity-utilization variables, which the authors considered unacceptable, generally were better predictors than either constrained models or models with the change in stock price variable, which they perferred. Also the residual funds downswing model outperformed the accelerator upswing model, but did not approach the more complete models for accuracy.

The general approach of Meyer and Glauber has been to take the accelerator-residual funds theory as it was formulated by Meyer and Kuh and to concentrate on tests of statistical significance and forecasting usefulness. Kuh, however, has chosen to clarify and broaden the theoretical underpinnings of this hypothesis and to give more emphasis to structural estimation of the model. As was the case with Meyer and Glauber, however, most of Kuh's findings cast doubt on the validity of the main hypothesis.

Kuh's data consist of yearly observations from 1935 to 1955 for 60 firms and can, therefore, be used for both time-series and cross-section analysis. A set of equations, employing different functional forms and different combinations of variables, are estimated both ways. The dependent variable is gross investment, and the main independent variables are sales, gross retained profit, and lagged capital stock. The general finding is that sales has considerably more explanatory power than does the residual funds variable. While this does not directly contradict the accelerator-residual funds hypothesis, it is clear that the gross retained profits variable does not have the statistical importance that the theory suggests.

One exception to this over-all result should be noted. For the cross-section samples, when the capital stock is used to deflate the other variables, instead of being included as a separate independent variable, gross retained profit is more important than sales. This exception is of some interest, since the ratio form was used by both Meyer and Kuh and Meyer and Glauber. It is unfortunate that the author was unable to find any reasonable explanation for it.

One version of the accelerator-residual funds hypothesis advanced by Kuh is that firms adjust capital stock and dividend payments according to the Chenery and Lintner hypotheses and choose a set of adjustment parameters that will permit the internal financing of investment on a continuing basis. If

this theory were correct, it would help to explain the low reaction coefficients that frequently arise in capital stock adjustment models. Unfortunately, the estimated reaction coefficients show dividends to adjust much more rapidly than capital stock, a result not consistent with this hypothesis.

The rectangular array of data provided an opportunity to analyze the relationship between cross-section and time-series estimates, of which Kuh has taken full advantage. Working with the sales and residual funds models, Kuh finds that cross-section estimates generally vary over time and are significantly different from time-series estimates. For example, the cross-section coefficients for residual funds were found to be highly correlated with cyclical indicators and roughly twice the size of the time-series estimates.

Despite these negative findings, Kuh has suggested two ways in which cross-section data may be useful, if enough samples are available, in the analysis of behavior over time. First, a series of cross-section estimates will permit one to reject models whose coefficients are unstable on the presumption that such models are improperly specified to explain behavior over time. Kuh argues, for example, that sales models are to be preferred to residual funds models because their coefficients are less sensitive to dynamic disturbances. It should be noted that this argument constitutes an implied criticism of both Meyer and Kuh and Meyer and Glauber, whose conclusions were largely based on the cyclical variation of coefficients. Ultimately, it would be desirable to have models which hold under various conditions.

Second, cross-section data may be useful for forecasting if it has been possible to establish some systematic relationship between time-series and cross-section estimates. When there is a change in cross-section estimates that had previously been stable over time, this information might be used to modify the time-series forecasting model. It remains to be seen whether this use of cross-section data will be possible in any specific forecasting problem.

In conclusion, neither of these books represents a substantial advance in our understanding of investment behavior. Most of the models presented contain little that is both new and of theoretical interest; and, aside from the expected empirical support for the accelerator principle, most of the quantitative results are negative. Nevertheless, each book does contain sections that should prove to be useful reference material. Readers may find Meyer and Glauber's chapter on forecasting and Kuh's chapters on the relationship between cross-section and time-series models of particular interest.

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The Behavior of Income Shares—Selected, Theoretical and Empirical Issues. National Bureau of Economic Research Studies of Income and Wealth, Vol. 27. Princeton: Princeton University Press, 1964. Pp. x, 394. $8.00.

Interest in the functional distribution of income, so pronounced in the early history of economic thought, has recently been reviving—thrust to the fore, in part at least, by its relevance for contemporary problems such as those relating to inflation and growth. The evidence immediately at hand is a new

volume based on a meeting of the Conference on Research and Income dealing with the behavior of income shares.

Tibor Scitovsky leads off with a survey of distribution theories. Stanley Lebergott, Robert Solow, and S. A. Goldberg are concerned with the long-run movement of income shares (the latter in Canada), and Charles Schultze and F. H. Leacy with short-run changes in shares (the latter in Canada). Two other major papers by Michael Gort and George Borts are concerned with factor shares by industry and with the estimation of produced income by state and region respectively. An introduction by Charles Schultze and Louis Weiner provides a judicious summary both of these main papers and of the points made by various discussants.

Scitovsky distinguishes between the microtheories which center attention on the individual decision-maker's behavior and macroeconomic theories which explain shifts in income in shares in terms of changes in supply, demand, or institutional factors. Scitovsky tends to base his review of the theories on the empirical proposition that the secular increase in labor share of the national income can be accounted for by the diminution in the relative importance of agriculture and unincorporated business and by the rise in the relative importance of government. Among the microeconomic theories, Scitovsky suggests, the marginal productivity theory is still the most widely accepted. Scitovsky holds that there is too little direct evidence to support either of the main propositions upon which this theory must be based—viz., (1) that relative prices determine the proportions in which the factors are demanded, and (2) that total demand for the factors affects their prices. Scitovsky makes it clear that he does not reject the theory, but he speculates that market forces in factor markets "may be much weaker and more sluggish than is generally supposed" (p. 25). While Scitovsky makes some telling points and raises some important questions, this reviewer does not believe that he has cast much doubt upon the validity of the main propositions for purposes of explaining long-run shifts in distributive shares. (Perhaps he did not intend to.) In an economy characterized by rapid technological change and growth, decisions about factor proportions must be continually made. The cumulative impact over the years on relative factor rewards is likely to be significant even if, as Scitovsky states, new manufacturing capacity created by gross investment in the average year is only 10 per cent of manufacturing capacity. New investments rather than sunk costs tend to call the tune. Furthermore, the sharp contrasts that exist between factor proportions in the United States and in other advanced economies in the same industries *do* suggest that market forces in factor markets are pervasive and powerful, though these contrasts do not prove that adjustments are speedy.

Some light is cast on the responsiveness of entrepreneurs to long-run shifts in relative factor prices by the elasticity of substitution. For constant shares to prevail, percentage changes in the relative quantities of the factors employed must be exactly equal (and opposite in sign) to the percentage changes in relative factor prices; that is, the elasticity must be unity. With a secular increase in the capital-labor ratio, the elasticity of substitution must be less than one to be consistent with a rising share for labor.

Empirical work on the elasticity of substitution was represented in the Conference by a paper by Robert Solow. Solow applied the Arrow, Chenery, Minhas, and Solow constant elasticity of substitution (CES) function to cross-section data representing different regions of the United States in 1956 and to time-series data for different industries of the United States for 1949-58. In contrast to the earlier paper in which the CES function was first presented,[1] good fits were obtained only in about half the cases and these were about equally divided between those with elasticities less than one, around one, and greater than one. Solow himself and Robert Eisner in his comment offer reasons for believing that the elasticity estimates are biased upwards. In another comment, John Kendrick offers historical substitution elasticities computed for a score of two-digit manufacturing industries based upon factor quantities and prices in 1953 and 1957; 18 fall between 0 and 1. However, it is clear that much remains to be learned about this key parameter in the income shares puzzle.

The over-all elasticity for the economy as a whole can, as Solow points out, be affected by changes in the industry mix. In an earlier article,[2] he suggested that the labor share would tend to be stable or at least less variable if there were a tendency when labor shares are rising for industries with high labor shares to diminish in relative importance in originating national income and for those with low shares to increase in relative importance. Such a tendency might come about if the rise in the relative price of labor-intensive goods (owing to the increase in the relative price of labor) led consumers to shift their consumption into capital-intensive goods. Solow was, however, unable to find evidence of the actual operation of such a share-stabilizing mechanism (referred to by Scitovsky as "indirect factor substitution by consumers") in the data he examined.

Scitovsky considers that the most satisfactory macroeconomic theory can be pieced together from the work of Phelps Brown, B. Weber, and Kaldor. These theories turn upon the tautological relationship between the property share and the rate of return on capital and the capital-output ratio. That is,

$$\frac{R}{Y} = \frac{R}{K} \cdot \frac{K}{Y}$$

where R is property income, Y total income or output, and K the stock of capital. Aggregative theories then proceed to attempt to explain the behavior of the two ratios on the right side of the equation. If it can be shown that the rate of return and the capital-output ratio are stable in the long run, it follows that income shares remained constant. Franco Modigliani, in a

[1] "Capital-Labor Substitution and Economic Efficiency," *Rev. Econ. Stat.*, Aug. 1961, *43*, 225-50. Victor Fuchs subsequently argued in the same journal, Nov. 1963, 436-38, that the estimates produced in this paper were biased downwards owing to the inclusion of both developed and underdeveloped countries in the calculation of the production functions. When Fuchs computed separate functions for the two groups of countries, he found the data "substantially consistent" with an elasticity of unity.

[2] "A Sceptical Note on the Constancy of Relative Shares," *Am. Econ. Rev.*, Sept. 1958, *48*, 618-31.

comment on Scitovsky's paper, argues that an explanation of the capital-output relationship requires a theory of savings, and he draws on some of his prior work to supply the deficiency.

However, as Edward F. Denison points out in another comment on Scitovsky's paper, the first two ratios in the equation given above are usually expressed in terms of current dollars, whereas the capital-output ratio is usually cited in terms of constant dollars. Indeed, Denison suggests, the theoretical rationale of the stability of each ratio requires this formulation, yet there is no set of definitions which, when uniformly applied to all three ratios, will allow them to show long-term stability in the United States.

The Conference volume also includes empirical work addressed directly to the measurement of share trends. An important contribution in this sphere is Stanley Lebergott's critical analysis of the national accounts data on income shares for the first 20 or 25 years of the century which shows that we have little basis for firm conclusions about the behavior of shares in that period. He also offers an explanation for constant shares by trying to make the shares a function of the quantity and price ratios of capital and labor. The quantity ratios in the Lebergott formulation of the problem are expressed as a function of the price ratios in an earlier period. Jack Alterman, in addition to criticizing Lebergott's theoretical formulation, offers an analysis of factor shares for the corporate sector alone. He finds relative stability in factor shares between 1922-29 and 1947-59 with some indication of an upward drift in the labor share during the postwar period.

S. A. Goldberg's careful and pathbreaking study of Canadian share data leads him to the conclusion that the wage share of domestic income increased from 56.7 per cent in 1926-30 to 66.2 per cent in 1954-58. The shift was largely at the expense of unincorporated income. The wage share rises also when only private business product is considered. The increase in the wage share is greatly reduced but not eliminated when allowance is made for the decline in agriculture and in unincorporated business. Goldberg also calls attention to the possibility that the rise in the wage ratio may have reflected in part the changing industrial composition of the labor force and may have been affected also by changes in the size of establishments.

With respect to the United States the lapse of time since the papers were prepared for the Conference enables us to gain the benefit of a few more years of observation of the movement of factor shares. The U.S. data show a rise in the share of employee compensation from 58 per cent in 1929 (a figure probably representative of the 1920's) to 71 per cent in 1962-63. After 1929, the share rose sharply, reaching a peak of 74 per cent in 1933 as profits dropped precipitously in the Great Depression. With recovery, the employee share dropped to a low of 62 per cent in 1941. The share of employee compensation cannot rise indefinitely, but the clear over-all indication of the Kuznets-Commerce data is a rising trend in the share of employee compensation over the past four decades, which was temporarily exaggerated by the collapse of profits in the 'thirties.

Most of the writers in this volume—and many other economists who had written about the share question—lean to the view that the rise in the share

of employee compensation is attributable either to the particular accounting methods used in measuring national income or to structural shifts in the economy that are irrelevant to an interest in relative factor prices, factor shares, and factor substitution in the market place.

The accounting problems have been discussed elsewhere.[3] None of them, either individually or in the aggregate, is quantitatively important enough to affect markedly the rise in the employee share. Even if all the questionable accounting issues are resolved so as to minimize the rise in the employee share, the share of employee compensation still gains. Moreover, some possible methods of coping with the accounting problems, such as excluding government completely or using replacement cost depreciation, tend to leave the results observed in the official series substantially unchanged.

The other major basis for rejecting the rise in the employee share as relevant to the analytical questions of distribution theory rests on structural changes in the economy. The most important change advanced in this connection is the relative decline of unincorporated business and the rise of corporate enterprise. In this view the rise of the employee share merely reflects the fact that men who formerly received entrepreneurial income have become wage or salary earners (or at least their sons and grandsons have).

In fact, structural changes have played a significant role in the changes in shares, but, as Edward C. Budd shows in a discussion of Goldberg's paper, the rise in the employee share persists even after standardization for the relative importance of corporate and noncorporate activity and of various industries within the corporate and manufacturing sectors.

Furthermore, attempts to "explain" factor shares in terms of such shifts or by limiting attention to a particular sector of the economy such as the corporate sector suffer from the disadvantage that they exclude the effects on factor markets of shifts in the importance of employers who take different factor mixes or that they exclude whole groups of factor hirers altogether. Noncorporate entities still account for nearly 40 per cent of the nation's employee compensation and thus play a significant role in factor markets.

This argument leads to the question of the treatment of entrepreneurial income. Some writers have argued that no analytically meaningful separation can be made of the property and labor components of entrepreneurial income, and only Budd, of the authors represented in the Conference volume, is in favor of allocation. The need for allocation arises because without it we are unable to say what has happened to the division of income between shares representing rewards for current efforts of persons engaged in economic activity (i.e., what may be called the "total labor" share) and those representing the return on past accumulation of wealth (i.e., the "total property" share). The task of imputing entrepreneurial returns to capital and labor is not after all so impossible. Guidelines for the valuation of the entrepreneur's factors do exist since he has the opportunity—not infrequently taken—to enter factor markets as a buyer (to add to his own labor or capital) or as a seller (to employ some or all of his labor or capital outside of his own firm). It is true

[3] See I. B. Kravis, "Relative Income Shares in Fact and Theory," *Am. Econ. Rev.*, Dec. 1959, *49*, 917-49.

that the entrepreneur's returns on his factors—at least in some sectors and at some times—tend to be less than those available in factor markets, indicating that his preference for being his own boss makes him willing to accept lower returns on one or both of his factors. The problem of imputation is to determine which factor should be made to bear the pecuniary sacrifice. Now it is true that there is no uniquely valid answer to this question, but it is not difficult to formulate a few sensible alternatives which among them encompass the range in which the answer must fall. Furthermore, these alternatives yield different estimates of the *level* of the shares but are in substantial agreement about the *trend*.[4] The *direction* of the change in the total property share on almost any imputation formula is clear. Since the entrepreneurial share which we are splitting up is sharply declining, the amount added to the other property shares (rent, interest, and corporate profits) is likely to be smaller and smaller. For this not to be the outcome, the imputation method would have to be one which resulted in allocating a rapidly rising fraction of entrepreneurial income to property. In other business sectors, in recent decades at least, the property share has tended to fall rather than rise. Hence the probable outcome of any reasonable allocation method would be to add declining amounts to fairly constant property (rent, interest, and profits) shares (21 or 22 per cent in the period before the Great Depression and 18 per cent afterwards). At the same time the addition of declining amounts to the rising employee-compensation share would attenuate but not eliminate its upward movement. Thus if we take entrepreneurial income into account, the record points not only to a rapidly rising trend in the employee share, but also to a more modest secular growth in the total labor share and decline in the total property share.

With respect to short-run share changes, F. H. Leacy finds that while the cyclical variability of wages and salaries in Canada has usually been smaller than that of other factor returns, the opposite has tended to be the case in some industries including construction and finance insurance and real estate. Within manufacturing, the export and primary industries showed the most variation in wage shares.

Charles Schultze's paper is focused mainly upon the short-term stabilizing effects on U.S. nonfarm gross income of cyclical changes in corporate profits and saving, rather than upon an explanation of short-run changes in the distribution of income per se. Schultze treats the share of retained profits in GNP as the product of three independent ratios: (1) gross retained profits to gross profits; (2) gross profits to gross corporate product; and (3) gross corporate product to nonfarm private gross national product. He accepts Lintner's finding which is essentially that dividends are a function of a weighted moving average of past profits. With respect to the second relationship, between profits and gross product, Schultze hypothesizes that the share of profits in gross corporate product is above or below its "normal" level (around 28 per cent) according to whether the volume of output is above or below "normal capacity" (defined in terms of a trend of actual output fitted to full-employment years). For the periods he studies, 1922-41 and 1948-54,

[4] [illegible]

Schultze finds that the elasticity of profits with respect to income falls between 1.7 and 1.9.

The corporate share of total private nonfarm gross product (averaging 55 to 57 per cent) tends to decline as output falls below capacity and to rise as output increases relative to capacity; however, the amount of change in the corporate share is inversely related to the amplitude of cyclical fluctuations. The net effect of these relationships is to produce a powerful built-in stabilizer. Small shocks to the economy may be fully absorbed by changes in corporate profits with no aggregate changes in other forms of income and thus on aggregate consumption.

Michael Gort is concerned with the problem of measuring factor shares from production in individual manufacturing industries taking into account the problem of multiproduct lines within individual establishments.

In a somewhat different category from the rest of the volume are estimates of income produced in 1929 and 1953 by state and region prepared by George H. Borts. The U.S. Department of Commerce produces data on personal income by states which is composed mainly of the factor payments *received* by residents of the area. Statistics of income *produced* may however be more useful for the analytical problems dealing with the level and growth of particular regions, and Borts' paper represents a pioneering effort to fill this gap. Since the national income division already provides data on the compensation of employees by state on a "where-worked" basis, the focus of Borts' inquiry was to produce estimates for the remaining portion of income produced, which on the average comprises about 30 per cent of total national income. His critics make it abundantly clear that much remains to be done both conceptually and in the development of data estimating techniques before satisfactory estimates of income produced by state and region can be developed.

This review has concentrated largely upon issues related to the analysis of the long-run behavior of income shares. Papers and especially comments dealing with matters peripheral to this subject have been slighted or even passed over completely. Some such criterion of selection was necessary in reviewing a volume containing eight major papers and comments by 16 critics, many of whom made original contributions in the course of their discussions.

Even within the limits of its main focus, the behavior of income shares, there are some glaring gaps in the coverage of the Conference. Very little is said or done about the analysis of the supply of the factors of production, although Lebergott and Modigliani deal with the supply of capital briefly. Also, more attention might have been given to the behavior of distributive shares in countries other than the United States and Canada. Still, a conference of this character need not strive for comprehensive treatment of the subject, particularly if it succeeds—as this one did—in offering much that is new and thoughtful, perceptive reviews of what has gone before, and keen interchanges of views with respect to both. Anyone interested in the subject of the functional distribution of income or in the many problems in economics which are directly or indirectly related to it will find frequent occasion to consult this volume.

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Equilibrium, Stability, and Growth—A Multi-Sectoral Analysis. By MICHIO MORISHIMA. New York: Oxford University Press; Oxford, The Clarendon Press, 1964. Pp. xii, 228. $9.60.

In this book Professor Morishima covers extensively and intensively a special but highly important domain of modern economics which with an appropriate general term may be called the economics of constant real cost structures. In the main, the book represents the author's contribution to this domain which he had previously communicated in a series of articles. But for the occasion, the material has been reorganized into a unitary presentation and amended by several new results. The Preface states with unusual clarity and conciseness the topics treated in the book and relates them to the writers who first broached them: from Walras to Leontief and von Neumann, from Hicks to Samuelson and Joan Robinson. Thus the author himself recognizes that, broadly speaking, the topics are not new. In fact, throughout the book he aims at giving ample credit to every writer he knows to have preceded him in each topic. One should not be surprised that the author, in his eagerness to be both modest and gracious, gives, in a couple of instances, credit to the wrong author. But one should not be mislead by the author's modesty either; in the book he also explores some new and highly interesting avenues for the first time.

The book opens with an analysis of a multiple-exchange market along the lines initiated by Slutzky and rediscovered in a more interesting form by Hicks. A host of contributions—almost everyone cited by the author—already exist for the case of gross substitutability between all goods which, in contrast with the more realistic situation where gross complementarity also is present, has proved to be highly fertile in mathematical results. Morishima, too, confines his analysis to the case of general gross substitutability, for which he generalizes and extends some previous results. The author succeeds in proving this particular group of theorems without recourse to differential calculus or determinants. Certainly, this represents—as he claims—a progress in simplification over his predecessors in the same field. But at the same time, the achievement veils the formal analogy between the problems concerning a multi-exchange of gross substitutes and those raised by the shifts in the final demand in a Leontief system. Thus the uninitiated may have some difficulty in realizing why Morishima places the two topics under the same cover, Chapter 1.

Of all the theorems in this chapter, that which he calls the "LeChatelier-Samuelson principle in strong form" will certainly arrest the attention of every reader by its novel and interesting content. In essence, it states that if all goods, say G_0, G_1, G_2, are strong gross substitutes, and if tastes shift in favor of G_1 at the expense of G_0 alone, then the new equilibrium price of G_1 will increase less, in case the supply of G_2 is adjusted so as to keep the price of G_2 the same, than if the market is left to its own fate (pp. 11-15). From the context, one gathers that the author had in mind a rather artificial situation from the economic viewpoint. For the proof implicitly assumes that there is absolutely no relation between total supplies and consumers' incomes. The reviewer doubts that the proposition is true for a more realistic situation where

an increase in the supply of G_2 would have an income effect additional to the shift in tastes.

Chapter 2 deals with the stability of what the author calls a "Walras-Leontief system": a system in which the productive sector has the same structure as that found in a Leontief dynamic system, but in which gross outputs are determined by the interplay of supply and demand instead of by a given bill of net outputs. Even though the problem of the stability of such general, nonaggregative systems has received a great deal of attention in recent years, Morishima adds some interesting variations to the long list of existing contributions. The whole chapter admirably illustrates one particular bent of the author's work. Realizing that precise results can be achieved only by adopting also some assumptions remote from actuality, he endeavors to encircle, at least, the actual by several analytical models each involving a different axiomatic basis. To the same endeavor we should impute his novel results of non-*tâtonnement* pricing in a multilateral barter under two different sets of assumptions (note to Chapter 2). But the reader will, no doubt, regret that the author did not go on to discuss the economic implications of his analytical distinction between semiglobal and global stability, both of which differ from local stability.

Chapter 3 contains a thoroughgoing analysis of the equilibrium prices and outputs in a system more general than that considered in the preceding chapter: the limitationality restriction is removed and interest rate and dynamic capital accumulation are introduced. The reader should note one very interesting point in Morishima's system. Capital goods are never the object of consumer demand; also they are produced, but only their services are used and paid for in production (pp. 56 f.). Consequently, Morishima's capital-input coefficients b_{ik} have not the same meaning as Leontief's. The result is that the matrix $[b_{ik}]$ has the dimension $(n - m) \times n$ instead of $n \times n$. Also current input-coefficients, a_{ik}, of capital-goods, i.e., the last $n - m$ rows of $[a_{ik}]$, are determined from the b_{ik}'s and the particular depreciation factor of the corresponding capital good—which factor, we should note, is assumed to be independent of the industry using the capital good (p. 57).

The first problem the author takes up concerns what we may call the "static" equilibrium prices of produced goods, wage rate, and interest rate, i.e., those equilibrium values that bring about equality between prices and unit costs, the constraint of available stocks being ignored.

The nature of the results presented by the author is apt to impress almost any reader. Take Theorem 1, page 66, for instance: the *normalized* equilibrium prices of products and the equilibrium interest rate are uniquely determined for any preassigned wage rate p_{n+1} such that $0 < p_{n+1} < 1$. This theorem recalls the proposition (proved by Samuelson and the reviewer) that the equilibrium *price ratios* in a Leontief nonlimitational model are completely determined by the technological horizon alone. But it adds the certainly surprising result that the equilibrium interest rate is also determined by the same horizon. On closer examination, however, the author's result raises two questions in the reviewer's mind.

The first concerns the mathematical artifice by which the author increases

the number of the equations of the economic system so as to obtain a mathematical system capable of determining both the product prices and the interest rate: he adds the relation

$$\sum_{1}^{n+1} p_i = 1.$$

While this relation is used *within* many a mathematical proof, its inclusion in a system supposed to describe a real phenomenon is questionable. The relation is dimensionally heterogeneous. Hence, it can hardly have any phenomenal meaning for the simple reason that relations pertaining to any real phenomenon must be invariant with respect to a change of units. $\Sigma p = 1$ does not fulfill this essential condition. The reviewer thinks that the use of such a relation might be the source of unsuspected difficulties for the author's argument, as has been the case with other arguments violating the principle of dimensional homogeneity.

The second question concerns the argument upon which Theorem 1 rests. On pages 61-66, we find several statements concerning the solution of the author's quasi-equilibrium system (7′). To wit, the solution of (7′) for any normalized $0 < p_{n+1} < 1$ is such that the product prices are positive, and that the profit margin, π, is a continuous and decreasing function of the interest rate, r, with $\pi > 0$ for $r = 0$. Hence for the equilibrium involved in Theorem 1, i.e., for $\pi = 0$, one should expect $r > 0$. But let us take a simple Morishima model with only one consumption good and one capital good. Let us also assume that it is a limitational model—which does not affect the author's assumption—and that the input coefficients are such that the author's system (7′) for $\pi = 0$ is

$$(1) \qquad \begin{aligned} p_1 &= p_2 + p_3 \\ p_2 &= (0.25p_1 + 0.5p_2 + 0.25p_3)(q_2 + r). \end{aligned}$$

This system satisfies all the author's axioms (p. 59). (For Axiom 3, take $p_1 = 4\lambda$, $p_2 = 2.8\lambda$, $p_3 = \lambda > 0$.) But for normalized prices, $p_1 + p_2 + p_3 = 1$ with preassigned $p_3 = 0.5$, (1) yields $p_1 = 0.5$, $p_2 = 0$, and $r = -q_2 < 0$.[1]

In the last sections of Chapter 2 one finds a fascinating analysis of the relations between workers' propensity to consume, the rate of balanced growth of the system, and the interest rate. Almost every thorem in this group is loaded with policy implications likely to attract general attention. To wit, Theorem 3 (p. 75) states that "if the average propensity to consume of

[1] In relation with the counterexample (1) above, Professor Morishima wishes to avail himself of the publication of this review for communicating the following *Errata:*

1. The first two lines of Assumption 3, (p. 59) should read "For any positive $p_{n+1} < 1$ there exists a positive vector $\bar{p}$ such that $\bar{p}_1 + \bar{p}_2 + \cdots + \bar{p}_n + p_{n+1} = 1$, and $\cdots$."
2. After the sentence ending on line 4, page 64, insert, "As $\Sigma\bar{p}_i = \Sigma p_i^0 = 1 - p_{n+1}^0$, we get $\lambda \geq 1$."
3. On line 11, page 64, insert "for $i = 1, 2, \cdots, n$" after "one equality."

The reviewer feels that the new version of Assumption 3 is excessively strong. In fact, one can prove that it is satisfied only by a system with an unbounded technological horizon, i.e., by a system such that *any* bill of goods can be produced with one unit of labor.

workers is less than 1, the rate of balanced growth is greater than the rate of interest, while if it is 1, the two rates are equal." (The reviewer, at this point, wonders whether he was wrong in assuming once a propensity to consume greater than one, a possibility which the author considers to be analytically ruled out.)

To be sure, these last results are based on additional assumptions concerning the demand for consumer goods. But in contrast with the normalized prices, they seem fairly reasonable, actually far more reasonable than they sound in the author's formulation. Thus, the proposition that only workers have a demand for consumption goods is tantamount to saying that only wage income affects demand. In the same manner, one can take out the sting from the assumption, borrowed from Joan Robinson, that "workers only consume and capitalists only save [and invest all]" (p. 76). It is easily seen that the assumption that total demand is proportional to total employment is equivalent to "all wage-earners have identical tastes."

In Chapter 4, the author analyzes, first, stability in the long run (of prices and growth rate). The basis of the argument is now a dynamic Leontief system in which limitationality is removed by a "spectrum" of discrete processes. The reader may feel that the shift is unfortunate, for he may wonder whether the results of the previous chapter would apply in this case too, and conversely. However, one must bear in mind that the source of the material presented in the book comes from separately written articles. And, the reviewer feels sure, this shortcoming is amply compensated by the highly instructive and methodical excursion over a notoriously rough ground—innovation and mechanization—which concludes this chapter.

It is the reviewer's judgment that the last two chapters, 5 and 6, represent the real highlights of the book. Morishima can rightly claim the contribution of Chapter 5 as one in which he did not walk on trodden grounds. The model is borrowed from von Neumann, but the remarkable synthesis between simple, clear, mathematical marshalling and the economic interpretation of results, reaching into Marxian tenets, is truly the author's own.

Chapter 6 deals in fact with the so-called Turnpike Theorem formulated by Dorfman, Samuelson, and Solow. In addition to a rigorous proof—with which the author must be credited from one of his previous papers—the chapter offers additional results concerning the development path and points out some unsuspected exceptions to the main theorem.

The reviewer does not want to discharge his task without saying something also about the mathematics contained in the book: it is highly condensed. In fact, the author's strict adherence to the principle of economy of thought has cost the reviewer an unbelievable amount of hours. The reader with the same modest mathematical ability must not therefore expect a light reading. But good things—including condensed argument—are worth a high price.

Errors in a mathematical argument are known for their property of remaining unnoticed for years. It would therefore be presumptuous for the reviewer to aver that the book contains no mathematical slips besides those alluded to in this review. But he would be surprised if many more would be discovered in such careful mathematics as the author has used throughout the

book. (Incidentally, as minor things I may mention a *lapsus calami* in Assumption 7′ on page 31, as well as the peculiar substitution of "quasi-concavity" for the customary "convexity" in the definition on page 43n., which is likely to confuse the reader.)

Good books on mathematical economics have always been a rarity: in early times because it was an accident for an economist to know mathematics, nowadays because many economists are so versed in mathematics that more often than not they become fascinated by the delightful game of mathematics and thus slide away from meaningful economic problems. But in the work under review the author, while proving how consummate a mathematician he really is, seems to have let himself be carried away by "pure" mathematics only once, in the second section of Chapter 3. In most other places where mathematics prevails over economics, Morishima sees to it that the reader shall not be mislead by the gilded wrapping. Frequently we come to such frank admissions as the following: "There is no reason why the demand and supply functions should satisfy this assumption. But once it is assumed, we can prove that the equilibrium is unique" (p. 27). Through most of the book, however, the author uses his mathematical armamentarium in battles with pertinent problems of economics and conducts these battles with great insight into the economic process. And it is appropriate to note that the highly interesting remarks in plain English by which he rounds up the analysis of some chapters also bespeak such an insight. And whether he deals with already explored or new topics, Morishima seizes what is truly essential or relevant. Stepping over details, he states his results in a very methodic sequence and in an eloquent, often striking, form.

Being in essence a monograph, Morishima's book does not have the sweep —still unmatched—of Samuelson's *Foundations*. But for the reasons stated in this review, there can be no doubt that it is a remarkable book. Because of the special domain covered by the book with method and insight, the reviewer also predicts that it will become an indispensable reader for selective advanced courses, as has happened with other equally excellent works not intended as textbooks.

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National Income Analysis. By CHARLES L. SCHULTZE. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. x, 145. $4.50; paper, $1.95.

We are told in the first introductory chapter that this book is "concerned . . . with problems of economic growth and stability." Yet only one out of six chapters is devoted to the subject of growth, the bulk being allotted to that of cyclical fluctuations. The book as a whole is a useful addition to the existing elementary texts on macroeconomics, however. It is the sort of book that would please empirically minded and institutionally oriented readers, whether they be students, teachers, or publicists. This volume is kinder to the reader than most elementary writings, although sophisticated students would find much of its material too tedious and obvious for them. With the exception of Chapter 3, "The Determinants of Gross National Products," this

book is preponderantly descriptive rather than analytical, as evidenced by both the contents and the accompanying "Selected Readings" involving mostly empirical writers.

Some specific comments are in order. Chapter 2 on "National Output and Income" owes much not merely to S. Kuznets (whom the author mentions as "the most important pioneer in providing national income data"), but also to the unmentioned contributions of R. Stone, G. Jaszi, and R. Ruggles. For "providing national income data" is one thing, but formulating *conceptually cogent* and *internationally comparable* national income accounts on the basis of such data is quite another. In Chapter 3, the effect of changes in taxes on the multiplier expansion of income is explained wholly in terms of a shift of the consumption function. It is not realized that tax-rate changes also affect the *slope* of the consumption function and hence the implied value of the multiplier itself, via $k = 1/(1 - c'(1 - t))$, where k is the multiplier, c' the marginal propensity to consume *out of disposable income*, t the government marginal propensity to tax incomes, and $c'(1 - t)$ the marginal propensity to consume *out of national income* (slope in question).

In Chapter 4 on investment, and specifically in connection with the accelerator, no distinction is made between the short-run *behavioral* marginal propensity to invest $(\Delta I/\Delta Y)$ and the long-run *technological* capital-output ratio $(\Delta K/\Delta Y)$. Such a conceptual distinction is important for the light it throws respectively on the cyclical nonlinear behavior of investment-demand and on the secular growth of productive capacity. Nor is there any systematic explanation of the marginal efficiency of capital theory in the discussion of expectations and investment. In Chapter 5, "Money, Prices, and Inflation," one-sided emphasis is placed on trade unions' tendency to push wages above productivity as mainly responsible for "cost-push" inflation. Faint allusion is made to monopolists' tendency to administer the prices of raw materials and other nonlabor inputs entering marginal cost. There is no awareness here of the technological bearing upon productivity and hence upon "cost-push" inflation.

The last chapter, "Economic Growth in the United States," seems to be little more than platitude, since it completely fails to demonstrate the technical mechanism of growth while amply providing vague statistical generalizations. The author, while rightly mentioning the importance of the growth of productivity for determining the rate of growth of full-employment potential output, nevertheless neglects to show exactly what *functional* differences it makes to that target rate of growth of potential output and hence to the corresponding capital requirement to equip a growing labor force with an increasing productivity. It is necessary to show that capital must grow at a rate equal to the rate of increase in labor population *plus* the rate of increase in labor productivity if *full*-employment output is to grow at a positive constant rate, and without technological unemployment or excess capacity. Moreover, attention must be paid to the *interdependent* effect of the capital-labor ratio (K/N) and labor productivity (Y/N) on the capital-output ratio (K/Y) inasmuch as the former two codetermine the latter, via $K/Y = (K/N)/(Y/N)$. If automation increases labor productivity faster than it decreases the capital-labor ratio, or more rigorously $d(\log K/N)/d(\log Y/N) < 1$,

then the capital-output ratio (K/Y) will decrease so as to entail a higher rate of capital growth $\Delta G_k(t) > 0$ given $G_k = (S/Y)/(K/Y)$, where S/Y is the constant saving ratio and $K/Y = \Delta K/\Delta Y$.

Must all the elementary macroeconomic textbooks be so mundane and unromantic instead of being a bit more celestial and idyllic?

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Outline of Price Theory. By ALBERT M. LEVENSON AND BABETTE S. SOLON. New York: Holt, Rinehart, and Winston, Inc., 1964. Pp. xii, 260. $5.25.

Exercises and Problems in Price Theory. By ALBERT M. LEVENSON AND BABETTE S. SOLON. New York: Holt, Rinehart, and Winston, Inc., 1964. Pp. vi, 107. Paper, $2.25.

The authors of this text on intermediate price theory felt that by "concisely presenting the topics usually covered in a one-semester course it avoids the encyclopedic approach" (p. vii). Unfortunately in avoiding the perils of an encyclopedic approach, it also avoids some major topics of price theory, including welfare economics and intertemporal allocation. Such fundamental concepts as Pareto optimality, efficiency vs. equity, savings, capital, and interest never appear in the book. On the other hand the authors have seen fit to include with little justification a chapter on linear programming.

Topics covered include mathematical and graphical techniques, the theory of utility and demand, the theory of cost and production, the firm in alternative market structures, the determination of factor prices, general equilibrium, and linear programming. The content is the basic classical theory, with no reference to modern extensions or skepticism. In fact, aside from the final chapter on linear programming, there are virtually no references at all. The approach is primarily graphical, the presumption being that the student need have no mathematical training beyond high school algebra. Methods of the calculus have, however, been introduced in footnotes and appendices. The approach is also the standard stylized one of most intermediate theory texts, neglecting any empirical examples or policy issues which might motivate the student by showing the relevance of the theory to the real world.

The following strong points, however, might make this book useful as a secondary text:

1. There is a good discussion of the concept of elasticity, giving the definition and its motivation, relation to average and marginal revenue, and applications to agricultural price supports, excise taxes, exchange-rate depreciation, and the cobweb theorem.

2. The treatment of indifference analysis is fairly complete, including graphical presentations of the corner solution for nonconvex indifference curves and the Giffin good case. In the latter case the authors venture one of their only empirical statements, namely, that "although a Giffin good is a theoretical possibility, an empirical case of such a good has, as yet, not been found" (p. 105). Such a statement is not only incorrect (potatoes in Ireland in 1845 are often cited as an example of a Giffin good), it also strengthens the skepti-

cism of the student as to the applicability of the theory to the real world.

3. The treatment of the production function is also fairly complete, including a discussion of returns to proportions and scale, isoquants, and the symmetry of stages I and III. Some empirical examples would have been very helpful, however. Increasing returns to scale, for example, could have been illustrated by a pipeline, where doubling the radius doubles the input of sheet metal needed but quadruples the flow of output. Another example is the case of fixed proportions (illustrated for indifference curves but not for isoquants) which could be discussed using an explicit production function such as:

$$\text{Bicycles} = \min\left(\text{Frames}, \frac{\text{Wheels}}{2}\right).$$

4. There is a good derivation and discussion of long-run and short-run total, average, and marginal cost curves, including an explanation of the Viner-Wong paradox.

5. The short (six-page) chapter on general equilibrium gives a fairly good thumbnail sketch of the classical theory, counting unknowns and equations and proving Walras' Law of redundancy of one equation. This chapter, however, introduces the notion of numeraire rather casually; and, in general, throughout the book, there is insufficient stress on relative as opposed to money prices. There is no mention of the classical dichotomy of real and monetary economics or even the zero degree homogeneity of demand functions.

Fortunately the workbook, *Exercises and Problems in Price Theory*, makes up for some deficiencies in the text. This workbook is superior to the usual study guide and workbook: far from being a crutch for students poor in reading ability, it uses exercises and problems to show how classical price theory can shed light on real world problems. For the topics of demand, supply, cost, market structures, and factor prices there are empirical examples and questions of public policy. The workbook covers the same topics as the text but unfortunately omits general equilibrium and fortunately omits linear programming. It can be used with any intermediate price theory textbook and, if not used as a student workbook, could well be used as an instructor's manual, giving many valuable suggestions for classroom discussion and examination questions.

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Economic History; Economic Development; National Economies

Development of the Labor Surplus Economy—Theory and Policy. By JOHN C. H. FEI AND GUSTAV RANIS. Homewood, Ill.: Richard D. Irwin Co., 1964. Pp. x, 324. $8.50.

There is a good deal of analytic skill and economic insight in this book. It will certainly become a standard work on some of the most important features of the "labor surplus" type of less developed area. Yet, I believe the

book has a narrow coverage and that the problems of the labor surplus, less developed economy which it fails to mention are not much less significant than those with which it deals in detail. Let me start, however, with its positive contributions.

The book is an unusually good example of how much can be wrung out of a relatively simple model with relatively limited tools, and a few dashes of realism. The model has two sectors, agriculture and industry, with production functions which are first-order homogeneous in each sector. The only inputs in agriculture are land and labor and in industry only capital and labor, though in refinements capital is also an input in agriculture. The ingredient of realism is the initial assumption of redundant labor in agriculture whose presence has forced wages to a "subsistence" level which is a "constant institutional wage." (It might be mentioned in passing that the authors brush aside the issue of the validity of this assumption and while I am inclined to agree with their view, I think that the objections should be taken more seriously.) With painstaking care and impressive geometric skill the authors then trace the sources and effects of growth in the agricultural and industrial sectors under a variety of assumptions about technological change, population growth, and relative investment allocations.

Though the authors do not trace historical precedents beyond citing W. A. Lewis, the model is a familiar one. With slight changes it can be found in Ricardo and Marx and in Paul Samuelson's interpretation of them, in Lewis, of course, and in the work of other modern writers on less developed areas, and again with small changes in some modern growth models. The analytical conclusions are, therefore, not surprising, even in the variations on the theme, but they are presented with a textbook care, detail, and variety for which readers will be grateful.

Distinguishing the book is the constant and imaginative interpretation and application of the theory. The specific countries to which the theory is applied are nearly always India and Japan with occasional references to others.

The book starts with a survey of development in labor surplus economies. It is a good, straightforward exposition of the basic model in which redundant labor in agriculture and its subsistence are potentially available for transfer to industrial production. In spite of its zero marginal product, redundant labor in agriculture nonetheless manages to be supported at subsistence wages. Though recognizing the incompatibility of these conditions with the ordinary competitive assumptions, the authors forego the analysis of the circumstances under which such conditions can prevail. Yet they fall into a pattern of discussion in which it is the "dualistic landlord" who owns the "surplus" eaten by the redundant labor. It would, I think, be important to go more deeply into this question as it plays an important role in a number of analyses of the amount of resources available when redundant labor moves out of agriculture. It has been pointed out, for example, that when an extended family "owns" the farm's output, removal of redundant labor will not automatically make its subsistence available to the market. Likewise, it has often been pointed out that the cost of absorbing labor into the industrial and

urban sectors is not just the subsistence requirements of the transferred labor, but additional industrial and social capital, unless it is assumed that labor alone can produce this capital.

The chapter on capital accumulation has an ingenious and unusually clarifying discussion of the classification of technological change. It then proceeds to relate carefully the transfer of labor from agriculture to industry to investment and technological change. While there are interesting empirical insights, there are also occasionally obiter dicta which do not follow from the argument. For example, the authors say that "Real capital formation in the sense of increasing the quantity of materialistic agents (whether irrigation facilities or tractors, or farm tools) cooperating with each cultivator is thus likely to play only a limited and marginal role in the labor surplus type of underdeveloped economy" (p. 62). This is surely an empirical issue which cannot be settled on the basis of a theoretical argument, and as a practical judgment it seems to me to be open to serious question in a number of countries.

The general lines of the analysis suggested in the earlier chapters are spelled out in detail in subsequent chapters on industry, agriculture, and the role of markets. In these latter chapters the growth rates of output and average marginal factor productivities are related in skillful analyses to the factor elasticities of the production functions, the growth rates of the factors, and the intensity and quality of technological change.

Success in development is identified by the authors with the more rapid growth of the industrial labor force than of population as a whole. The criterion plays a central role in the theoretical and empirical analysis. Briefly, it is based on the following argument: "the rationality of labor reallocation must be sought more in terms of the need to produce industrial investment goods than in terms of industrial consumer goods following Engel's Law pressures" (p. 116). The possibility of using foreign trade as an escape from this criterion is put aside on empirical grounds of low price and income-demand elasticities for the exports of the less developed areas. Under such conditions the criterion must *over some period* be fulfilled by countries with growing per capita incomes. It does not follow, however, that the criterion must be fulfilled or should be fulfilled in *every period* in every country trying to develop. That is, while the criterion has long-run validity under their assumptions, the authors cannot claim that it is useful for policy prescriptions about the path of development. It is quite conceivable that a specific country with its own pattern of consumer demands and its given and prospective foreign exchange availabilities and special "initial conditions" would find it optimal to behave differently over some intermediate period. It is important to keep in mind the possibility that the criterion is not necessarily applicable in every period in judging it as a general guide to policy and in relation to Japan and India, the two countries studied. By this criterion Japan is a "success" case and India is not. Yet, the Japanese data span over 40 years and the Indian data 11 years. The relative sectoral comparative advantages, foreign exchange availabilities, and "initial conditions" were certainly different, and there is no reason to expect the long-run development criterion to prevail after 11 years.

This is not to argue that the Indian growth experience should be judged a success, particularly in recent years, but in my opinion it has not been successful for reasons somewhat different than the authors adduce.

It is necessary also to raise some doubts about the applicability of the argument by analogy with Japan, that India could adopt many labor-saving innovations which are not now being used in industry. There is some evidence that small-scale industry in India is more labor-intensive than large-scale, but the differences are not great and, unfortunately, there is little evidence so far of an indigenous "reservoir of innovational ideas" in this direction. It would be nice if it were so, and it may even be true that different government policies would be more encouraging to such developments. The authors make a cogent argument in this respect. Still, nearly every economist I know with experience in India, each, perhaps, for different reasons, would concur in the recommendation for greater scope for the market mechanism there. Another minor objection I would make as an Indian specialist is the repeated presumption that India has suppressed or eliminated (e.g., p. 45) its landlord class. The land reform, unfortunately in my own view, has in general not been nearly so effective as implied.

The chapters on markets and the agricultural sector are very much in the spirit of the earlier chapters and continue the careful analysis of the transformations that occur as labor shifts from agriculture to industry with capital formation and innovation in each sector. While I respect the authors' choice of content and method, I think that their decision not to develop the analysis along the lines of a multisector growth model was an unfortunate one. Had they chosen that approach, the criticism above of their success criterion would have been avoided.

Throughout the book the authors stick to the assumption of competitive markets, constant returns to scale, and no externalities. The only exception is their stipulation of a positive constant wage rate in agriculture in spite of zero marginal productivity of labor. This must certainly be one of the few books on economic development since the war in which indivisibilities and externalities are not even mentioned as possible qualifications. Indeed there is a school of opinion which would place such concepts at the center of development analysis, so their neglect is not a minor issue but a major one. For example, I do not think that a careful reading of R. Nurske or P. N. Rosenstein-Rodan would admit of a definition of "balanced growth" and "critical-minimum-effort" concepts free from any consideration of externalities and indivisibilities in production.

The final chapter on the role of trade is a good example of the insight of the authors. It is a convenient short summary, but is, I think, not closely related to the analysis of the preceding chapters.

Altogether, the book is a useful addition to the literature. Teachers and students will like it for the careful exposition and the opportunities to extend and modify the analyses in directions which the authors chose not to follow.

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Economic Development with Special Reference to East Asia. Proceedings of a Conference Held by the International Economic Association. Edited by KENNETH BERRILL. New York: St. Martin's Press, Inc., 1964. Pp. xvi, 435. $12.00.

This volume, like most conference proceedings, is difficult to review—only more so. This is true because in spite of the editor's valiant efforts—and I am sure those of the Conference organizers before him—the papers offered at Gamagori comprise a rather mixed bag, mixed in subject matter, in approach, and in quality. It is true, as Kenneth Berrill states, that the papers can be divided into two categories, those specific to Asia and those more concerned with development problems generally. They can also be divided, as he in fact does, into those dealing with "Factors Affecting Growth" (Part I) and those dealing with "Policies Affecting Growth" (Part II). Reflection on the difference between "factors" and "policies" doesn't help—to wit, a paper on trade in planned development appears under the former and one on the role of agriculture under the latter rubric. Nothing will quite do, except to proceed with a discussion of the individual papers, if on a selective basis.

Trevor Swan's lonely lead-off essay on Golden Ages and Production Functions may have been delivered at the wrong conference (was it meant for Corfu?). But, never mind, it is a brilliant piece which won international recognition—in mimeo form—long ago. (The truth will out: even though all reference to dates has been deleted from the present volume, the Gamagori Conference took place in early 1960!) It deals with a somewhat esoteric subject in mature economy growth theory, namely the choice between various golden ages (i.e., states in which all variables are growing at the same rate in the long run). As others, e.g., Phelps, Mrs. Robinson, and Van Weizsecker, have since independently shown, the golden age which achieves the highest permanent consumption stream is attained by following the "golden rule" of saving, i.e., the fraction of output saved must equal the capital elasticity of output, or, in a competitive world, total saving must equal the capitalist's share. Swan need not be reminded of the rather restrictive assumptions of his model with regard to full employment, the nature of the production function and technical progress, nor of the fact that a long-run growth theory can at best serve as "a device for sorting out our ideas." But the innocent reader stumbling upon it in a volume concerning growth experience and development policy in the less developed world is entitled to such warning.

H. B. Chenery's paper (Chapter 2) on the effects of natural resources on economic growth is really composed of two rather separate inquiries, one into the relationship between the natural resources endowment and the pattern of production (as measured by the proportion of primary production to total output) at given per capita income levels, the other into the relationship between natural resources endowment and per capita income. With respect to the former, Chenery's methodology, not uncharacteristically, is to use a 50-country cross section to obtain a "normal" relationship between per capita income and the proportion of primary production in total output, and then to "explain" individual country deviations from this norm (at similar levels of per capita income) by differences in natural resources endowments. He finds

that the 20 poorest countries (below $200 per capita) lie close to the norm, indicating that here the natural resources endowment does not substantially affect the production pattern, while among the wealthier countries deviations in the direction of more primary production than normal can be attributed to a relatively generous resource endowment, and vice versa. There are obvious difficulties with the definition of natural resources endowment as trade in "re source-intensive" products. Also Chenery seems to be saying that the poorest countries are locked into a position in which approximately 50 per cent of production is primary regardless of the resources endowment. But if this is due largely to the heavy preponderance of staple food production, as Chenery agrees, can we deduce that the relative insensitivity of this total to different natural resources endowments indicates no relationship between an economy's resource base and capacity for structural change? One suspects that the production pattern itself should be defined in a more disaggregate fashion for sensitivity analysis. But more importantly, as Chenery well realizes, the norm he arrives at is really devoid of normative connotations; nor do we find any firm behavioristic link between deviations from that norm and natural resource differentials. Yet he achieves his basic aim—that of pointing up an interesting structural phenomenon and suggesting some relevant hypotheses via his customary admirable admixture of ingenuity and empirical sense.

In the final section, Chenery proceeds to ask his second and, at least potentially, much "richer" question, namely what is the relationship, if any, between the natural resources endowment and per capita income. Here he concludes that a relatively poor natural resources base makes a good deal more difference to the countries at the lower end of the per capita income spectrum than to the richer countries which are presumably much more capable of substituting capital and labor for natural resources. But the analysis here is also admittedly more impressionistic. For not only must the natural resources base be made to approximate more nearly a stock concept, in juxtaposition with a *level* of per capita income, but the logic of any causal relationship between the two remains to be demonstrated. Once an open system is functioning well and growing in a sustained fashion, the lack of domestic raw materials may not prove much of a bottleneck—and that is true even of countries at low levels of per capita income, e.g. nineteenth-century Japan; if an open system is not functioning well, it may prove troublesome even at high levels of income, e.g., twentieth-century Argentina.

Chapters 3-5 deal with some of the other possible sources of growth, labor, capital, and entrepreneurship, with technological change conspicuously neglected. With respect to labor, Minoru Tachi presents an informative but rather descriptive summary of Japanese population and labor force characteristics. Many of the raw materials for analyzing the economic advantages enjoyed by historical Japan as a consequence of her relatively low rates of population growth are adduced, but little attempt is made to relate such demographic variables to the over-all Japanese growth record or to distinguish between the country-specific and the transferable elements of that particular experience.

The twin papers on savings and the supply of capital by Shu-Chin Yang and C. N. Vakil and P. R. Brahmananda serve to cast doubt on the usefulness of aggregate, usually residually derived, savings estimates for the less developed world. By tracing the sources of finance by type of income recipient we are in a position to obtain a clearer picture of the intersectoral and interregional flow of resources in the course of the development effort. One item of particular interest which emerges, and one on which there has been considerable dispute in the profession, is the relative importance of personal savings (92 per cent of total domestic savings in India in 1957-58) as compared to corporate and government savings. Moreover, it is noteworthy that small savings amounted to about 10 per cent of total *personal* savings, not including direct investments in agriculture and small industry (amounting to 58 per cent of that same total). While it may be true, as Houthakker found, that the wages share cannot be counted on as a substantial contributor to the economy's savings fund, to ignore the savings potential of small proprietors and landlords while concentrating exclusively on government savings and corporate reinvestment is tantamount to writing off a preponderant proportion of the economy's over-all savings capacity. This is underscored by the importance of postal savings and other financial intermediaries serving the small saver in nineteenth-century Japan.

Two papers on entrepreneurship by Luc Fauvel and P. S. Lokanathan complete the array of inputs under discussion. Both authors rightly reject the "shortage of entrepreneurship" argument to explain underdevelopment; but they are less clear with respect to the relatively crucial issue of the proper mix of government and the market mechanism in achieving the maximum participation by the economy's human resources in productive and innovative activity.

Two papers by Hiroshi Kitamura and Austin Robinson concerning the foreign trade dimension of growth follow (Chapter 6). These represent very useful, balanced pieces describing the real resources functions of international trade in considerable detail. Kitamura rightly emphasizes the dethroning of trade as the major engine of growth, and points to the need for initiating change domestically. His conclusion that the foreign exchange problem is best solved by import substitution and less dependence on unfavorable foreign markets is difficult to accept, especially when the very domestic changes he speaks of are likely to permit more and more "export substitution," i.e., the shift to exports incorporating relatively more labor and human ingenuity and relatively less natural resources. Kitamura seems to adopt this view in a final obiter dictum on the merits of regional common market groupings within the underdeveloped world. But his faith in government planning as a resource allocator and growth promoter seems a bit ex cathedra. Where is his planned economy, in which the civil servants do such a superior job in taking into account time lags, linkages, and other externalities in setting investment priorities? Kitamura should be reminded that the failure of the price structure to reflect real costs correctly is as often the consequence of planning via overvalued exchange rates and direct controls.

Robinson also stresses the opportunity offered by trade for using a more efficient (foreign) production function to obtain the imported capital goods required. Even when trade cannot serve as the major stimulus to development it is likely to play a major role in relieving the over-all resources tightness by providing a vent for its (embodied) surplus labor in the presence of a necessarily narrow domestic market. His interpretation of the importance of trade in explaining not only English but also nineteenth-century Japanese growth seems entirely correct. He concludes by discussing in some detail the precise ways in which foreign exchange constraints may impede growth and by all too briefly reviewing some of the policy levers available to governments in situations of declining export ratios.

A more general paper by Berrill, the editor, dealing with the historical experience and the "take-off" concept concludes Part I on "Factors Affecting Growth." Coming as it does on top of the recent spate of discussion on Rostow's notions, including the prior appearance of the IEA's own Constance Conference volume entirely devoted to this subject, such a paper might appear a bit unnecessary. But not so. Chapter 7 turns out to be a fresh and insightful critique of the lack of rigor inherent in the Rostowian thesis and stresses the gradual nature of economic change. One could quarrel with some of Berrill's "lessons of history," e.g., that countries during take-off must adopt the most up-to-date technology—a lesson contradicted by the very Japanese experience he cites so frequently—or one could remind him that it is the increase in the rate of savings, not investment, from 5 to 10 per cent which is the telltale mark of Rostowian take-off. However, all of that matters rather less than his able and lucid exposition of the strengths and weaknesses of the concept and its current applicability in times of greater population pressures and levels of impatience.

Part II of the volume, entitled "Policies Affecting Growth," contains papers ranging from a discussion of agriculture to the problem of the transfer of technology. Chapter 8 is given over entirely to a paper on price stability and economic growth by H. S. Ellis, who seeks to marshall all the secondhand—and mostly qualitative—testimony available on the high costs of inflation in the underdeveloped economy. I would suggest that Ellis may be largely right in his concern for the destruction of markets and incentives where deficit financing is "excessively" deployed as a tool of development, but he produces no new evidence and the sheer weight of numbers of economists who can be cited to take that position does not make it so.

Chapter 9 contains two articles on fiscal policy, a general piece by J. H. Adler and one reviewing the by now rather well-known Japanese experience between 1868 and 1895 by Motokazu Kimura. Adler's contribution is one of the best in the volume, not so much for the new ground it breaks as for its succinct and well-organized presentation of what little is known in this area, both conceptually and empirically. No doubt Adler also uses the opportunity to get some less generally accepted things off his chest, not all of which we need agree with. This reviewer doesn't understand, for example, why the "right" kind of agricultural taxation (e.g., a land tax levied on land values which reflect base-year average yields and are adjusted only at long inter-

vals) need harm production incentives at all (p. 295); nor is Adler convincing on the inevitable merits of a full-cost pricing policy in public utilities (p. 306). But his emphasis on the undue neglect of direct taxes, on the "rule of reluctance" with respect to direct government intervention in the economy, on per capita consumption and savings (voluntary and forced) rather than total, and on differential sectoral growth rates rather than aggregate are all exceedingly well taken, i.e., they coincide with this reviewer's own prejudices. But, in addition, Adler has done a good job of summarizing what little we know in this area, beyond our prejudices.

In Chapter 10, we have a general paper on agricultural policy by W. H. Nicholls and one specifically related to the Japanese experience, by Kazushi Ohkawa. Both argue convincingly on the need for productivity increases in the large traditional or agricultural sector of the dualistic underdeveloped economy if sufficient surpluses for the industrialization effort are to be generated. Both concentrate on analyzing the real resources role of each sector in a successfully growing (Japanese) case, as well as (especially in Nicholls) the pitfalls of unbalanced industrialization drives. But it is not enough to differentiate, as Nicholls does, between the "agriculture first" and the "industry first" factions—or even to opt for the former on the basis of historical evidence. It is necessary to push the analysis a bit further, i.e., toward a better understanding of the circumstances which are likely to make the multisectored system perform over time in this particular smooth and balanced fashion. Ohkawa rightly de-emphasizes the role of the government in the expansion of the industrial sector and points to the importance of large numbers of small proprietors in small-scale rural industries operating in markets complementary to the simultaneously growing large-scale industrial sector. He stops short, however, of asking what precisely made the agricultural sector begin to supply the surpluses required. One suspects that the proximity of investment opportunities in rural industry which reduces the need for extensive financial intermediation had much to do with providing landlord-entrepreneurs with the incentives for changing technology and increasing agricultural productivity.

Chapter 11 contains two papers on the choice of techniques. One is a somewhat discursive treatment by Saburo Okita that cites all the good reasons why labor surplus countries like nineteenth-century Japan should concentrate on labor-intensive production functions. The other, by A. K. Sen, constitutes an apparently rather hurriedly written run-through of many of the extant theoretical arguments on the choice of techniques in underdeveloped countries. Both papers are a bit disappointing in that they traverse relatively familiar ground and don't provide us with much forward motion in a difficult and frustrating area. Sen's caveat against the misuse of partial analysis in this area is well taken, but it is much less clear what alternative he offers, especially as he rejects the use of accounting prices—a nod towards general equilibrium analysis. His unwillingness to include consideration of changes in the output mix, especially via trade, moreover curtails the policy relevance of his paper.

The volume ends (Chapter 12) with a paper by Ingvar Svennilson on the

transfer of technology between the advanced and underdeveloped societies. The problem of how to provide most expeditiously for the government-to-government transfer of technical know-how, which can no longer be viewed as automatically "carried" by private capital, is clearly of major importance and Svennilson equally clearly offers no easy answer. This reviewer agrees with much that is said but would query the assumption that technology needs to be transferred in "packages" with the suppliers vying to capture the attention of the elusive indigenous entrepreneur. To our mind the real bottleneck is in creating the environmental or market conditions in which already existing domestic entrepreneurship can be turned loose and begins to knock on the door of the international storehouse of technology—innovating domestically where appropriate and adapting foreign technology where appropriate. I doubt we have very much to offer in terms of "selling" specific proven techniques, but much in terms of assisting with the creation of the necessary preconditions and the instituting of policies which permit the activation of latent domestic ingenuity and entrepreneurship.

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International Development: Growth and Change. By HANS W. SINGER. New York: McGraw-Hill Book Co., 1964. Pp. xiv, 295. $7.50.

This book consists of a collection of Dr. Singer's essays on the topic of development. Most have been previously published. However, in view not only of the stature of the author but also of the large number of different sources in which the essays appeared over the last 15 years, this volume is a most welcome addition to the literature in the field.

The book is divided into six parts. Two deal with an assessment of growth problems in particular areas, specifically Northeast Brazil and Africa. The Brazilian chapters, which were written after a visit to the region in 1953, constitute a comprehensive analysis of the causes of the comparative stagnation in that region as well as of possible measures for raising living standards. The African section, written since 1961, is less comprehensive and consists of a paper discussing the demographic features of the area, one considering the potential role of small-scale industry in African development, and an article analyzing the merits of the African Development Bank. Two other parts of the book contain essays devoted to particular policy issues. One of these covers issues of planning and financial development, whereas the other deals with certain trade and aid issues. The latter section includes what is perhaps Singer's most widely known article, namely, "The Distribution of Gains between Investing and Borrowing Countries," which appeared in the *Paper and Proceedings* of this journal in May, 1950.

One of the most interesting essays in the entire book is in the section on planning. This is the author's summary of a series of lectures on development planning that he delivered in Lahore in 1950. In nontechnical, straightforward language, Singer covers the entire gamut of planning problems and points out with remarkable clarity and wisdom the advantages and, in particular, some of the dangers to be avoided in development planning

The other two parts of the volume deal with a number of analytical issues in the development field. For example, one article stresses the importance of the human, technological and data infrastructure (the "preinvestment infrastructure," as Singer calls it) for rapid growth. Others give Singer's views on the balanced-growth controversy, his analysis of the obstacles to development, and his appraisal of the role of education in the growth process. In short, there is scarcely an analytical or policy issue in development economics that is not commented upon with insight and wisdom somewhere in this comprehensive book of essays. Since most of the essays are written in an interesting, nontechnical style, they would be highly suitable as additional reading material in elementary courses.

Singer must be regarded as one of the leading postwar pioneers in the economic analysis of the growth problems of less developed countries. As he rightly notes in his preface, the kind of analysis in which he was a leader just after World War II was regarded with suspicion in most academic circles. Yet today the insights and analyses made by the early postwar writers on development are standard fare in even elementary courses. Singer's contributions to a better understanding of the causes of underdevelopment have been many, but perhaps the two particular notions for which he is best known deal with the secular deterioration in the terms of trade of the less developed nations and the lack of harmony between relative factor-supply conditions in these countries (a high ratio of unskilled labor to capital) and the nature of the technology (capital-intensive) they obtain from the developed countries. Development economists are still debating the importance of these factors as growth obstacles within much the same framework set forth by Singer 15 years ago.

Just as important as Singer's analytical contributions have been his ideas concerning ways of accelerating growth by means of public policy measures. Singer has never been one of those who has made so much of the obstacles to growth that the increasing of the development rate appears hopeless. Instead, with the exception of a few early essays, he has stressed the favorable growth possibilities of public investment in selective economic sectors in contrast to the massive, big push approach. As a consequence, the comparatively recent recognition among economists that the underdeveloped economies in general have been doing remarkably well since World War II is no novelty to Singer. As one who has long been listened to with care by economic policy leaders in the less developed countries, he has been in the forefront in influencing the particular policies that have helped create this result.

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Leading Issues in Development Economics: Selected Materials and Commentary. By GERALD M. MEIER. New York: Oxford University Press, 1964. 572 pp.

This book of readings and commentary, intended "as a new kind of course book in economic development," is a very useful volume.

One reason is its scope. It contains, at a rough estimate, 375,000 words. Nine chapters present 110 readings plus an introductory note to each chapter by the editor-author and twelve other "Notes" by him which summarize readings not presented and fill gaps. The readings have been considerably pruned to reduce them to their average length as presented of some four and one-half pages each, but the pruning has been skillfully done. In a few cases the flavor has been removed, but the substance remains.

The chapters are headed: stages of development; dualistic development; capital accumulation; inflation; allocation of investment resources; industrialization and agriculture; the export sector; scope of development planning; and techniques of development planning. In the complex web of relationships among topics, any organization will do violence to some. In Gerald Meier's book one will, for example, find protectionism and fiscal-monetary policy each discussed piecemeal in various contexts. Hirschman's criticism of the "big push" thesis is placed under "Growth—Balanced or Unbalanced?" in the chapter on allocation, while Ellis' is in the first of the two chapters on development planning. But all of the topics one would expect are rather well covered, with the one exception of population, which receives only a very brief discussion under the heading of unemployment. There are a dozen or more "case studies," such as "Industrialization in the Gold Coast," by W. Arthur Lewis. All have theoretical implications. There is a rather comprehensive bibliography at the end of each chapter.

The greatest virtue of the volume is the range of sources from which readings were taken. In addition to the major economics journals, publications of international organizations (including the *IMF Staff Papers*), several well-known books on economic development, and journals of economic history and public administration, there are selections from some of the less readily available symposia and periodicals, such as the *Malayan Economic Review* and the *Bulletin of the Economic Society of Ghana*. Any student except one with access to an elaborately comprehensive library will find here useful papers to which he would not otherwise have ready access.

One wonders at some omissions: well-known pieces by Swan, Solow, and Kaldor on the theory of growth, Kaldor on taxation, Myrdal on the "backwash effect." The question is partly answered by the apparent purpose of the volume. One may divide graduate students of economic growth into those for whom the field is one in which, given assumptions not necessarily very realistic but convenient for academic purposes, they can exercise their analytical ingenuity and desire for rigor and those who have an equal interest in the analysis of growth in the actual "underdeveloped" countries. Clearly Meier's volume, though it is analytical, is for the latter. But the Swan, Solow, and Kaldor articles are about the underdeveloped countries, at one or two removes of abstraction. Their inclusion and that of one or two others, directly or by summary, would have extended the range of the volume.

My other criticism is not unrelated. The volume is, to my taste, too conventional. In a field in which theory is not yet fully mature, I would have liked sharper criticism of some "orthodox" theses, by inclusion of relevant

readings or in Meier's own "Notes," which are somewhat prosaic. As examples: He presents the usual pieces on the structuralist-monetarist controversy. They do not mention, nor does he, the logical inconsistencies of the structuralist thesis or the increase in the modesty of that thesis which results if the assumptions necessary to make it internally consistent are introduced. The underemployment concept is debated, but no empirical evidence is presented, not even general deductions from the postwar internal migrations in the low-income countries. Immediately after pieces by Kuznets, Cairncross, Habakkuk, and Gerschenkron which make basic attacks on Rostow's concept of the take-off, Meier uses that concept without comment. In his reference to my own piece on protectionism, he misses the central (unconventional) point that the assumption of disequilibrium as the normal state, because of the continuing disequilibrating effect of growth, alters the analytical frame of reference. And so on.

Perhaps these are idiosyncratic criticisms. In any event, for a teacher or student who wishes one basic reference book, the volume seems to me much the best available.

The index, unfortunately, is completely inadequate, and the "analytical table of contents," which simply lists all of the readings and notes individually under appropriate chapter subheadings, does not make good the deficiency.

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Public Policy and Private Enterprise in Mexico. Edited by RAYMOND VERNON. Cambridge: Harvard University Press, 1964. Pp. vi, 324. $6.95.

This is a companion volume to Raymond Vernon's *The Dilemma of Mexico's Development,* published a year earlier. Each of the four essays in this second volume constitutes a study of specific institutions or policies which are relevant to Vernon's original objective—namely, that of identifying the relative roles of government and private enterprise in the course of Mexican economic development. In a brief introduction Vernon takes a parting shot at the subject, and in so doing not only establishes an appropriate context for the essays which follow, but also provides some further elaboration of the thesis he developed in *The Dilemma.*

The four essays which comprise the body of the book under review differ in viewpoint and style as well as subject matter. The first one, which is authored by Miguel Wionczek, is a study of the electric power industry. This is followed by two essays dealing with the Mexican financial system and its operation—one by David Shelton which focuses on Banco de México, and another by Calvin Blair which is concerned with Nacional Financiera. The final essay by Rafael Izquierdo contains a study of protectionism in Mexico.

Wionczek's essay is the best of the lot, not only because of the thoroughness of the research which it evidences, but also because of the incisiveness of the analysis and the clarity of exposition. Wionczek relates the course of events in the electric power industry from the time of its founding by foreign financial interests in the 1890's through various stages in the relations be-

tween the privately owned companies and the government to the ultimate ascendancy of public power with the total nationalization of the industry that occurred in 1960. It is Wionczek's thesis that the record does not support the views that the nationalization represented a justified victory by the state over antisocial capitalists or that it was an unjust expropriation of private property. He argues instead that nationalization was a logical, not an ideological, solution to the impasse which developed as a result of factors over which neither side had full control—namely, on the one hand, the infeasibility of efficient and rational public regulation and, on the other hand, the irreconcilability of the legitimate pursuit of profit by the companies with the requirements of the country's development program. When the government, for political reasons, was no longer able to accommodate the companies' demands for upward rate adjustments, and when as a consequence the companies, for financial reasons, refused to invest in expanded generating and distribution facilities to the extent necessary to fulfill the requirements of the development program, the only practical solution was for the government to assume full control of the industry. Wionczek's description of the circumstances and his explanation of the ultimate outcome are both fascinating and convincing. However, this reader is left with the suspicion that Wionczek has not revealed all he knows about the negotiations between the Lopez Mateos Administration and the private companies, and in particular about why the government evidenced so much nervousness about the deals made with the power companies, and why the companies themselves appeared to be so little concerned about being taken over.

Shelton's essay dealing with the evolution of central banking and central-bank policy also shows careful research, but the author apparently finds it hard to move beyond description to analysis and interpretation. Those judgments which are offered are terribly guarded and equivocal, as evidenced by the following quotation taken from Shelton's discussion of the relation between financial development and capital formation:

> But, whatever has happened to rates of voluntary saving, one could not say with confidence that financial policies in the 1950's and early 1960's have increased the aggregate rate of capital formation—hence economic growth—as compared with what would otherwise have happened, all other things being equal. A more probable conclusion is that the rate of inflation likely to accompany a politically tolerable rate of capital formation and economic growth had been reduced (p. 184).

Shelton's principal conclusion appears to be that the present network of financial institutions and regulations in Mexico is a product of compromise between the various interested parties in the public and private sectors.

Blair's essay on Nacional Financiera contains the best information yet published on the entrepreneurial functions performed by this important and complex institution, although a truly definitive study of the over-all contribution of NAFIN to Mexican economic development must await fuller disclosure of official records. The contribution of Blair's essay is not so much the factual information it contains (although he has performed a useful service in piecing together the available quantitative information), but rather is the

clarification of the position NAFIN has come to hold at the nexus of a variety of public, private, and foreign sector relationships. With regard to what role NAFIN will play in the future, Blair concludes that this depends upon the response of the private sector to the requirements of the country's development program:

> Should the private sector respond on a grand scale, Nacional Financiera would likely continue in its passive role of screening and financing proposals. But if the response is not satisfactory to future administrations, NAFIN could be made the active promoter of many of the ventures, with or without participation on the part of private investors (p. 239).

Izquierdo's essay discusses the evolution of import policy since 1940, emphasizing especially changes which have occurred in the attitudes of private businessmen. He shows that it is an oversimplification to characterize Mexican policy over the past twenty or so years simply as one of import substitution, and that numerous considerations—such as tariff revenues, encouragement of foreign investment, and restraint of domestic price increases as well as promotion of industrialization—have governed the specific forms protectionism has taken. He argues that because conflicts of interests have arisen within the private sector with regard to tariff policy and also because the undesirable consequences of further reliance on *ad hoc* measures are now widely recognized, import policy in the future must necessarily be more discriminating and more closely attuned to the requirements of a long-run, over-all plan for promoting industrial investment. In Izquierdo's own words:

> The subordination of import policy to an over-all plan for investment in industry cannot be delayed. This is the only way to avoid abuses which inevitably arise when instruments of protection operate independently, unrelated to any long-range objectives (p. 288).

Each of the four essays contains a mixture of economic and political analysis of the sort that characterizes Vernon's earlier volume. Furthermore, each essay supports in one way or another many of Vernon's broader interpretations of the Mexican record of development. It is significant, however, that none of the essays lends much support to Vernon's essentially pessimistic prognosis of the prospects for the future. Indeed, in his introduction Vernon himself rather carefully avoids reiterating his earlier pessimistic appraisal. It would appear that Mexico's "dilemma" is best viewed as the problem of deciding which horn to use as the lever for twisting the bull's head in the desired direction rather than as of the less tractable problem of choosing between politically risky but economically sound policies and policies which are safer politically, but which would endanger the prospects for continued rapid development of the Mexican economy.

DWIGHT S. BROTHERS

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The Challenge of Development in Latin America. By VICTOR L. URQUI
New York and London: Frederick A. Praeger, 1964. Pp. xiv, 209. $6.0
paper, $1.95.

One theme runs through this loosely knit group of essays:

> For historical reasons, for ethnic affinity, for political motives, and for economic convenience, the economic and social development of all Latin America should be viewed as a process of integrating its component nations and abolishing economic, cultural, social, and perhaps someday political frontiers (p. 124).

The author openly advocates the merger of smaller states into larger ones, force if necessary (p. 92), as well as the abolition of "fictitious sovereignt of local political entities originating in historical circumstances that were r only fortuitous but far removed from considerations of economic prograi ming" (p. 94). Economic programming should apparently be the basis of n states, but that it will treat man less harshly than accident is not the expe ence of countries in and out of Latin America that are reconstructing natior life on such a basis.

Programming is in fact inevitable in Victor Urquidi's view:

> Paradoxically, although Latin America hesitates to enter into a genuine program of development, the longer such a program is postponed, the more drastic, and therefore less acceptable to democratic principles, programming will have to be when it is finally initiated (p. 155).

The only choice seems to be to accept programming now in harmony with d mocracy or later in conflict with it. We are thus not only to accept what t author advocates but to do so in a hurry; in any case argument is diffic with those who know what will finally be.

The "major political principle of economic development," the auth writes, "is the acceptance of functional restrictions on freedom of action the economic sphere" (p. 91). Moreover, these restrictions will "vary in i tensity in direct proportion to the degree of economic poverty in the comm nity" (p. 87); in other words, the poorer the community, the less liberty should have in its headlong dash toward economic growth. Liberty as wealth-creating factor never seems to enter here. However, Urquidi assures that the restrictions will not pinch too much for they will be accepted volu tarily—in fact "must be accepted voluntarily" (p. 91). Does "must be" me that voluntary acceptance will be forced on people, as "voluntary" labor today forced on the Chinese?

The author would have Latin American governments, whose past and pre ent achievements are scanty, take up tremendous burdens in their singl minded pursuit of programmed economic growth. "In short," he writes, "t government should have complete control over the allocation of financial r sources, both domestic and foreign" (p. 90). Will public and private uni abroad submit their funds to such allocation? And could not local capital e cape by flight? Yet only the allocation of "total resources" (p. 89) w suffice—certainly a giant step forward for those contemporary governmen

often several months late in meeting teachers' salaries. The danger is, o course, that governments will tend to concentrate resources—all newl} amassed in their hands—to sustain themselves rather than promote the gener al welfare. Urquidi would doubtless answer that popular pressure will forc governments to attend to greater mass consumption, not self-perpetuation though several governments in and out of Latin America are showing tha people can also be fed today on hope of tomorrow's cornucopia—peopl never left far out of sight at any rate of the *paredón*. The "invisible hand,' also to be dumped on the rubbish heap of history (p. 88), could yet be a boon to those living under the all too visible hand grown heavy.

The Latin American economy must be "programmed" to grow rapidly i order to raise rapidly the living standards of peoples whose numbers ar growing rapidly; the expected population growth is the starting point and i effect the independent variable of the process whose inevitable unfolding Ur quidi follows so confidently into the future. Yet it seems strange that an au thor advocating a clean sweep of much of the past does not also urge effort to lower the expected demographic expansion, especially for one acknowl edging (p. 2) that "in many cases" (never named) population growth is un favorable to development. Stranger still is his neglect of the possibilities fo population control that lie in modern applied science, the open sesame fo him in ridding Latin America of backwardness. It cannot be said that popula tion control would not work fast enough since the author's concern is wit "long-term, basic trends" (p. 16). He can neglect the social control of popu lation increase because he is radically optimistic about technology's capacit to turn out ever-growing supplies of goods, being also neglectful of the mora and political consequences of absolute surrender to technology. For this rea son his passionate concern for the welfare of his fellow man is missing in hi arguments, though real in his spirit.

Urquidi finds that the key to economic development in Latin America ma be in solving the problem of "uneven income distribution" (p. 14). Incom inequality not only hampers economic growth, he points out, but it also fos ters inflation "because it channels investments into activities supplying pros perous urban sectors, and away from industries producing general consume goods for which the market thus remains limited" (p. 36). But he overlook that income inequality also conveniently funnels marginal increments of in come to the rich, thus obviating the need for a rise in the supply of genera consumer goods; for the same reason it favors investment over consumptior Hence the tension, not recognized by Urquidi, between rapid economic devel opment and more equitable distribution of income. Incidentally, he states tha "Excessive inflation is certainly undesirable, because it impedes orderl growth" (p. 39). He makes no claim that moderate inflation is desirabl though the use of "excessive," if it is not simple tautology, suggests such view.

The foregoing criticism should not give the impression that the book con tains a closely reasoned argument. Rather it rambles widely in covering som recent trends in the Latin American economy, especially in foreign trade, in ternational investment, and the stabilization of commodity prices. It is in fac

much more informative than argumentative, but the author does deliver himself freely of many comments, almost all offshoots of his central plea for a programmed, continent-wide attack on backwardness.

The plea was originally delivered in 1961 to Mexican university students and the present work is a translation of the book containing his lectures. Its value is incontestable as an illustration of current thinking among many Latin American economists, especially those associated with the UN Economic Commission for Latin America, the author's center of operations for many years. ECLA in fact comes in for high praise: "This systematic and patient labor, free of prejudices and illusions, based on harsh reality, far from the diplomatic intrigues of Washington, sober and competent . . . " (p. 139). Its freedom from prejudices may in particular surprise those who know it. ECLA is also the source of almost half of the 42 tables that as an appendix make up almost a quarter of the book—tables relying generally on well-known secondary sources that only rarely go beyond 1960.

THEODORE A. SUMBERG

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The Management of the British Economy, 1945-60. By J. C. R. DOW. National Institute of Economic and Social Research, Economic and Social Studies No. 22. New York: Cambridge University Press, 1964. Pp. xix, 443. $11.50

Christopher Dow's study of recent British economic history has long been known to be in process. I first learned about this line of research nine years ago and have been awaiting the outcome for some time. It provides another bit of evidence that the really good things in life are worth waiting for, and that good scholarship ought not to be unnecessarily hurried. Dow has given us a beautifully written and incisive commentary on British economic life of the immediate postwar years and the decade of the 'fifties.

A chief virtue of the book is the historial synthesis and detailed chronology of the economic side of Britain in the recent past. To any one doing serious economic research on postwar Britain, this book is obviously recommended for assembling facts and bringing major events, not always transparent, to the surface.

This being an economics book, the Chancellor of the Exchequer takes precedence over the Prime Minister, and the first hundred pages deal with the measures taken by the Labour Chancellors, 1945-51, and then the Conservative Chancellors, 1952-60. There is also a useful appendix at the end of the volume giving a calendar of events, 1945-60.

In addition to a history of people and chronology of events, we get from Dow a summary of controls (rationing, price controls, import controls, materials allocation, etc.) and fiscal and monetary policies.

The economic historian then turns to more general economic theory and analysis, taking up essentially macrodynamic economic theory in the post-Keynesian vein and an analysis of economic performance in terms of these theoretical tools.

On the actual records of the British economy, Dow's position might be summarized this way: The economy has been reasonably well managed in the sense that major fluctuations in aggregate activity have been avoided, partly through fortunate circumstances and partly through application of Keynesian medicine, but management could have been better in ironing out some of the fluctuations that did occur. If policies had been better in giving a steadier growth, the balance-of-payments position would probably have been somewhat less serious, but the over-all rate of growth would not have been at the level now desired—4 per cent per annum. Longer-run policies of the sort now proposed by the National Economic Development Council and a wages policy are more suited to Dow's taste as a basis for securing faster over-all growth. On inflation, one of Dow's strong points of original research, he would have preferred a steadier home demand induced through a more rational and careful fiscal-monetary policy and a lower level of effective demand, giving somewhat less pressure on capacity.

The policies of the National Economic Development Council have yet to prove themselves in bringing Britain to a faster growth path. It would seem that more activist policies and plans than have yet been suggested would be needed to step up the growth rate. As for the desire to have a "little" deflation to slow down the rise in prices, this sounds all right on paper, but it requires a very delicate adjustment that runs the risk of creating a hard core of unemployment, as we have had in America for some years, at a much higher rate than is socially desirable.

For a study of British economic history, Dow's book devotes surprisingly little attention to the foreign trade and international payments position of the United Kingdom. He does deal with these matters, however, and concludes that better management of the economy, bringing steadier internal movement, would have reduced fluctuations in the balance of payments, but could not have met with all the external shocks. He does not offer a detailed prescription for getting a better long-term balance-of-payments position, but he does observe that greater domestic price stability would improve the export position and enable the country to pay for more imports that are needed for growth. It appears to me that much more attention should be devoted to specific ways in which U.K. goods could become more competitive in world markets or in which some inessential imports could be restrained.

Monetary policy comes off with pretty low marks at the hands of Dow. He is highly skeptical about the influence of monetary policy in affecting economic behavior, particularly in the real sector of the economy. During much of the period covered by the historical survey, and indeed while the author was at work on this subject, monetary policy was used with great vigor. Controls on consumer credit and intervention in the housing market are the two exceptions cited by Dow for significant monetary efforts outside City dealings. By contrast, this is a period in U.S. economic analysis when more influence has been attributed to monetary policy, especially because the wider swings in interest rates seem to have brought out more significant effects on investment decisions. It remains to be seen whether such effects can be detected in the U.K. data.

I would not disagree with Dow's preference for fiscal over monetary policy, other than requesting a closer look at recent marginal changes in our appraisal of interest elasticities, but I would point out that his fiscal policy preferences are possibly shortsighted. He lays great stress on tax manipulation, and with his judgment of the short-run impact I would not disagree. He heavily discounts public investment, however, as a countercyclical measure. He is very hasty in concluding that this side of fiscal policy is too inflexible and unworkable in the short run and does not take into consideration that judicious use of this tool can also have a big impact on future growth at the same time, probably a more efficient and bigger impact than "directive planning" of the NEDC variety.

Many of the analytical problems taken up in this book cry for the application of the natural tool—econometric model building. It is on this subject that I am most at odds with the author. Emphasis is invariably placed on the difficulties faced by econometric analysis and inefficient, crude numerical methods are substituted, save for the treatment of one problem, namely, the study of wage-price movements and inflation. Here Dow, in collaboration with Leslie Dicks-Mireaux, shines in the careful econometric analysis of wage determination and price markup. If they were able to overcome the difficulties and do such fine work on this branch of the research, why couldn't the same type of analysis be extended to consumption, capital formation, foreign trade, and the money market? The over-all analysis would have benefited a lot from a less negative approach to econometrics.

But points of criticism aside, this is a fine book and an indispensable reference for a study of English history, 1945-60.

LAWRENCE R. KLEIN

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Pricing and Fiscal Policies—A Study in Method. Edited by P. N. ROSENSTEIN-RODAN, Cambridge, Massachusetts: The M.I.T. Press, 1964. Pp. 216. $6.00.

The nine papers collected in this volume have to do with various aspects of Indian development policy. They range from substantial contributions that have already appeared in professional journals to memorandum-type notes written for the consideration of the Indian Planning Commission.

Two papers by Louis Lefeber (one written with M. D. Chaudhuri) are concerned with the location of economic activity and with transportation issues. In the paper on location, emphasis is placed on the fact that the regional location of an activity affects not only its costs of production, but other costs—e.g., transportation—as well. At the same time social and political considerations dictate that investment in retarded regions be undertaken to provide long-run relief. The problem then is to achieve a satisfactory over-all growth rate without forcing the retarded regions into an even more inferior position relative to the advanced regions than they now occupy. On the transportation question Lefeber and Chaudhuri show quite conclusively that the pricing policies followed by Indian railways have imposed a large unnecessary

burden—in the form of excessive movement and uneconomic location of industry—on the Indian economy. Also, the pricing policies have made it impossible to ascertain the amount of investment required to eliminate an apparent shortage in this sector. This is a convincing paper.

I. M. D. Little also has two papers, one on tax policy in the Third Plan and the other on the real cost of labor. The latter appeared in the *Quarterly Journal* for February, 1961. The tax article is concerned with a review of sources of tax revenues and an examination of various estimates of tax receipts. Though the author does make several points of general interest, his discussion of how he arrived at his estimates and his criticism of other estimates were probably more useful to the Planning Commission than to the general reader.

The editor of the volume also has two contributions. The opening essay in the book is a short and very general discussion of the economic and social objectives of India's development plans. The second presents some data to show that nuclear energy for the generation of power in India is more costly than the use of coal or oil.

A. K. Sen contributes a paper on working capital in the Indian economy. He distinguishes among a number of motives for holding stocks, and then tries to analyze some rough data on the extent of stocks in the Indian economy. He comes out with a conclusion consistent with casual observation, namely, that stocks are a significant absorber of savings and hence investment requirements for increasing output are greater than is generally assumed. Though Sen's data are necessarily subject to a wide margin of error, his argument is important and brings into a good focus an often-ignored aspect of capital formation.

The principles of electricity pricing are discussed in a note by Harberger and Andreatta. They seek a rate structure that will foster the full utilization of available capacity. To accomplish this objective, heavy emphasis is placed on the distinction between peak and off-peak demand. When demand fails to reach capacity, the price of electricity to users should equal the marginal cost of its production. When demand is great enough for full capacity utilization, the price should include, in addition to marginal costs, an amount based on annual fixed costs. If receipts from the latter source provide a rate of return on capital in excess of some norm, capacity should be expanded. If the rate of return on capital is below this norm, then capacity is adequate. The problem is complicated a bit if two sources of power—e.g., thermal and hydro—exist, but the argument remains essentially unchanged. These principles are acceptable but—as the authors note—they may be difficult to implement at reasonable costs.

The last paper in the book, by Sir Donald MacDougall, has to do with India's balance of payments problems. It is concerned chiefly with problems of increasing exports. The author places emphasis on recognition of the need to export and the concentration of high level talent on "pushing" exports. Control of domestic consumption of exportables, subsidies, marketing efforts, and increased efforts to increase efficiency are all discussed as important aspects of the export problem. MacDougall concludes that to solve her export

problem "an almost revolutionary change will be required in the structure India's export trade."

Given this conclusion, it is surprising that he rules out devaluation of t rupee in a discussion of less than one page. The author is afraid of inflati loss of confidence in the rupee, and of reduced incentive toward cost redu tion if devaluation occurs. He also believes that, with a little luck, the ecor my may grow into the current rate of exchange and that direct controls c achieve as much as devaluation. This is a difficult argument to accept. T best argument against devaluation—not made by MacDougall—is that t demand for India's three principal exports—tea, cotton textiles, and jute man factures—is probably quite inelastic. Devaluation would therefore reduce fo eign exchange earnings from these sources, and the short-run positive eff on other exports would not be sufficient to offset this reduction. But in a lor er-run context the effect on the allocation of investment will tend to favor e ports and make importing less lucrative than now. MacDougall points c that imported capital equipment is not used as sparingly as it could be, a that substitution possibilities between capital and labor are often underes mated. But the incentive to use capital imports sparingly is very limited long as they are as cheap as they are under existing rates of exchange. Fin ly, if the Indian government can implement the kind of tax and subsidy pı gram that MacDougall calls for as a substitute for devaluation, it can sure control the kind of problems that worry him about it.

All of these papers were written as part of the Indian Project at the Cent for International Studies of Massachusetts Institute of Technology. Th were presumably all made available to the Indian planning authorities a were, I am sure, extremely useful to that group. The emphasis on pricing a allocation policies is good indeed and is particularly pertinent in the Indi context. Most of the papers, dealing as they do with specific sets of numbe and not primarily concerned with technique or method, now seem a bit date and will not be as interesting to the current reader as they doubtless were the Indian planner at the time they were written.

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Industrial Development in Communist China. Edited by Choh-Ming l New York: Frederick A. Praeger, 1964. Pp. 205. $5.75.

This collection of essays, originally published early in 1964 as a spec issue of *The China Quarterly,* deals with more than industrial developme The twelve authors, all of whom have devoted themselves over some period one aspect or another of contemporary China, range over a wide array of su jects. There is an interpretive piece on what is the current economic philos phy of China's leaders, as well as essentially factual accounts of the planni machinery and of the shifts that have recently taken place in the location China's steel industry. There are papers on policy—the scheme of work i centives for labor in agriculture and industry, or the priority given

different types of industry—as well as technical papers on such subjects as the definition and scope of handicrafts enterprises, and the terms of trade between China and the Soviet Union.

The book's title is thus too restrictive. Indeed, some of the best contributions deal with the problems of agricultural mechanization, and with investment levels generally. It is true that the first and most comprehensive article, that by the editor, Professor Choh-Ming Li, analyzes industrial development over the important years 1958-63, but even this, quite appropriately I feel, touches upon the intimately related problem of agriculture. Several other articles emphasize the importance to China of a key problem which is coming to be generally appreciated by students of economic growth: industrial development in a developing country must be discussed in the context of the relationships evolving between industry and the remainder, the bulk, of the economy.

Despite the book's breadth, and despite these interrelationships, the volume remains a miscellany of individual pieces without a central thread. It is not obvious why these essays belong in a single volume. The title of the book, and Chinese developments more broadly, are of great importance in the world today. Officials in nations everywhere must want to know what economic might or potential supports this aggressive nation which seems to have little inclination to make friends among other powerful nations. Students and practitioners of the art and science of economic growth seek new insights on the success of the greater controls of a communist society in launching economic progress from very low levels of average income.

Apart from the conclusion (in one of the articles) that significant agricultural mechanization is unlikely "for some time to come" despite the official policy in support of such mechanization, the articles reflect no real pessimism about China's growth prospects under present plans and procedures. Indeed, one can discover some feeling on the part of writers that "eleven years of planned development, including five years of economic trials, have driven home some valuable lessons." The new industrial goals are "more balanced, more tempered" than those put forth in 1953. The new policy emphasis on agriculture may well provide a stimulus to industrial expansion, although, the writers hasten to add, "only time can tell" whether "Communist China will succeed in her new effort and whether with improved economic conditions her leaders will reverse their policy for another great leap to industrialisation."

Maybe this is as much as we should expect from scholars in a book on this subject in 1964, however great the interest and need for more rigorously formulated hypotheses and conclusions. Admittedly, there are great lacunae in the essentially official factual picture, especially after the reverses of the Great Leap Forward. But scholars in the West have made efforts to fill these gaps—and in one or two of these articles, these estimated figures are used. It would seem appropriate that great efforts should now be focused on the structural relationships which emerge with these new figures, so that some quantitative judgments of what may be possible in China can begin to be constructed. After all there has already been over the past decade or so a significant professional literature on Communist China's economy by skilled observers

abroad. Little of this may be definitive, but it is high time to build upon existing foundations or, alternatively, to change specific assumptions in this work on the basis of this new knowledge and insight.

Most of the documentation in this book, especially for the years after 1958, is from contemporary Mainland literature, and especially the speeches and reports of Chinese officials. This is a voluminous source, and one of great interest because so many of the issues which occur to social scientists are debated (what investment priorities? what advantages to larger as against smaller scale enterprises?—to provide obvious illustration). But these writings, we have seen, do not provide answers for any prolonged period, as the experience before and during the Great Leap Forward indicates. Skillfully used, they can supplement hypotheses and propositions, but they cannot substitute for such basic analytic work.

It is regrettable that, rather than reprinting existing articles, the editor did not take this occasion to expand his own long article into a fuller statement on the current Chinese economic situation and its prospects.

WILFRED MALENBAUM

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Développement économique de l'Allemagne orientale. By GUY ROUSTANG. Paris: Sedes, 1964. Pp. 240. F 16.

Since 1945 there have been two Germanys, free-market West Germany and centrally planned East Germany. The existence of East Germany is a political and economic reality that will probably be with us for some time to come. Therefore, detailed information on the economic development of East Germany is very desirable. Guy Roustang's book is another effort to present an over-all picture of East Germany's economic development since 1945. This is of course quite a task, especially as the author tries to trace not only the many institutional changes during the period considered but also to investigate the quantitative aspects of East Germany's economic growth. With all these many things to do, the author must paint with a rather large brush.

Roustang begins his analysis by asking a number of important questions: What was the effect of the socialization of agriculture and industry in a country already highly developed? How did a Soviet-type planning system work, and what happened to the standard of living? Finally, and this is of considerable interest, how does the performance of the East German economy compare with that of West Germany?

Before investigating developments in the various sectors of the East German economy, the author wisely observes that, to understand the events after 1945, one has to know what was left of productive capacity when Germany emerged from the war. This is especially important if one wishes to make a comparison between the two postwar economies. Thus, an estimate is made of capacity increases and decreases up to 1945. For this the author relies especially on Gleitze's work, various publications by the Deutsches Wirtschafts-Institut, and the U.S. Strategic Bombing Survey. He confirms earlier estimates that productive capacity at the end of the war was by no means negli-

gible and, though structurally different, was similar to that on the eve of World War II.

A special problem is to evaluate the resources which the Russians withdrew from East Germany after 1945. This is a highly controversial subject on which the author cannot throw new light. He quotes some of the widely varying estimates of the amount of actual reparations. Stalin admitted, for example, to having received $3.658 billion, whereas a West German publication puts the reparation figure at $28.0 billion. In fact there are only two things one knows for sure. One is that the reparation burden was much heavier on East Germany than on West Germany, which, instead of paying reparations, received foreign aid for reconstruction. The other is, and the author stresses this, that whatever the Russians took in reparation from East Germany, it was very little compared to the immense damage which the German attack caused in Russia during World War II.

The largest part of the book is devoted to an investigation of the structure and volume of total production, especially production in industry and agriculture. Here, the author encounters the same thorny problem which has already been investigated in another larger study of the East German economy, namely how to evaluate East German production.[1] As East German statistics give production values in *Brutto Produktion* or in *Messwerte*, these mean little to Western economists. Also, the lately published *Nettoprodukt* figures cannot be compared with value-added figures in Western countries' national income accounts. The author recognizes this problem and shows aggregate production values according to East German as well as Western methods of calculation. It is fortunate that in the analysis of the different sectors and industries output data in physical units are generally used. These can meaningfully be used for comparison.

What are the main results of the study? What does the author—who had an opportunity to visit East Germany and get a firsthand impression of prevailing economic conditions—find as to the performance of the East German economy? Relying heavily on data supplied by the West Berlin Deutsches Wirtschafts Institut, he states that since 1960 the East German economy has experienced considerable difficulties and that the rate of expansion has not been as high as in earlier years. There was a slowing down of both investment and private consumption, but more resources were used for armament. This was accompanied by a growing balance-of-payments deficit.

As far as a comparison of the West German and East German economies is concerned, the author's verdict is clear: the performance of the East German economy was inferior, especially in the last year.

What were the reasons for the leveling off of East German economic growth? The author believes that he can identify several important causes. The first is agricultural production, which remained low after the socialization of farms. Second, the forced attempt to create a heavy industry was, with given resources, costly and often wasteful (e.g., the efforts to create a

[1] Wolfgang F. Stolper with the assistance of Karl W. Roskamp, *The Structure of the East German Economy* (Cambridge, Mass. 1960).

steel industry on a lignite base). Third, the continued population drain in the 'fifties sapped much of the country's highly skilled manpower and decreased the ability to innovate. In the author's opinion the erection of the Berlin Wall in 1961, which stopped the continuous manpower loss, was probably the only way to prevent a catastrophe for the economy. Another serious handicap was the adherence to rather rigid planning procedures, which in many cases prevented efficient use of resources. Finally there was the problem of the competency of the planners. According to Roustang, officials in key administrative positions very often did not seem to have a clear understanding of the dynamics of the general development of the economy, something which must, at least in part, be attributed to insufficient formal theoretical training.

The book gives a panoramic view of the East German economy. It does not provide the specialist on socialist economies or planning with new analytical tools or pathbreaking empirical analysis. Yet it has its merits. For the reader who wishes to get a general idea of the main problems which the East German economy now faces, without working through a specialized and widely scattered literature, this book should be both informative and stimulating.

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Makroökonomische Bestimmungsgründe des wirtschaftlichen Wachstums der Bundesrepublik Deutschland von 1950 bis 1960—ein Beitrag zur Theorie des wirtschaftlichen Wachstums. By WINFRIED VOGT. Tübingen: J. C. B. Mohr (Paul Siebeck), 1964. Pp. viii, 226. DM 30.

This book was submitted to Kiel University as a *Habilitationschrift,* the major postdoctoral dissertation required for initiation to the professorship in Germany. It was written in close consultation with Professor Erich Schneider.

The exposition follows conventional lines. The first part contains the theoretical model, including a competent survey of existing literature on the subject; the second part consists of the description of the empirical data and how they were used in the computations; while the third part is concerned with the interpretation of the computed results.

Dr. Vogt has set for himself in this study the objective of determining some of the factors which limited the real rate of growth of the German economy between 1950 and 1960, concentrating especially on the rate of growth of demand and the rate of growth in supply. The demand side was broken down into the component growth rates of consumption, investment, government expenditures, and export, while the growth of supply was broken into the labor, capital, and imports component growth rates. For the analysis of these relationships Vogt develops a macroeconomic model containing 20 endogenous and 26 exogenous variables, for which he derives quarterly estimates for the period 1950-60, using previously published data for the exogenous variables. The entire complex model finally leads to the computation of the following key variables: real growth of output, the behavior of which is to be explained; "desired" growth in output, representing the demand side; and the growth limits set by the availability of capital, manpower, and intermediate products

The results of the analysis are not startling from the point of view of what is quite generally known about the German economy during this decade. At the beginning of 1950 capacity exceeded demand, a situation which was quickly reversed by 1951 owing to the Korean boom. After 1952 the rate of growth in "demand" once more determined the rate of growth in output until in 1955 the capacity was reached. Since then in every year with the exception of 1958 the growth in demand was greater than that of supply.

The statistical techniques and presentation of the data in this study leave much to be desired. The analysis of changes is based on the computation of percentage increases or decreases from one quarter to the next, then the important series are plotted on graphs. Inspection of these graphs yields the results reported above. There are no statistical hypotheses to be tested. The graphs and underlying tables give no idea of real magnitudes, the only absolute figures available are those of growth in GNP. The failure to present the basic absolute figures even in a graph makes it difficult for the reader to get a feel for the magnitudes and relationships involved. For instance, it is possible only to tell that every year between 1955 and 1960 with the exception of 1958 year-to-year increases in demand were greater than increases in output. Since one of the prime determinants of the series on "demand" is the amount of order backlog, one may be curious to see how large this backlog has become by 1960, but the data presented do not allow such an insight.

One of the most interesting statistical series in the anlaysis is that of "desired growth in demand," which is built around a useful series regularly published by the German statistical office, namely, the quantity of orders received by industry and its relation to the quantity of output produced during the same period. While this variable, for simplicity called order backlog, appears to have great potential in estimating the strength of demand and cyclical instabilities, it was not used very well in this study. Vogt assumed that the order backlog at the beginning of the period plus the preceding period's actual output are related to desired output in the current period by a "cyclical adjustment coefficient." This coefficient was computed by the use of output and change in order-backlog statistics under the assumption that it should not exceed a certain, arbitrarily selected interval of .9 and 1.1. The computed values for the cyclical adjustment coefficient were in turn multiplied by the period's actual output. Simple manipulation of the algebraic expressions used in the study shows that circular reasoning is involved and that the entire concept of "desired growth" amounts to the following: Desired growth is equal to the current output growth divided by the previous period's growth in order backlog. If the computed figure indicated a desired output of between plus or minus 10 per cent over last year's, then this rate is accepted. In quarters where computed values were outside the 10 per cent limits, the desired growth of the period was given as a range of plus-minus 10 per cent. For the final analysis of the desired growth and actual growth of output relationship, only the lowest of the intervals, i.e., minus 10 per cent, was used. This unsophisticated use of statistics characterizes the entire empirical part of the study.

The graphical presentation of the results gives rise to the following important question of interpretation of the results. Every year in the fourth quar-

ter, except in 1956, the line representing the capital-stock growth barrier, appears to be the limiting factor for real output. In the first three quarters the capital stock regularly grew at rates faster than total output. If this phenomenon is due to the fact that, because of seasonal influences, some capital stock is forced to remain idle during certain parts of the year, then the statistics will show excess capacity. But in reality throughout the year the economy's capital is used fully and may in fact be the factor limiting growth of output. This puzzle could probably be answered by proper seasonal adjustments of the series or even by concentrating the analysis on annual averages. The explanation of trends by quarterly figures is often misleading where annual data are not.

If this study is typical of the quality of economic model building and testing in Germany, then German economists are farther behind their Anglo-Saxon colleagues than is generally believed to be the case.

HERBERT G. GRUBEL

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Statistical Methods: Econometrics; Social Accounting

Aggregation in Economic Analysis—An Introductory Survey. By H. A. JOHN GREEN. Princeton: Princeton University Press, 1964. Pp. ix, 129. $4.50.

Suppose that a utility function, U, of an individual is defined over n goods, where n is a very large number. Finding the task of maximizing U subject to the over-all budget constraint too difficult, the individual wishes to proceed in two steps. He partitions the set of n goods into N subsets and defines a quantity index and a price index for each subset; he defines "sub" utility functions, U_i, $i = 1, 2, \ldots, N$, over each subset of goods, and an "aggregate" utility function, W, over quantity indices of goods. He then proceeds to maximize W subject to the budget constraint treating the quantity and price indices as though they measure quantities and prices of homogeneous goods and determine the proportions of total income to be allocated to each subset of goods. Then, taking this "aggregate" allocation of income as given, regarding them as new constraints, he maximizes individual U_i's independently of one another, thus determining the allocation of income within subsets of commodities.

The first question the author of this volume deals with is that of defining a set of conditions (1) under which indices and functions W and U_i's exist, enabling the individual to follow the two-stage maximization procedure; and (2) which insure that the two-stage maximization leads to the identical allocation of income among goods as the direct maximization of U.

The author presents a good summary of the existing knowledge on this problem, but it is a pity that he did not have the benefit of the paper by Goldman and Uzawa,[1] which appeared after the book under review had been published. If he had, the author would have been able to make his presentation much more concise and easier to follow.

[1] S. M. Goldman and H. Uzawa, "A Note on Separability in Demand Analysis," *Econometrica,* July 1964.

The second problem with which the author deals may be paraphrased as follows: Consider a set of n scalar functions:

$$y_s = y_s(X_s), \quad s = 1, 2, \cdots, n,$$

where X_s is an m-dimensional vector,

$$X_s = (x_{1s}, x_{2s}, \cdots, x_{ms}), \quad s = 1, 2, \cdots, n.$$

What are the conditions under which a set of m scalar functions,

$$z_r = z_r(x_{r1}, x_{r2}, \cdots, x_{rn}), \quad r = 1, 2, \cdots, m,$$

and two additional scalar functions,

$$y = F(z_1, z_2, \cdots, z_m)$$
$$y = f(y_1, y_2, \cdots, y_n),$$

exist, such that, for any given value of X's, F and f give an identical value for y? The reader may think of y_s as the production function of sth firm, X_s as the input vector of sth firm, z_r as a measure of "aggregate" quantity of rth input used by all firms together, and y as a measure of "aggregate" output. Then f constitutes a measure of aggregate output as a function of outputs of individual firms, and F represents an "aggregate" production function, relating "aggregate" input to "aggregate" output.

Here again, the author presents a respectable summary of the existing literature, but his discussion would not be easy for an uninitiated reader to follow, partly because the author is somewhat ambiguous in his statement of the problem at the outset (the beginning of Part III). A casual reading will leave the reader with an impression that the problem treated in Part III is an extension of that discussed in Part II, while in fact they are two quite separate problems. These questions of readability may not be of major importance elsewhere, but in a book purported to be an introductory survey, containing little which has not been known to specialists in the field, they assume rather crucial importance.

Finally in Part IV, the author attempts to apply the theorems he has summarized in earlier chapters to the problem of defining the concepts of aggregate capital and the aggregate production function in the context of models of economic growth. I am afraid that the author's results will not be very helpful for students of this subject. The only conclusion he obtains is that "if the aggregate production function is homogeneous of degree one, consistent aggregation requires that the relative marginal costs of all goods must be constant, so that in competitive conditions their relative prices must be constant. . . . [The] constancy of the relative prices of capital goods implies the constancy of their relative *quantities*. Net investment, therefore, must consist of addition of batch of capital goods in the same proportion as the stock that previously existed" (p. 83).

The author offers a number of other observations on the problems related to the definitions of aggregate capital and aggregate production function in the context of growth models, but they are only very remotely related to the mathematical propositions presented in earlier chapters. Furthermore, his observations are not always very helpful. For instance, at the end of Chap-

ter 10, the author states that the measure of output defined as the sum of the output of consumption good and the output of capital good multiplied by the ratio of the consumption good price to the capital good price is not satisfactory because it is not a very good measure of welfare of the society, and he quotes P. A. Samuelson as saying that "some sort of wealth measurement, though even more difficult to come by, is what we need" (p. 90). Important though the Samuelson argument may be, it really has little to do with the problem under discussion because if one were to have an aggregate production function defined, one needs a concept of aggregate output, whether or not it is inferior to a concept of aggregate wealth as a measure of welfare. By interjecting this kind of discussion out of context, the author seems to create unnecessary confusion in his presentation of the problem which is, after all, quite difficult on its own.

In a remarkably well-formulated introduction in Chapter 1 of this book, the author states that "Aggregation will be judged satisfactory by the economist to the extent that he believes that the costs of handling information in greater detail outweigh the greater reliability of the results he might obtain by using more detailed information; the judgment must depend, of course, on the purpose of the investigator" (p. 3). It may be my wishful thinking to suppose that, if he had paid more faithful attention to the statement just quoted, the result would have been a book that is easier and more stimulating to read.

ALBERT ANDO

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Elements of Regional Accounts. Papers Presented at the Conference on Regional Accounts, 1962. Edited by WERNER Z. HIRSCH. Baltimore: Johns Hopkins Press for Resources for the Future, 1964. Pp. xviii, 221. $6.00.

The Committee on Regional Accounts (CORA) is a small group of zealous exponents of regional economic accounting that sponsors gatherings every couple of years to review the state of the art, to explore the implications for regional accounts of research going on in other areas, and to advance the cause through proposing and promoting studies with a regional focus. This is the second volume of papers and comments from these biennial meetings. The first, *Design of Regional Accounts,* set forth in some detail possible uses of regional accounts and included valuable background material on their structure and their relation to national accounts.

Participants in the second Conference were asked to concentrate on some of the more difficult specific problems of regional accounting. The papers fall into two groups: those whose primary emphasis is the conceptual structure of regional accounts and those more exclusively concerned with data availability. Of the former, the most substantial are James Henderson's "Flows and the Analysis of Regional Development" and Perloff and Leven's "Toward an Integrated System of Regional Accounts: Stocks, Flows, and the Analysis of the Public Sector." Henderson, reporting on his work as Director of the Upper Midwest Economic Study, develops a framework for recording and analyzing flows of goods and services, employment, income, and population;

identifies the critical coefficients; and poses several hypotheses that might be tested in this framework. The Perloff-Leven paper tackles the difficult question of how stocks of productive assets (both private and public) can be related to flows of output, income, employment, etc., in a consistent set of regional accounts. In the authors' words, "what is needed is a system which not only can record historical changes in the quantity and quality of resources but also can provide an analytical description of the relationships between changes in economic flows and subsequent changes in resource stocks and, in addition, the relationships between changes in stocks and subsequent changes in the levels and composition of flows" (p. 181). Both of these papers are distinguished by their authors' awareness of the need to design their accounts to meet clearly recognized and specified decision-making or analytical objectives and their willingness to discuss the relation of the regional accounting approach to more conventional types of economic analysis.

Among the remaining papers, Edwin Terry reviews and evaluates the methods used by the O.B.E. for estimating personal income by states and then proposes a procedure for allocating income on a where-received as opposed to a where-earned basis; Jesse Burkhead discusses the problems of integrating the public sector—federal, state, and local—into regional accounts; and Dick Netzer surveys the truly formidable data problems implicit in Burkhead's public accounts. This is followed by reports on two of the more imaginative urban transportation studies of the past five years—Britton Harris for Penn-Jersey and William Niskanen setting forth the structure of the RAND model. The other two papers are Leo Schnore's discussion of the problems of estimating population movements and selecting meaningful areal units and George Stolnitz's brief appeal for a set of "manpower matrices" showing shifts of the labor force among regions, industries, and (possibly) occupations.

While this collection, along with its predecessor, provides the best available summary of the aims and methods of regional accounting, it raises for the uncommitted reader several basic questions. The first is: just what comprises this field of study? If regional accounts are viewed as subnational versions of the income and product accounts to be employed, as suggested by Perloff and Leven, "in the analysis of long-run secular trends or structural change" (p. 180), only the Henderson, Terry, Perloff-Leven, and possibly the Burkhead and Netzer papers fit squarely into the regional-accounts domain. And the latter three papers raise an additional question: why is so much attention accorded the problem of integrating the public sector into an accounts scheme? It is, of course, true that it is a difficult problem, that there is a large amount of the wrong data available, and that the policy orientation of the regional approach requires a very different political structure from that which exists, but the really pressing conceptual and data questions appear to me to lie in the development of useful accounts for the *private* sectors, and it is to this end that scarce research resources should be directed.

Finally, the most basic question raised by this book is: whence regional accounts? The writers are nearly unanimous in pointing to the shortage of appropriate data, the high cost of generating the information they would like,

and the desirability of getting a set of accounts into operation. The conclusion is also inescapable that this is an area where the research input to date has been minimal. To achieve the objectives set by the members of CORA, particularly that of analyzing regional growth and decline, will require, in Mrs. Mack's words, a "mammoth edifice of data" comparable to our national income estimates. As she reminds us, the latter came into existence in response to the twin spurs of the Depression and the theorizing of Keynes. While regional accounts may be finding its depression in Appalachia, northern New England, and other depressed regions, it is not clear that its Keynes has yet appeared. From the evidence presented in CORA's first two volumes, the next step appears to be a sharpening of the definitions and objectives of regional accounts and the development of a systematic theory of regional growth to which they can be tied.

William C. Pendleton

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Economic Systems; Planning and Reform; Cooperation

Economic Planning—The French Experience. By Pierre Bauchet. Translated by Daphne Wood. New York: Frederick A. Praeger, 1964. Pp. xiv, 299. $8.75.

To the reader, bewildered by the sheer volume of specialized books and articles on French planning, Pierre Bauchet's book will be highly welcome. The author, a convinced, but not uncritical, admirer of the system, considers briefly the diverse aspects of French planning, points out unresolved problems, and offers numerous criticisms and suggestions. Bauchet puts the current plan in its historical perspective and discusses the politics of planning (there is even a chapter on planning and the class struggle). He tells how plans are prepared and implemented, and attempts to make an appraisal of planning results. All the important facts are presented, yet he avoids the trap of losing himself (and the reader) in a maze of details.

The discussion of planning in a parliamentary setting is particularly illuminating. Planning is a highly technical task with which parliaments are not equipped to deal, and it is very difficult to find a just middle between parliamentary rubber-stamping and parliamentary meddling in the details of planning. Plans are prepared for multi-year periods, while parliaments vote annual budgets. The execution of plans must leave room for rapid changes in response to unforeseen circumstances; parliaments do not wish to delegate vast powers to administrators, while the legislative procedure is much too slow to introduce *ad hoc* changes in plans.

There are conflicts, too, between planners and the executive branch of the government. Should planners constitute a superministry? If they are not in that position, what will keep the ministries (and especially the powerful Ministry of Finance) from disregarding the plan? To what extent should the planners participate in plan execution? The tentative answers worked out in France are not fully satisfactory, yet the experiment is fascinating, and Bauchet discusses it with considerable insight.

Bauchet's description of the planning process is concise and precise. The reader is taken through the steps of the early preparatory stages, through the work of the vast specialized commissions, vaguely reminiscent of Fascist or Guild Socialist councils, to the final balancing of the plan. Then approval and—enforcement. Here things get to be a bit confused, since the government has many means of enforcement at its disposal, ranging from credit control to price controls, but it is not clear which instruments are used, when, and to what effect. In fairness to Bauchet nobody else has been able to fathom precisely how the plan is enforced—if at all.

A careful reading of this book will certainly tell the reader how the plans are made and in what setting; it will also tell him about the goals and the scope of the successive plans. But should the reader ask why plan, and what are the results of planning, the answer is less obvious. Of course one could say that planning is necessary for political reasons, and that it is self-justifying in the sense that it is a Good Thing in itself. But politics aside, is it a Good Thing? Bauchet is convinced it is, but at times he seems to fish rather desperately for reasons why. We are told, for instance (p. 5), that one of the primary aims of planning is to ensure full employment. But surely, many countries have full-employment policies without planning for individual sectors. We are told, moreover, that "the existing means of intervention" available to French planners and to the government "would not be sufficient" to cope with a serious depression (p. 229). Another reason for planning is the long gestation periods of present-day investment, as contrasted with the short gestation periods of nineteenth-century investment: "In 1890 a textile manufacturer could set up his plant and order coal for a year ahead; nowadays, the supply of energy at normal prices depends on investments made more than fifteen years ago" (p. 11). While it is possible (though by no means obvious) that, on the average, gestation periods have lengthened, I doubt very much whether it was possible in 1890 to bring a new coal mine into production on a year's notice, or whether it now takes 15 years to set up a power plant. We are also told that planning is needed for "expansion, and preferably optimal expansion" (p. 18), but that French planning has no substantial effect on the growth rate because "in the social atmosphere which prevails in France there can be no question of increasing output in a greater proportion than wages on the plea that investment—and consequently profits—must be stepped up to produce more goods" (p. 73).

If Bauchet could show that the system is justified by the results, he could be excused for being vague as to the precise reasons for planning. Unfortunately, the demonstration is far from satisfactory. True, the Third Plan goal of domestic price stabilization and of external equilibrium was achieved, but achievement can be traced directly to the Rueff reforms rather than to the actions proposed under the Plan. It is true, too, that the physical targets of the plans are often fulfilled, but is this because the planners are good at forecasting market behavior or because they influence the market? And if the market is influenced by the Plan, are the results improved thereby?

Bauchet ends up by assuming that, whenever the planners and the market deviate, the planners are right, and the market is wrong. The assumption is

"proved" on the basis of ambiguous evidence and with the aid of rather strange economic analysis. Since self-financing by enterprises weakens the government's ability to influence production through credit controls, self-financing is "bad." Indeed, few U.S. economists know how bad it is: "it enriches owners and middlemen or bankers who speculate on the stock market" (p. 106), and "it raises prices at the expense of the consumers" (p. 103). Competitive behavior is bad, too. Competitive firms reject "the discipline of a common policy" and "systematically overinvest" (p. 230). Deplorably, "official intervention has been powerless to prevent the unnecessary squandering of capital in shipbuilding, motor car manufacturing and petrol refining" (p. 91), (we may wonder whether the squandering of capital on shipbuilding had anything to do with the program-law on shipbuilding subsidies which "ensures real continuity" to which the author alludes on page 95). International competition is particularily deplorable because the "rivalry for the conquest of new markets is leading to over-capitalization and cutthroat prices." On the other hand, concentrated industries which conform to plans come in for high praise (p. 78 and *passim*).

Gradually from Bauchet's study there emerges a picture of a vast, government-led cartel, which meets every five years to prepare a plan for action. In between the planning meetings there occurs a continuous "dialogue with the authorities" (p. 90). "All the firms, public and private," we are told, are involved "in a maze of incentives and prohibitions" (p. 90). The authorities tell the firm where to locate so as to get the greatest tax advantage, what to produce to get the lowest-interest loan, and even what to do to get an advance "quasi contract" for its products.

The cartelization hypothesis might explain why the French "triumph of democratic planning" bears such a striking resemblance to the West German "miracle of free enterprise." It might also throw some light on the mystery of plan enforcement without compulsion: in a rising market it is to the advantage of the cartel participants to follow the set rules. The hypothesis leaves, of course, many unanswered questions. We are still eager to learn what the preparation of the plan-forecast does to the French economy, and how the government-industry partnership affects the distribution of income, the rate of growth, and the efficiency of resource use. Bauchet does not give any answers to these questions. He has told us what French planning is. What French planning does remains to be discovered.

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Soviet Planning—Essays in Honour of Naum Jasny. Edited by JANE DEGRAS AND ALEC NOVE. Oxford: Basil Blackwell, 1964. Pp. xi, 225. 35s.

Naum Jasny has always been a difficult man. Even at the eightieth anniversary of his birthday his long-time friends, J. H. Richter and Alec Nove, found it impossible to disguise this fact. But, being such an outstanding scholar and productive author (the book under discussion offers a well-selected bibliography), a man has perhaps no obligation but certainly a right to be difficult.

This British-sponsored collection consists of a short statistical remark (Colin

Clark) and eight essays varying widely in scope and length. They range from "priorities and shortfalls in prewar planning" (H. Hunter) to "towards a theory of planning" (A. Nove). They include "plans to urbanize the countryside" (L. Richter), the "role of the Gosbank" (G. Garvy), a "theory of international comparisons of economic volume" (P. Wiles), and discussions of the social, political, and psychological profile of "planners in 1936-37" (J. Miller), "welfare criteria in Soviet planning" (M. C. Kaser), and Russia's foreign aid program (W. Klatt). Three of these contributions are research papers (Hunter, Wiles, Kaser), one is thinking aloud, as the author (Nove) himself mentions, and the remaining four offer valuable informative summaries.

Hunter uses absolute data for five selected activities (production of electricity, rolled steel, grain, cotton cloth, and urban living quarters) in nine annual plans (1929-37), to construct a frequency-distribution profile of planned and actual percentage increases in the outputs of the Soviet economy and to test which targets had been sacrificed and which stubbornly retained, if the plan attempted too much. Wiles offers a discussion of W. Nutter's calculations on the volume and growth of Soviet Russia's industrial production. The conceptual framework employed is admirable, especially his distinction of quality (good or bad), brand (trade name), type (long or short), and product (group of commodities which does not compete as to function with any other group) and the introduction of the "unit-incomes-originating" concept as a substitute for prices. His statistical conclusions remain nevertheless somewhat meager. He argues (p. 114): "If Bornstein and Galenson and Tarn and Campbell agree it does not mean that they are right. It is on the contrary quite certain that they are wrong. They have exaggerated the purchasing power of the ruble and the volume of Soviet production, by an amount unknown. Their agreement merely means that they have all used the orthodox procedures correctly. Mr. Nutter could well be right, for the wrong reasons."

Kaser presents a *tour d'horizon* of recent Russian and Polish discussions on growth models. He puts special emphasis on unsolved problems that arise from the introduction of additional variables as the time incidence of consumption, the relationship of amortization to replacement, the returns to nonproductive investments, and points to the first signs of market research and demand analysis in Russia. Nove investigates the relationship between growth and the composition of output. He feels that "perhaps a fully fledged theory of planning, integrally linked with linear programming, and input-output analysis, reconciled at least in words with the Marxian dogma, is in an advanced stage of preparation" (p. 203). And though the determination of the basic purposes of economic policy will remain a matter for the political leadership, such a theory may "set out the way of finding the most effective means of carrying out the given objectives."

The reviewer has difficulty avoiding the temptation to imagine what Jasny might think about this book. His personal memories of the old man are limited to a long walk at the Kieler Förde that included a private lecture on Greek vases and one of Jasny's few statistical estimates that fortunately proved to be wrong. Jasny at that time (1954) calculated the probable aggregate work-

ing time still at his disposal to be five thousand hours. Though now ten years older he will very likely not look upon reading this volume as a misuse of his time, and this holds not only for the Nestor of Soviet studies.

ERICH KLINKMÜLLER

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Stabilization Policies. A Series of Research Studies Prepared for the Commission on Money and Credit. By E. CARY BROWN, ROBERT M. SOLOW, ALBERT ANDO, JOHN KAREKEN, MILTON FRIEDMAN, DAVID MEISELMAN, LAWRENCE E. THOMPSON, ARTHUR M. OKUN, MERTON H. MILLER, ALLAN H. MELTZER, OSWALD BROWNLEE, AND ALFRED CONRAD. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 558. $7.50.

The title of this instalment in the series of research studies prepared for the Commission on Money and Credit is a bit too elliptical. To only a limited extent do the papers in this volume systematically review and evaluate effectiveness of the array of stabilization policies that have been or might be used in the United States. The foreword provides a more accurate description. The papers provide "analyses of *underlying considerations* related to stabilization policies" (my italics). These "underlying considerations" involve the sorts of functional relationships with which students of monetary and fiscal policy have long been concerned.

The first paper by John Kareken and Robert Solow deals with the nature of the lags in the economy's reactions to monetary policy. The study begins by rejecting Milton Friedman's conclusions regarding the length and variability of the lag in the economy's response to changes in monetary policy, a challenge to which Friedman has now twice replied, most recently in the September, 1964 issue of this *Review.* After extensive exploration (largely by regression analysis), the authors conclude that while the full impact of a change in monetary policy "may be a long time coming, nevertheless the chain of effects is spread out over a fairly wide interval. This means that *some* effect comes reasonably quickly and that the effects build up over time so that some substantial stabilizing power results after a lapse of time of the order of six or nine months."

The Kareken-Solow paper is frankly exploratory, and the authors emphasize the tentativeness of their findings. Albert Ando and E. Cary Brown are on firmer and more familiar ground in their study of lags in the effects of fiscal policy. Their findings tend to confirm those of earlier studies, particularly with respect to the strong and fairly prompt effect of changes in the personal income tax on consumer spending. The magnitude and promptness of the impact of changes in the corporate income tax are less clear. And as for discretionary changes in government spending, the authors confirm general impressions as to the long lags that are involved.

In a rather startling paper, Friedman and David Meiselman seek to evalu-

ate the relative stability of monetary velocity and the investment multiplier. The authors maintain that their results are "strikingly one-sided"—in favor of a close and stable relation between money and consumption and against the existence of such a stable relation between consumption and autonomous expenditures. Their study is indeed one-sided—but as much in method as in results. Starting from the identity $Y = C + A$ (where A is the sum of net private domestic investment, government deficit on income account, and net foreign balance), they regress C on A for various subperiods during 1897-1958. They conclude that what they refer to as "the investment multiplier" is "consistently and decidedly" less stable than the income velocity of money except during the 1930's.

The bias built into this approach has been explored in detail in a paper by Ando and Modigliani which will soon be published in this *Review*. As the latter point out, the Friedman-Meiselman approach relates C to an independent variable (A) which has important induced components, some of which are inversely related to C; consumption is implicitly related not to disposable income but to a larger total including corporate saving; no systematic attempt is made to measure the effect of the separate components of A, which changed in relative importance both cyclically and in the long run; and the simplest possible form of the consumption function is used, thus ignoring ratchet, "permanent income," and similar effects. In addition, a number of the regressions include war years.

To say all this is not to deny an important role to money. But the oversimplified and essentially biased correlation game played by these authors is not likely to advance our knowledge of the whole complex of factors affecting the level of income and output or how monetary and fiscal policy can be most effectively used to mitigate economic instability.

Space limitations preclude more than passing mention of the other papers. I was particularly impressed by Arthur Okun's paper on the impact of monetary policy and debt management on interest rates. More than the other authors, he has sought to quantify the effects of specific types of policy action. Perhaps he has been almost too bold in his attempts at quantification. Among his conclusions are that changes in the maturity structure of the federal debt have relatively small effect on the term structure of interest rates and that, as a corollary, the significance of the Fed's "bills only" policy has been badly exaggerated. In his words, "*how much* the Federal Reserve buys (or sells) is far and away more important than *what* issue it chooses to deal in."

Lawrence Thompson, in his competent paper on income velocity and liquid assets, reaches the conclusion, among others, that the shift from demand deposits to other liquid assets has not run counter to the objectives of monetary policy. Merton Miller demonstrates that the high level of the corporate income tax has not in fact caused a significant increase in the ratio of debt to common-stock issues. Allan Meltzer, following up on an earlier study, analyzes the redistribution of the money stock brought about by trade credit in response to monetary tightening. He argues that tight money does not discriminate against small firms because, when money tightens, large firms lend to their small customers. He does not, however, consider the question as to

whether such trade credit is, in fact, a full substitute for bank credit from the point of view of the borrowers.

The final paper by Oswald Brownlee and Alfred Conrad considers the effects of a tight money policy on the distribution of income. They conclude that "Monetary policy is not an effective and reliable instrument for obtaining any desired redistribution of income." Has anyone claimed that it was? To reach this not very surprising result, the authors engage in a game of estimating income effects that this reader was not always able to follow.

The empirical findings in this volume are anything but definitive, and the authors clearly look on their papers as being primarily exploratory in nature. It is too bad, also, that it has taken so long to get these papers into print. Even so, the volume is a useful contribution to the literature—so long as it is read with critical discrimination.

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Private Capital Markets. A Series of Research Studies Prepared for the Commission on Money and Credit. Englewood Cliffs, N.J.: Prentice-Hall, 1964. Pp. 531. $7.50.

Besides communicating their specific scientific content, the recent wholesale release of CMC-sponsored research studies has cast light on a very interesting area of economic behavior. Just what do different members of our profession consider to be the proper task of a diagnostic specialist reporting to a commission convened for the express purpose of investigating every detail of the nation's financial system? What kind of a study should such a specialist produce? How far in the background should an investigator keep his own preferences and prejudices?

The three studies included in the volume under review present three different answers to almost every aspect of these questions. In form, they run the gamut from relatively pure theory to relatively pure description. In intent, they range from the mainly persuasive to the mainly informative. In content and style, however, they all share a certain tedium, and their common neglect of international complications stands in mute testimony to the profession's sluggishness in coming to grips with this important phase of the stabilization problem.

Of the three papers in the volume, Irwin Friend's study, "The Effects of Monetary Policies on Nonmonetary Financial Institutions and Capital Markets" (pp. 1-172), probably had the greatest impact on the outlook of the Commission. Reading it, one finds it somewhat easier to understand why the Commission's Report was so modest in its recommendations for reform. For, with respect to the crucially important empirical question of the degree of substitutability between ordinary money and various near-monies, Friend manages to give the impression of having produced both a definite answer and a definitive work. His answer is that in the postwar United States substitutability between money and the assets of the major nonmonetary institutions has been negligible and that, consequently, no extension of central-bank control over nonbank intermediaries (à la Gurley and Shaw) appears justified.

Friend begins with a massive presentation and laborious interpretation of national balance-sheet and flow-of-funds data. He proceeds to employ these figures, both by themselves and in combination with series on interest rates, to test the asset-substitutability hypothesis. While none of these tests is wholly satisfactory, they nevertheless vary enormously in quality. As the paper progresses, Friend passes from simple—often frankly inappropriate—tests (e.g. computing the [positive] correlation between demand deposits and savings and loan shares on the null hypothesis that substitute assets ought to move inversely) to more properly "econometric" ones. Everything culminates in the estimation of alternative forms of a crude 25-equation quarterly economic model. This model and estimates of its parameters are discussed equation-by-equation in an appendix by Murray Brown (pp. 117-72).

Through most of the text, Friend cautions the reader regarding the weaknesses which beset each of his tests. In fact, the phrase "admittedly inconclusive" takes on the status of a *leitmotiv*. Unfortunately, important as it is in the composition's development, this theme plays precious little part in its resolution. Friend maintains (p. 109) that, while none is completely adequate, "together" his tests constitute "about as satisfactory a picture as it is presently possible to obtain of the respective roles played by monetary and nonmonetary intermediaries." The unwary reader, all but buried alive in evidential detail, is encouraged to believe that Friend's many tests add up to something far greater than their "admittedly inconclusive" greatest ingredient. To convey this impression to a group as powerful as the Commission on Money and Credit seems a questionable service indeed.

The second paper in the volume, "Financial Crisis, Financial Systems, and the Performance of the Economy" by Hyman P. Minsky (pp. 173-380), both in spirit and in execution is totally unlike the first. Whereas Friend constructs a coherent empirical case for the *status quo* in monetary control, Minsky produces a loose theoretical justification for initiating sweeping changes. Much of this difference traces to different points of emphasis and departure. Friend worries about the Federal Reserve's ability, as an active agent, to constrain booms. Minsky is concerned with the Fed as lender of last resort in times of economic decline.

Furthermore, while Friend inherited an analytic framework (that of Gurley-Shaw) on which to build, Minsky had to start from scratch. It is not surprising, then, that Minsky turns out a patchwork piece which he himself feels compelled to label "a pilot investigation . . . rather than a polished and finished study." As such and despite various logical and empirical gaps, it raises an interesting and important question: Has the United States really seen the last of financial panics? Needless to say, Minsky is far from convinced that it has.

As his central hypothesis, Minsky offers a variant of the familiar notion that in business fluctuations the boom is responsible for the bust. His version has two parts: (1) that financial changes which accompany a sustained boom in private demand (mainly increases in the ratios of private debt to income and of intermediary to total debt) increase the likelihood that routine interruptions in income growth will engender financial crisis and in such a way

that the probability of financial crisis increases with the duration of such booms; and (2) that truly serious recessions only occur when expenditure contraction is reinforced by financial difficulties.

Minsky exposits these hypotheses with reference to a linear growth model of the multiplier-accelerator type, one whose parameters are presumed to change with labor-force and capital-consumption constraints and with changes in the financial environment. He asserts that a serious economic depression can occur only if stabilizing forces which normally tend to reverse economic declines (various expenditure lags and floors) are somehow negated or offset. Minsky finds such an offset in the occurrence of widespread financial distress—where "units' decisions are dominated by the need to meet financial commitments." On such occasions, the collapse of asset values and of the liquidity of financial institutions prevents units from carrying out their ordinary spending plans.

In his empirical chapters, Minsky attempts to test both the necessity and plausibility of this explanation. First, economic fluctuations in the United States between 1919 and 1958 are found to be roughly consistent with this interpretation. Also, while different in some respects, the boom of the 'fifties is enough like that of the 'twenties for Minsky to declare that the financial system is becoming increasingly less stable. Next, nine simulation experiments, each employing a slightly simplified version of the Duesenberry-Eckstein-Fromm quarterly model of the U.S. economy in recession, are undertaken. In these experiments which posit continued financial stability and expenditure reductions of varying severity, only the largest decline in income (18 per cent in eight, 25 per cent in eleven quarters) appears comparable to 1929-33. But so severe a recession involves a great and prolonged collapse in private investment, one which, according to Minsky, could only occur in the case of concomitant financial crisis. Finally, Minsky attempts to assess the degree of pressure (in the form of consumer deficits to be financed) the larger of the simulated declines would focus on saving and loan associations. Although he finds that associated outflows could be sustained overall, he points out that a sufficiently unfortunate interregional distribution of fund flows could reverse the picture.

The paper concludes with recommendations for reform of the Federal Reserve System designed to prevent distress in one part of the financial system from burgeoning into full-fledged crisis. Minsky would make the Fed the overseer of, and lender of last resort to, the entire financial system. He would place federal insurance and guarantee schemes under central-bank control. Elsewhere in the paper, he supports Fed management of the national debt, universal System membership for commercial banks, state-wide bank branching, and greater freedom of investment for nonbank intermediaries. While they bear upon the problem he has set, consequences of introducing these reforms are nowhere systematically explored. As a result, in context these recommendations appear somewhat gratuitous.

The third and final research study contained in *Private Capital Markets* is a paper by Victor L. Andrews with the ponderous title, "Noninsured Corpo-

rate and State and Local Government Retirement Funds in the Financial Structure." In stark contrast to the performances of Friend and Minsky, Andrews eschews both models and explicit policy recommendations. He merely describes events for the reader's general information. The impact of noninsured, nonfederal retirement funds, especially during the 1950's on the flow of funds into and out of various financial markets constitutes his subject and careful discussion of a veritable horde of tables his method. Along the way, Andrews examines such questions as: Why have these pension funds been so slow to enter new asset markets? What funds redistributions have they brought about? Has their existence tended to increase or decrease the aggregate savings-income ratio? In every case, the issues are treated conscientiously and the author's own views duly recorded. While it is unlikely that so antiseptic a study provided much positive assistance to members of the Commission, at least it could not have led them very far astray.

EDWARD J. KANE

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The Demand for Liquid Assets: A Temporal Cross-Section Analysis. By EDGAR L. FEIGE. Ford Foundation Doctoral Dissertation Series, 1963 Award Winner. Englewood Cliffs, N.J.: Prentice-Hall, 1964. Pp. xii, 91. $4.50.

The demand for money plays a conspicuous role in aggregative theory, classical or Keynesian. Much of the controversy regarding the effectiveness of monetary policy in a world of complex financial intermediation relates to the nature and stability of the demand for money. In this Ford Foundation award-winning doctoral dissertation, Edgar Feige sets himself the difficult and important task of empirically testing several hypotheses about monetary behavior. Feige regards three hypotheses as particularly relevant to the effectiveness of monetary policy (p. 1): (1) The liabilities of nonbank financial intermediaries are close substitutes for money. (2) The demand for money is a stable function of a limited number of variables. (3) The demand for money is independent of the supply of money.

Using regression analysis, Feige estimates demand functions for demand deposits, time deposits, and saving and loan shares for each of the years 1949-59, using cross-section data for 48 states (plus the District of Columbia). The dependent variables are the per capita holdings of an asset in a state. The principal explanatory variables for each asset are state per capita income and the rates of return on these three assets and on mutual savings bank deposits. He estimates altogether 11 $\times$ 3 cross-state regressions, one for each asset for each year. He also estimates 3 "pooled" relationships, one for each asset, in which the 11 years of observation are thrown together.

On the basis of his regression results, Feige concludes (pp. 43-44) that the different assets are not close substitutes, that the demand function for money does not appear to have become less "stable," and that the demand for money and the supply of money are not interdependent as a result of induced financial innovation.

Unfortunately, there are several conceptual problems with Feige's procedures and, as a result, I do not find the evidence for his conclusions persuasive. The first difficulty relates to the character of his data. Feige does not discuss the appropriateness of using cross-section data. When the monetary authority changes the money supply, it is interested in the effect on the whole economy, and in the adjustments in income and rates of return which take place in a relatively short period of time. In contrast, the relationships estimated from cross-section data are likely to be long run, reflecting largely completed adjustments to differences in income, wealth, and rates of return.

A second difficulty relates to Feige's test of stability. He correctly (p. 1) emphasizes that predictability of the response of the economy to monetary actions is much more important for effectiveness than the magnitude of that response. The monetary authority can adjust the size of its actions to allow for the responsiveness of the economy. Although I am in wholehearted agreement with this emphasis, Feige's treatment would have benefited from a more careful consideration of how it relates to stability in the demand function for money.

Feige tests predictability by examining the "stability" of the coefficients in the cross-section regressions to see whether these are significantly different over time. Although predictability in the response of the economy is related to stability of these coefficients, I would have thought that changes in the variance of residuals are more directly relevant to the question of effectiveness. In a world of uncertainty, knowing the expected size of response is not particularly useful if the variance in response is large. Feige's test for "stability" of the coefficients can itself be misleading. He compared the unexplained variance when the coefficients are restricted to being the same every year (the pooled regressions) with the total unexplained variance when separate estimates are made for every year. This tests at once the coefficients of all the independent variables. But there are many variables in the annual cross-section regressions whose coefficients are not significantly different from zero, and therefore not surprisingly insignificantly different from each other. This makes the test insensitive to possible instability of the more interesting rate coefficients. Casual inspection suggests, for example, that the coefficient on the time-deposit rate in the equation for time deposits was significantly different from the pooled coefficient in approximately half the years.

Despite Feige's intention of examining the "independence" hypothesis, he in fact assumes it to be true in his empirical work; "the level of the supply curve in each state is determined primarily by state laws and regulations governing the activities of financial intermediaries. The rates of return are therefore considered as exogenous variables" (p. 21).

This assumption conveniently by-passes a serious specification and identification problem which plagues monetary analysis of the type Feige undertakes. It is commonly assumed that in the aggregate the supply of money is fixed, and consequently it is not surprising that the value of demand deposits shows little relationship to yields on alternative assets. On a cross-section basis, supply is sensitive to the same variables as demand. Is it regulation or is it the building boom which makes the California S and L rate high? It is

difficult for me to believe that states, where loan demand (and income) at S and L associations are high, will not be, for that reason, states where high rates are paid on S and L shares. The way in which rates of return and quantities are determined in the 49 "markets" Feige considers is far from obvious, and his analysis would have greatly benefited from some discussion of these "markets."

Several of Feige's regression results should be interpreted with considerable caution. Feige uses total service charges divided by average demand deposits balances for the period as his "rate" on demand deposits. But this is a very poor proxy for the marginal return to increased holdings of demand deposits. Since there is a large "fixed" component to service charges, the large coefficient for this rate in his equation for demand deposits may be a supply phenomenon. The coefficient itself is implausible. It indicates that a reduction of .2 of one per cent in service charges would lead to a $110 increase in per capita demand deposits. This sizeable shift (somewhere on the order of 20 per cent of average per capita deposits) is in response to an increase in the earnings potential of these deposits of $.22 per year. I find it difficult to believe that individuals are sensitive to such differences. It is also hard to accept such a large coefficient of the demand deposit rate in the equation for demand deposits when according to the other regressions it has a relatively small impact on the volume of other financial assets. Where do the demand deposits come from?

Another problem relates to Feige's explanation of the apparent positive relationship between the rate on S and L shares and the volume of demand deposits. Feige explains this "complementarity" by arguing that increases in the S and L rate, which increase the direct demand for saving and loan shares, indirectly increase the demand for deposits (as Saving and Loan association reserves). But according to his own coefficients, the increase in the volume of saving and loan shares, which results from a given increase in the S and L rate, is *smaller* than the corresponding increase in demand deposits.

Although I found Feige's study stimulating, it leaves unanswered many of the critical questions to which it is addressed.

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Consumer Credit Costs 1949-59. By PAUL F. SMITH. National Bureau of Economic Research Studies in Consumer Instalment Financing No. 11. Princeton: Princeton University Press, 1964. Pp. xix, 160. $4.50.

This book continues the National Bureau of Economic Research "Studies in Consumer Instalment Financing," the last volume of which appeared in 1944. Paul F. Smith, formerly an economist with the Federal Reserve and now with the Wharton School, undertakes to compare the costs of granting consumer credit among the four major types of financial institutions which engage in consumer lending: commercial banks, sales finance companies, consumer finance companies, and federal credit unions.

The summary, Chapter 1, states that in 1959 average finance charges on

consumer credit varied from $9 to $24 per $100 of credit outstanding. Smith attributes most of this variation to differences in operating expenses, which ranged from $3.30 to $14.25 per $100 of outstanding credit and varied directly with finance charges. The major factors in this cost spread are: (1) the method of acquiring business, i.e., directly from the public, or indirectly from dealers, (2) differences in the character of risks, (3) variation in the size of credit contracts, and (4) legal restrictions and, in the case of credit unions, subsidy by members and by employers. Also mentioned as of lesser significance are methods of financing, i.e., the use of equity or nonequity sources of capital, and variations in income tax provisions.

Smith then devotes a chapter to an analysis of the costs of each of the financial institutions. These four chapters are a valuable contribution to the subject. In Chapter 6 he compares these costs, and in a final chapter he reviews the trends in costs during the decade under review. A number of appendices analyze the sample and the data adjustments.

All this might seem to describe a straightforward study which has produced an empirically determined and comparable set of cost figures. But it should be pointed out that the figures have meaning only in terms of Paul Smith's definitions. The study has value for the expert, but an uncritical use of these figures in cost comparisons or in public relations might be misleading.

In a sense all of these institutions are multiproduct firms. For example, they might also sell insurance on the life of the borrower or on the collateral, or operate subsidiaries, or do a general banking business. Thus, certain more or less arbitrary allocations of overhead costs must be made. No fault is found with this, but rather with the more fundamental definitions.

Smith distinguishes between "operating" and "nonoperating" expenses. From a quarter to a third of the operating expenses are lumped together in an "Other" category; only salaries, occupancy costs, advertising, and "provision for losses" are identified. Here no adjustments are made for any variation in accounting methods of determining these expenses. Nonoperating expenses include the cost of nonequity funds, income taxes, and cost of equity funds. Smith includes income taxes as a cost, without going into the intricacies of shifting. Smith makes some heroic adjustments of credit union data to enable them to fit these categories. But actually, because of the element of subsidy—voluntary contributions of members and of sponsoring firms—it is almost impossible, in the context of a study such as this, to derive meaningful cost figures on credit union operations.

Perhaps the greatest objection can be taken to Smith's definition of lender's profit as a "cost of equity funds." Smith bases this on the economist's view of "cost as including 'normal profit,' which is defined as the return on capital that is essential to the retention of funds in the industry" (p. 2). But the concept of a "normal profit" as a cost assumes the neoclassical competitive model, and the consumer finance industry is the very opposite, an imperfect market with legal restrictions and price fixing and all kinds of monopoly elements. Under such conditions economists would probably look upon some of the "lender's profit" as rent. If with an increase in interest-rate competition, such as developed in the 1960's, this rent should disappear, then, in Smith's

sense, costs would decrease. This is not the usual meaning of a decrease in costs.

Apart from the "normal cost" concept, Smith makes little use of the economist's equipment. He uses only an average cost, supplemented by a range. Nowhere does a cost function appear. Models are not employed, although in certain cases models would have been useful in clarifying the discussion. The element of time—loan duration—is ignored, although this has no little importance in determining profitability.

Much has happened in consumer financing in the 1960's—the interest-rate competition already mentioned, and a liberalization of legislation. The findings of this volume, therefore, call for qualification in view of recent developments in this field.

JOHN T. CROTEAU

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Capital, Saving and Credit in Peasant Societies—Studies from Asia, Oceania, the Caribbean, and Middle America. Edited by RAYMOND FIRTH AND B. S. YAMEY. Chicago: Aldine Publishing Co., 1964. Pp. 399. $8.50.

Interdisciplinary undertakings, such as this book of anthropological-economic essays, have an entirely legitimate purpose. For their own good, specialists should be desegregated, at least from time to time. However, it is not always easy to hit upon the most fruitful and economical form of cooperation between social disciplines.

Economists and anthropologists both stand to benefit from visits to each other's workshops. Economists concentrating on problems of economic development couldn't even be good specialists if they did not keep one foot in anthropology. The question is how well they can be served by individual anthropologists trying their hand at economic analysis of their customary material, collected during researches "originally not directed to answering . . . economic questions" (p. 381). The answer which emerges from the volume under review is that they can be served suprisingly well—on condition that anthropological and economic competence are combined in the same man.

The 15 essays of the collection—not counting the prologue of the anthropologist-editor (Raymond Firth) and the economist's epilogue (B. S. Yamey)—have their origin in a symposium on economics and anthropology held in 1960 in Austria. The organizers chose capital, saving, and credit as a "band of topics" for discussion. It was up to individual authors to produce their own structured theme from this trio of juxtaposed concepts. As a result, the quality of contributions varies with the author's ability to formulate scientific issues within this vaguely demarcated area. The lack of a common focus has been further compounded by uncertainties as to definitions. A few discuss "capital" as holdings of consumer assets or money hoards; net accumulation of producers goods is sometimes confused with redistribution and concentration of wealth. Credit fatally ends up as a discussion of consumption loans floating on the periphery of problems of capital formation. Some authors also tend to overload the exposition with descriptive anthropological

detail, not enough for the economist bound for an advisory mission in a particular area, but too much for the general practitioner—unless he can make himself savor behavioristic descriptions, sprinkled with exotic indigenous nomenclature.

Despite these features which blur the economic analysis one can derive from the ensemble of the essays a fairly clear picture of the most interesting problems of internal capital accumulation in underdeveloped countries. In this respect, the essays supply a series of case histories, gathered in widely scattered places from Fiji through Guatemala to Malaya, on the social mechanism of economic stagnation. There seems to exist, in many instances, a self-contained system composed of primitive technology, kinship relations, status hierarchies, social values, and sanctions which cooperate in preserving the "stationary state" as a positive goal. The lesson for policy makers is that growth measures may sometimes entail not so much aid and assistance, as conflict and destruction of existing cultures. But then again there are cases where indigenous systems offer a surprisingly fertile soil for home-grown entrepreneurship on the capitalist model as, e.g., among the Tolai of New Guinea (cf. paper by Scarlett Epstein).

As in most similar collections, one finds in this book a wealth of specific insights and partial analyses of great value. There are studies of functional economic specialization along ethnic lines (cf. Alice Dewey's paper on Javanese marketing and Raymond T. Smith's on British Guiana). There is enlightening discussion of the inhibiting role of "non-economic variables" as, e.g. the absence of a climate of general contractual legality among people whose only effective social sanctions are based on narrow kinship ties. In several studies we came across interesting references to various leveling mechanisms which prevent a permanent accumulation of wealth by individuals, one of the potential sources of savings. This seems to be the function (or side effect?) of expensive ritualistic feasts and celebrations, as well as rules of inheritance and dowries (see the compact and well-organized study on Persian pastoral nomads by Fredrik Barth). Bert Hoselitz contributed a paper somewhat off the mainstream of the volume, on the validity and usefulness of statistical inquiries among Indian peasants.

The essay by Manning Nash (pp. 287-304) deserves a special mention because of its excellence. It is a comparative study of the relationship between economic behavior and general social structure, based on observations of two Central American Indian communities. The study commands respect because of the author's sovereign touch and economic competence in the application of social theory. His mastery of methodology makes some of the other papers look like undergraduate term papers and serves to confirm our suspicion that intredisciplinary studies can best be carried out by individual scholars equally at home in the disciplines involved.

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A History of Interest Rates. By SYDNEY HOMER. New Brunswick: Rutgers University Press, 1963. Pp. xvi, 617. $10.00.

The Homer of the ninth century B.C. and the Homer of the twentieth century A.D. were both immersed in an odyssey of 20 years. The former wrote of the wanderings of Ulysses, and the latter of the course of interest rates.

Sydney Homer's book is a monumental work covering the history of interest rates on every continent from 2000 B.C. to the present. While pursuing a career as an investment counselor, the writer was drawn into the study of economics. The research took over twenty years and resulted in a treasure of 73 charts, 81 tables, and 615 pages of text. The author indicates that there are great gaps in the factual data, especially in the late Greek, late Roman, Byzantine, and early Dutch periods. He hopes his compilations will be a starting point for future research. But no other single volume has this overwhelming store of facts and statistical information. This book is a distinctive addition to the publications in the monetary field, which area is already heavily laden with speculation and theory but sparse in historical and factual material. This book is an extraordinary accomplishment in graphics. The charts and graphs are artistically arranged; some graphs have a three-dimensional and architectural appearance (pp. 380, 381) which may become familiar features in the textbooks of the future, predicts Paul Einzig (*The Economic Journal*, March 1964, pp. 163-64).

The Homer journey from ancient Mesopotamia to Brezhnev's Russia reveals a fascinating panorama. In the vast flashback into history two European nations, Holland and England, stand out. The Dutch established a fine credit system to raise money for trade and for protective wars. Annuities and perpetual bonds were used. The people had such faith in government credit that interest rates dropped sharply, as low as 2 per cent; "modern easy money was discovered." This "Dutch Finance" was carried to England with William of Orange and there improved. Like the Dutch, the advanced nations of the West strive to maintain low interest rates in government financing. The prize for such achievement must go to England. According to the Homer statistics, England from 1800 to 1960 had on the average the lowest long-term rates, and was second in the short-term area. Holland did best in the short-term field (pp. 164, 178).

From the profusion of statistics about investment yields and interest costs, some generalizations emerge.

1. The interest rates of history tend to decline with the advance of civilization. "Long-term yields have tended to be low or lowest in those countries which have been politically, commercially, and financially the strongest" (p. 505). "The higher are a people's intelligence and moral strength, the lower will be the rate of interest" (p. 200). In the ancient world, rates from 10 to 20 per cent on good security prevailed, but over the centuries they fell to 5 to 10 per cent (p. 61).

2. "Interest rates seem to decline from earliest history until a period of late commercial development and later to advance during the final centuries of political breakdown" (p. 63). This theory, according to Homer, applies to the histories of Mesopotamia, Greece, and Rome. The theory might be appli-

cable to the British postwar experience of a rise in the long-term rate. Homer, however, suggests: "In England the recent higher yields have not been high enough nor have they lasted long enough to indicate that the trend has been reversed" (p. 505). Paul Heffernan, in *The Weekly Bond Buyer*, July 29, 1963, notes that in contrast to England, the financial experience of the United States presents an optimistic picture. "After a century of economic follies and retributions, one might expect by 1900 to view the shrunken and dilapidated wreckage of a hopeful young nation. But lo and behold! The erstwhile agricultural outpost had become a giant among industrial nations, almost ready to assume financial, economic and political leadership among the greatest nations of the earth" (p. 280). Many tables showing a decline in the interest rates support this paragraph. In fact, by 1946, one issue of long-term Treasury Bonds sold at 109⅝ to yield 1.93 per cent (p. 355). Obviously the United States can not be "written off," nor is she facing "the final centuries of political breakdown," if one accepts Homer's theory.

3. Homer shows that modern times reveal a shift from perpetual annuity to fixed maturity debt contracts. An important feature of the bond market of the eighteenth and nineteenth centuries was a substantial debt, private and public, in the form of perpetual annuities. The obligations were redeemable at the option of the debtor and the investor bought income. These debt contracts, which were so beneficial to the government, all but vanished in the twentieth century. They were replaced by debts of fixed maturity. The shift occurred at a time of relatively low interest rates. Some readers, like Heffernan, might consider this a paradoxical situation and question the wisdom of the public officials and the money managers. Homer gives no unique solution but gives direction. "Investors became maturity conscious; the early concept of a permanent annuity lost its appeal. Corporate creditors wished to retain control of their capital structure by call, or serial maturities" (p. 331). Refundings were always conversions at lower rates.

4. History reveals a decline in debt during periods of peace. It is noteworthy that despite the many decades of peace in the twentieth century the reduction in the public debt has been slight. This is at "variance with peace periods of other centuries" (p. 190). Homer states, "Europeans and Americans have become reconciled to a permanent national debt. They have come to measure its burden, not in terms of principal, but in terms of interest, its inflationary effects and the undesirable competition it provides to private borrowers . . . . A very large part of the national debt has been created at short-term and is held largely by banks and corporations who require short-term investments. Thus the government has a floating debt as a permanent liability. In this way, some distinction between short-term and long-term debt has been obscured" (p. 331).

The book is not primarily a history of interest-rate theories; nor is it an analysis of the causes of interest-rate levels and trends. However, Homer treats admirably the theories of the ancient world. He discussess the concept of interest, the equity of interest rates, the problems of legal limits. He delves into the theological disputes of the medieval world concerned with usury, just price, and loans for consumption or production. The interest-rate controversy

does not end with the Reformation, but now it continues in terms of justice and expediency (p. 81).

Homer does not explore deeply modern economic thinking about the theory of interest. Many readers, like Einzig and Heffernan, might wish that the author had not rationed his thoughts on modern interest-rate theories because the other background material is so well written, so incisive, and brilliantly analytical. The obvious reasons are the already considerable length of the volume and the desire to retain the unique factual data.

A serious student of finance and of money markets will find a veritable treasure of information here.

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Public Finance; Fiscal Policy

The Role of Direct and Indirect Taxes in the Federal Revenue System—A Conference Report of the National Bureau of Economic Research and the Brookings Institution. Princeton: Princeton University Press, 1964. Pp. xii, 321. $7.50; paper, $2.95.

To a greater extent than many Americans realize, the federal tax system remains the product of emergency decisions taken in the depth of the Great Depression, the years of New Deal "reform," World War II, and Korean fighting. Longer-run, rather than emergency, considerations, of course, have played a prominent role in tax revision during and since the war. Yet even with the major changes of recent years this country has by no means finished the job of adapting the tax system as well as reasonably possible to current and future needs. The conference whose papers are published in this volume represents one of several scholarly efforts to help build a solid foundation for further remodeling of the tax system. Incidentally, however, the discussion does not deal with existing federal excise taxes.

No brief review can do justice to a volume with four major papers, eight "comments," an introduction laying out the issues, and a summary of at least some of the discussion. Though oriented toward policy, the papers are not written for the public. Economists are presenting problems, evidence, and analyses to other economists. The starting point is the "optimum role of direct and indirect taxes in the federal tax structure." Authors generally agree that the direct-indirect distinction in itself offers little or no help in guiding policy. The meaningful distinction is between taxes on personal and corporate income—"direct"—and those on the production or sale of goods and services (excluding payroll and property taxes)—"indirect."

J. F. Due's brief historical sketch leads into a summary of current issues. The U.S. tax system has recently been criticized as relying too heavily on income taxes relative to those on consumption. The criticism results in part from a belief that our high income taxes have slowed economic growth, in part from recognition that income taxes cannot be adapted as readily as can commodity taxes to influence competitive position in world markets. Moreover, compared with consumption taxes income taxes as they exist in real life

are not so superior, on equity grounds or for effects on resource allocation, as is widely assumed.

A. C. Harberger's paper, "Taxation, Resource Allocation, and Welfare," among other things, presents a relatively simple geometrical explanation of the theory of the welfare cost of both excise and income taxes. A more elaborate mathematical treatment follows. The author then estimates the "welfare costs associated with the distortion of the labor-leisure choice by the personal income tax" at possibly $1 billion a year (without allowance for the 1964 rate reductions). Comparing this with the welfare cost of an excise tax yielding the same amount, he concludes that the case for the personal income tax "remains comparatively unscathed" if the excises have "relatively high rates and do not strike a very high proportion of consumer expenditures." The personal and corporation income taxes lead to other allocative losses, e.g., relative undertaxation of owner-occupied housing. Harberger estimates the possible effects of tax changes on saving and economic growth. He concludes that the most we could hope for would be an increase of from .10 to .20 of a percentage point in the annual rate of growth. Both E. C. Brown and W. Fellner in their comments point to a wide range of uncertainty in the assumptions. The conclusions would be appreciably different if "other reasonable numerical guesses" were substituted. If I understand the paper and the comments, all three economists agree that present income taxes have allocative and equity defects and that some (marginal) shift to consumption taxes could lead to better results by permitting reduction in the high income tax rates which have the most distorting effect.

R. A. Musgrave and P. B. Richman in "Allocation Aspects, Domestic and International," examine the possible results of numerous kinds of change. The retail sales tax, individual income tax, corporation profits tax, and value-added tax (in two versions, income and consumption) are compared. Various combinations of assumptions naturally lead to different conclusions and leave the reader perhaps confused but certainly convinced that the authors have produced a reference source to which one will want to turn as the issues are discussed in the years ahead. The value-added tax scores well in comparison with the corporation income tax. Judgment of the international aspects of the alternatives also involves the "juggling of many balls," including not only the taxes themselves but also factor mobility with and without commodity flows, the nature of export rebates and import tax compensation, and exchange rates. Empirical material compares profits taxes on business in the United States with those in certain other countries, with allowance being made for differences in depreciation and other factors often overlooked. After the changes of 1964, our rate on undistributed profits will be about in line with the burdens of most other countries but often, though not always, greater on distributed profits. C. S. Shoup's comment deals with aspects of a shift from income to value-added taxation. L. B. Krause concludes that the balance of payments will benefit from a substitution of indirect for direct taxation.

"Equity, Administration and Compliance, and Intergovernmental Fiscal Aspects" by D. H. Eldridge is another paper packed with evidence and ideas. It contains a realistic discussion of problems of estimating the distribution of

al excise tax as it might exist with the personal and corporation taxes as they do exist; costs of administration and compliance of existing and possible taxes; and the possibility of increased federal-state-local conflict and harmony from greater federal use of consumption taxes. Eldridge concludes, as does H. E. Brazer in his comment, that on equity grounds the personal income tax stands clearly superior to excise taxation. The corporation tax is another matter. R. B. Welch draws upon long experience in tax administration to say "many of our most baffling problems in sales tax administration arise out of the fact that the tax . . . is short on underlying philosophy." A value-added tax offers prospects of improvement, in part from the opportunity of starting afresh with more deliberate preparations.

O. Eckstein and V. Tanzi compare the tax structures of Europe and the United States from the point of view of implications for economic growth. Considering taxes imposed by governments at all levels, our (pre-1964) direct taxes are greater in relation to the total than is the case in the other countries. But differences in growth are the result of many factors, including the allociation of invesment, e.g., residential and nonresidential. Eckstein finds several potential advantages of value-added taxation over the corporation income tax. F. Neumark points out that the distinguishing feature of German experience is not so much the balance between direct and indirect taxes as the use of selective tax abatements to channel investment in certain directions. D. T. Smith also emphasizes that the nature of particular taxes must be considered no less than the balance between direct and indirect.

In a final summary S. B. Chase, Jr., reports that the proceedings "provide little support for a major move in the direction of commodity taxation and away from income taxation." Yet there was some tendency to look with favor on substitution of a value-added tax for the corporation income tax.

This is an excellent volume. It will not be of much help in debating how to remodel our present federal excise taxes. It will, however, long be useful for studying problems which are likely to be with us a long time.

C. Lowell Harriss

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Government Fiscal Activity and Economic Growth in Japan, 1868-1960. By Koichi Emi. Economic Research Series of the Institute of Economic Research, Hitotsubashi University. Tokyo: Kinokuniya Bookstore Co., Ltd., 1963. Pp. v, 186.

This is a welcome addition to a growing body of quantitative case studies dealing with the long-term growth of government expenditures in various countries. Beginning with the pioneering study by Solomon Fabricant for the United States, there are now about a half-dozen studies completed and several others underway.

Koichi Emi points out in the Preface that he "is particularly interested in the dynamic relations of government fiscal activity to the growth of the whole economy. . . . This book, however, emphasizes only the statistical comparisons which must underlie the theoretical analysis of these relations and makes no attempt to provide such an analysis at this stage." He hopes to present in

the future the theoretical analysis of public expenditures and the revenue aspects of fiscal activity.

Basic statistical tables in the Appendix comprise about 46 pages of the book, and the textual discussion with charts and tables occupy the rest of the book, about 140 pages. The text presents summaries of statistical findings, and most of the discussion is a description of what the charts and tables try to show and a historical account of the changes in governmental and budget organizations relevant to the understanding of changes in government expenditures. It is difficult to summarize briefly the numerous bits of findings, most of which are scattered here and there. The more significant ones are as follows: In Chapter 1, Emi shows that the number of central government employees rose both absolutely and relatively to the total labor force, and the government-owned capital goods rose relative to the national wealth. In Chapter 2, it is shown that government expenditures (both central and local governments) increased from 11 per cent of GNP in 1880 to 56 per cent in 1950 and to 33 per cent in 1960. In Chapter 3, military expenditures before World War II are shown to be extremely high but fall to a low level (about 5 per cent of total government expenditures) in the postwar years. In Chapter 4, total government expenditures are divided into expenditures on current goods, expenditures on capital goods, and transfers and subsidies. The latter two tended to rise more rapidly than the first. Total government expenditures are also broken down into functional groups: administration, economic, and social; the share of the last two increased much more rapidly than the share of administration in total government expenditures, owing mainly to sharp reductions in military expenditures. This chapter ends with a discussion of subsidies which are claimed to be "not very large in absolute amounts." Chapter 5 contains a valuable description of changes in the organization of the central and local governments and the system of special accounts. In the last chapter, the author describes the role of the central government as a modernizing agent during the first half of the Meiji period—in providing for the transition from feudalism, in introducing science and technology, and in the promotion of education.

Although there is little that is particularly new in the foregoing findings, it is reassuring that Emi's study supports some of the earlier investigations. In a brief review, it is impossible to discuss in detail the expertly drawn charts and tables of which there are nearly two hundred. Generally speaking, the sources of the data plotted in the charts are not specified and for many of the tables, especially the more interesting ones on pages 5, 7, 30, 63, 67, 91, 97, 104, and 131, the citation given is simply "estimated by the author." One leaves the text of this book with a feeling of disappointment that the author did not address himself directly to the testing of hypotheses dealing with the causes for the increase in government expenditures, both per capita and as a percentage share of GNP. Without an understanding of these causes, it is very difficult (1) to foresee whether in the future government expenditures in the advanced countries will continue to rise or decline, and (2) to indicate even approximately what should be the priorities and patterns of government expenditures for the underdeveloped countries in their efforts to develop their economy.

The statistical appendix contains seven basic tables. A study of these indi-

ıtes that Tables A-3 and A-4 attempt to eliminate the duplications between ccounts, in order to obtain a consolidated total of government expenditures. ables A-2 and A-7 present functional, and Table A-6 economic, breakdowns f total expenditures. Table A-5 gives a classification of expenditures by levs of government, i.e., central, prefectural, cities and villages. I do not underand what Table A-1, entitled "Comprehensive Table," purports to do. olumns (1) and (2) give Ohkawa's figures of national income and GNP, nd these for the most part are rather obsolete, significantly overstating the rowth of the Japanese economy in the decades before 1920. From Column 3), Total Government Expenditures, are deducted Military Expenditures, National Dept. (which is probably a misprint for National Debt and which acually stands for interest on the national debt), and Capital Expenditures Columns 4, 5, 6), to obtain Other General Services, Column 7. This seems o me a misnomer; the residual is better designated as nonmilitary, current xpenditures.

It comes as a surprise that these basic tables are constructed from a host of econdary sources. In a basic statistical study of government expenditures, ne investigator should as much as possible go to the settled accounts and ven to the budget documents for further details. If this is not done, it is alnost impossible to eliminate the numerous intergovernmental flows which onstitute duplications; nor is it possible to get sufficient details to reclassify dequately the expenditure flows into functional and economic groupings. Iany of the defects in the tables are traceable to Emi's reliance on a number f secondary sources, not to his capability in the field of national accounting, hich is of a high order, as shown in every chapter of the book and especially ı Appendix 1. Because of his failure to use primary sources, he runs into nany problems; as in eliminating duplications (see, e.g., the confusing foototes to Tables A-3 and A-4); his failure to supply *detailed* definitions of unctional and economic groupings which go to make up his broader categoies in Tables A-6 and A-7; there are four different "total government expendures" (in Table A-1, A-5, A-6, and A-7), and the reasons for the differences re not clear from the footnotes to the tables; inexplicable big jumps and delines in his time series. (I have found that the big jump of about one-third ı total government expenditures from 1878 to 1879 is due to the failure to nclude local government expenditures from 1868 to 1878.)

Despite the foregoing limitations, the statistics compiled in this book and he historical information presented in the text are significant contributions to he study of government expenditure. The data in this book are sufficiently eliable for most purposes, when used with due care and caution.

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International Economics

The Theory of Trade and Protection. By WILLIAM PENFIELD TRAVIS. Harvard Economic Studies Vol. 121. Cambridge: Harvard University Press, 1964. Pp. xii, 296. $6.25.

Mr. Travis' aim is to extend the Heckscher-Ohlin theory of international specialization by incorporating considerations of protection in the framework

of this theory and indicating the distortions in resource allocation caused by the existence of trade barriers. In doing so, he provides a new formulation of the factor-price equalization theorem, as well as a reinterpretation of Leontief's results.

In reformulating the factor-price equalization theorem, Travis makes use of the concept of the equalization region which he derives by utilizing the theorem of corresponding points put forward by Kelvin Lancaster. According to the latter, factor prices are equalized if production takes place at "corresponding points"; in turn, all possible divisions of world factor supplies, for which corresponding points exist, make up the equalization region. Thus, whenever it can be shown that we are within the equalization region, factor prices will be *ipso facto* equalized.

But are the conditions for factor-price equalization fulfilled in the real world? Travis believes that some of these—such as the homogeneity and identity of production functions throughout the world, and the requirement that the number of products exceeds the number of homogeneous factors—are approximately fulfilled, and the nonfulfillment of several others (e.g., the absence of transportation costs) affects the results but little. Thus, while admitting that specialization would occur if endowment ratios of capital and labor differed very much, Travis considers the equalization region to be an operational concept.

The conclusions reached in regard to the factor-price equalization theorem are subsequently utilized in examining the interpretations and explanations offered with respect to the Leontief paradox. In Travis' opinion, "none of the various explanations provides a completely satisfactory solution; nor can these explanations be combined to dress a list of influences all working together to cause the phenomenon Leontief observed" (p. 111). Instead, Travis offers an alternative in the existence of protection. He asserts that "protection . . . amounts to a new explanation of the Leontief paradox, which in turn offers a powerful method for measuring the effects of protection" (p. 171). As long as the assumptions of factor-price equalization are approximately fulfilled, the only reason for the Leontief test to imperfectly represent the factor endowments of a country is the existence of protection, and, correspondingly, a comparison of actual factor shares and those obtainable under free trade provides a measure of the effects of protection.

In this connection, two questions arise. First, can one accept Travis' conclusions in regard to the realism of the assumptions of the factor-price equalization theorem? And, second, can the effects of protection be powerful enough to provide for a "reversal" of factor intensities in a country's trade, as indicated in the case of the United States?

To begin with the second problem, it is easy to see that a combination of tariffs and subsidies can always provide the desired structure of trade for a particular country—assuming that others do not retaliate. This is indicated by use of a numerical example, when a combination of an export-subsidy on the labor-intensive commodity and an import-subsidy on the capital-intensive product is shown to make a labor-poor and capital-rich country to export the former and import the latter (pp. 176-80). But is this a realistic example? Under present-day conditions countries generally employ tariffs to protect

their industries, export subsidies are rarely used, and import subsidies hardly ever. At the same time, it appears questionable that the above result could be obtained if tariffs were the only means of protection applied.

If the United States, for example, has a comparative disadvantage in producing manufactured goods, as Travis alleges, would the existing system of protection lead to the exports of manufactures? The present reviewer has doubts about this possibility and suggests that the explanation may indeed lie elsewhere and may be related to the first question mentioned above.

We have noted that, in Travis' opinion, we hardly falsify reality by assuming that production functions are homogeneous and identical in all countries. Regarding the postulate of homogeneity, he asserts that "recent work indicates that aggregate production functions are in fact homogeneous of degree one. This affords presumptive evidence that either component production functions are also homogeneous of degree one, or that the importance of those that are not is small" (p. 9n.). As supporting evidence, he cites the work of Arrow and others on the constant substitution-elasticity production function as well as the conclusions reached in regard to the constancy of factor shares (pp. 9, 175). Yet Arrow *et al.* purported to show that production functions are *not* linear homogeneous, and the evidence provided in connection with the constancy of factor shares has also been challenged. Further, the existence of increasing returns to scale has been indicated in an intercountry comparison by Chenery, as well as in a reinterpretation of the temporal aggregate production function for the United States by Walters.

By reason of the assumption of the homogeneity of production functions, Travis excludes the possibility that internal and external economies could have an influence on international specialization. Neither does he accept the view according to which differences in efficiency have something to do with the pattern of trade, although his counterargument is phrased in terms of a long-term adjustment process. He asserts that "while international differences in efficiency coefficients may exist in the short run, Darwinian competition would eliminate them in time . . . " (p. 37).

Armed with these assumptions, Travis finds it "surprising that the United States devotes a larger, and France a smaller, proportion of its labor force to the manufacturing sector, whose products on the average tend to be more labor-intensive than those of the rest of the economy" (p. 109). But this result can be understood if one takes account of the superior efficiency of U.S. manufacturing and the greater importance of large-scale economies in the manufacturing sector, as compared to agriculture, mining, and services. Protection can now be introduced as a reason for the market limitations facing French industry but, without invoking increasing returns, it can hardly provide an explanation.

These criticisms are not intended to detract from the value of Travis' contribution to the theory of international trade. By the ingenious use of the concept of the equalization region, he has indeed succeeded in extending the Heckscher-Ohlin theory and has indicated that "protection and relative factor endowment play similar logical and quantitative roles in determing trade flows" (p. 163).

But Travis appears to have been less successful in healing the alleged split

between theory and empirical research (p. vii), and the lack of realism in some of his assumptions affects not only the empirical results but his policy recommendations as well. The twin assumptions of the homogeneity and international identity of production functions have led him to conclusions in regard to the trade problems of less developed countries which many a reader may not be prepared to accept, for example. In Travis' opinion, "the restrictive commercial policies of the landed nations [of North America and Western Europe] oblige the proletarian [underdeveloped] nations to employ nearly their entire labor force in an attempt to secure food," and "the proletarian nations themselves, by protecting industry, prevent their own industrialization" (p. 245).

BELA BALASSA

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The Pure Theory of International Trade. By M. C. KEMP. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. viii, 324.

In this book, Professor Kemp has put together some of the more fundamental tools in the theory of international trade. The subjects covered in the book are by no means exhaustive, but most of the topics discussed in the past ten years or so are touched on or some references are given to them. It consists of four parts, except for a brief introduction and the final chapter listing relevant literature on the econometrics of international trade.

Part II begins with an introduction of the two-commodity two-factor model of a closed economy, where two goods are produced by two factors of production with production processes subject to constant returns to scale and a diminishing marginal rate of substitution between the two factors. The author gives an elegant algebraic treatment and derives several formulas which will be effectively utilized in later sections. The model then is extended to the case where the economy is confronted with the possibility of trading with the rest of the world, with given terms of trade. It does not differ from the ordinary model of the Heckscher-Ohlin type in terms of the basic premises, and the results obtained here do not go much beyond those in Harry Johnson's now classical paper, "Factor Endowments, International Trade, and Factor Prices" (*The Manchester School of Economic and Social Studies,* Sept. 1957, *25,* 270-83). However, the author has succeeded in putting the basic relations existing between various variables of the model into explicit algebraic formulas, and they enable the author to deal with the problems of the impacts of technical changes upon the patterns of domestic production, the implications of changes in the terms of trade and of tariffs. Chapter 4 discusses the model of two trading countries each having a two-goods, two-factor structure described in the previous chapters. The Lerner-Samuelson principle of factor price equalization is described with an outline of the proof, which is in Appendix to Chapter 4 extended to the general n-goods, r-factors case. It may be noted that the proof outlined here for the Lerner-Samuelson principle is not entirely justified, although it can be shown to be valid for the two-goods, two-factor case. The stability of such a model is discussed in Chapter 5. Chapter 6 describes the impacts upon the trade equilibrium of changes in

basic variables such as tariffs, demand functions, the balance of payments, factor endowments, or technological conditions. The rest of Part II is devoted to the problems of variable factor supply, increasing returns to scale, labor migration, and cost of transportation.

The first chapter in Part III is concerned with the problem of the gain from trade in the static context. It basically employs the technique introduced by Lionel McKenzie in his "Equality of Factor Prices in World Trade" (*Econometrica*, July 1955, *23*, 239-57, a reference conspicuously absent from the list of references to the chapter). The author then goes on to discuss the gains from trade in the dynamic context (with a model of what might be international capital movements) and a sketch of trade warfare.

The discussion of the international monetary mechanism in Part IV is primarily based on the model which is defined in terms of demand functions depending on prices of two goods and the difference in the desired level of cash balance and the actual cash balance. The effects of devaluation, both in the long run and short run, are discussed in detail for the case of fixed exchange rates, while the analysis of flexible exchange rates is fragmentary. The adequacy of such a model in analyzing international monetary phenomena is seriously questioned, in particular in view of the absence of the rate of interest (as distinct from the rate of returns to "capital"), and a model of the Keynesian type, like those recently developed by Robert Mundell and others, would seem much more appropriate.

Part V takes up the problem of trade between underemployed economies with a particular emphasis on the price rigidity.

On the whole, Kemp has remarkably succeeded in this book in developing analytical tools to handle the Heckscher-Ohlin model of international trade and in applying them to solve various problems of interest in the theory of international trade, but he has been less successful in handling the problems of international monetary mechanisms, although the algebraic techniques he has employed seem to be easily modified to take care of more complete models. The problems given at the end of each chapter are not only of some interest by themselves but also they serve as excellent exercises for students both in international trade and mathematical economics.

Hirofumi Uzawa

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Trade Prospects for Developing Countries. By Bela Balassa. Homewood, Ill.: Richard D. Irwin, Inc., for the Economic Growth Center, Yale University, 1964. Pp. xii, 450. $9.50.

One of the most vital questions of our times is whether the future foreign exchange receipts of the newly developing countries will be sufficient to enable them to finance the imports required to achieve relatively substantial rates of economic growth. By providing us with a quantitative assessment of this question in the form of projections of the trade and payments prospects of the developing countries for 1970 and 1975, Bela Balassa has rendered an invaluable service to economic policy-makers and practitioners in both developed and underdeveloped countries.

Using his projections of "most likely" or of "high or optimistic" target levels of income in the world economy in 1970 and 1975, Balassa estimated that the current-account deficits of the developing countries would rise from a total of $4.6 billion in 1960 to $9.4 or $10.5 billion in 1970 and to $11.3 or $13.7 billion in 1975. With the net flow of public and private capital to the developing countries projected by him to reach $11.6 billion in 1970 and $13.7 billion in 1975, he concluded that the capital inflow would be more than sufficient to finance the projected payments deficits.

Alternatively, assuming the realization of the projected targets levels of income in the developing countries and the most likely levels in the developed countries, he estimated that the current account deficits would be $12.0 billion in 1970 and $16.6 billion in 1975, which would be in excess of the projected capital inflows. The $12.0 billion estimated deficit just noted is of particular interest, for it is substantially below the $20 billion deficit projected for 1970 on an aggregate basis by the United Nations in Part I of its 1962 *World Economic Survey*, which was prepared as a background paper for the U.N. Conference on Trade and Development held in Geneva in the spring of 1964. The U.N. estimate, it may be noted, was predicated upon income assumptions similar to Balassa's, which would result in the fulfillment of the goals of the U.N. Development Decade ending in 1970.

The difference of $8 billion in the estimates, which consisted of $5.2 billion on the trade account and $2.8 on services, was attributed by Balassa in part to the drawback of the aggregate method of projection as compared to the commodity method which he used. This was especially the case insofar as the aggregate method could not take account of such structural changes as his projected decline in the self-sufficiency in nonfuel minerals and metals in the developed countries and a shift towards the importation of processed metals (p. 96). But without additional research, it unfortunately cannot be determined how much of the excess of Balassa's estimate of export earnings over that of the United Nations ($31.2 billion as compared to $29.0 billion) can be traced to structural or to other considerations. It is also not possible to establish the reasons for the difference between the $41 billion U.N. estimate of the 1970 imports of the less developed countries and Balassa's own estimate of $38 billion. As far as the discrepancy between the two estimates of the deficit on services is concerned, Balassa attributed it in part to an overestimation of the deficit by the United Nations in the base year of its projections (p. 105).

Given such wide and unresolved differences as have been noted in the foregoing projections, great interest obviously attaches to the methods underlying the projections. Balassa treats these questions of method in an appendix to his first chapter. As noted, he opts in favor of the commodity method of forecasting because it can take account of technical changes and of changes in the commodity composition of trade. He concludes from this last point in particular that individual commodity projections, when aggregated, will result in a smaller forecasting error as compared to the outright use of the aggregate method which entails the extrapolation of import demand functions for large aggregates of commodities (p. 346). But while Balassa's line of argument

here may be correct, he gives the impression that the merits of a particular method of forecasting can be established on a strictly a priori basis without the need for comparing the results of alternative forecasting methods with actual events. In any case, since he was interested in estimating the trade prospects for individual SITC commodity groups according to region of origin, he had no choice but to use the commodity method of projection.

Whether one accepts Balassa's projections thus depends a good deal upon the acceptability of his underlying assumptions about commodity markets, technology, and policy conditions, and the calculated relationships which form the basis of the projections. On these matters, Balassa rates a high score indeed. His commodity export projections, which form the core of the work in seven of the book's 12 chapters (pp. 131-339) and the bulk of the 76 pages of the statistical appendix, were very carefully and skilfully prepared and were based upon data of various kinds drawn mainly from the 1950-60 period. However, while the resultant projections are of very considerable interest, their accuracy will perhaps be vitiated owing to Balassa's inability, because of the lack of information, to take supply considerations fully into account and to deal with the interdependencies in some of his commodity classes. Moreover, because of his emphasis upon regions rather than countries, the usefulness of his export commodity projections is not entirely clear in those cases in which one might wish to disaggregate down to an individual-country level.

But the parts of the projections which give the most pause for reflection are those that deal with the future import requirements of the developing countries, the estimation of their service items, and the projection of their private capital imports. Even though Balassa's treatment of these matters appears eminently reasonable, it must be emphasized that his estimates rest mainly upon the experiences of the developing countries in the 1950's. To the extent that these experiences will be altered in the years to come, the relationships derived from data for the 1950's may therefore become increasingly inapplicable for forecasting purposes.

If, however, we nevertheless accept Balassa's estimates of the payments gap, at least tentatively until better alternatives are available, some very important implications follow. The export and foreign investment prospects for developing countries which produce minerals and metals are substantially brighter than the prospects for producers of foodstuffs and agricultural raw materials that are grown in or that compete with substitutes in the developed countries. As a consequence, the projected payments gap may be more a question of the regional distribution than of the over-all adequacy of the foreign exchange receipts of the less developed countries. In view of the foregoing considerations, there is a vital need on the part of the industrialized countries and the Soviet-type economies to liberalize their import trade, especially with regard to manufactured goods that can be produced efficiently above all in the Asian countries which are not favorably endowed with minerals and metals.

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Economic Development, Payments Deficit, and Payments Restriction. By I. K CHATTERJEE. Geneva: Librarie E. Droz., 1963. Pp. 168. F 20.

This book discusses the following question: If a country is interested i economic development, is it better off to pursue an international economi policy of free convertibility—currency convertibility at fixed exchange rate and relatively free trade—or a policy of restriction—exchange control an quantitative controls on imports. Several themes appear in different context throughout the book. Convertibility fosters competition and therefore efficien cy, while restriction fosters monopoly, and the monopoly profits may encou age investment. Convertibility subjects the domestic economy to shocks orig nating abroad, while restriction insulates the economy from foreign shock and produces more certain results. Chapter 2, "Convertibility School vs. Re strictionist School," discusses these points plus a number of points of lesse importance.

Chapter 3, "Conditions of Convertibility," stresses the importance c domestic monetary stability and an investment in international reserves fo the maintenance of convertibility. Succeeding chapters discuss the concept c structural disequilibrium, which the author feels is so vague as to be pract cally useless, and the proposition that inflation promotes growth, a proposi tion which the author regards with skepticism. The chapter on balance growth (Chapter 6) concludes that the concept is essentially vague. If bal anced growth means that production should follow the growth in demand c specific consumer goods, then the concept of balanced growth has added noth ing to existing theory if such production is profitable; if domestic productio of certain consumer goods is not profitable, then the plea for balanced growt is a plea for the misallocation of resources. Furthermore, an attempt to bal ance domestic demand for every good by domestic production can only lea to the closure of the economy.

Problems connected with terms-of-trade losses are discussed in Chapter 7 The statistical problems of measuring the elasticities in foreign trade are dis cussed as well as the significance of any deterioration of the terms of trad that may occur. A terms-of-trade loss is often the unavoidable result of shift in the foreign demand for exports or of domestic growth which increase the output of export industries. The terms of trade is not a good guide to th allocation of resources. Export production should be expanded if neede goods can be obtained more cheaply through trade than through domesti production; any terms-of-trade loss resulting from higher exports is irrele vant. Attempts to avoid terms-of-trade loss through restrictionist policies re tard the movement of resources in response to changes in demand, and th restrictions discourage private-capital inflows.

Finally, there is a chapter applying the general arguments to the case c India. Balance-of-payments problems have been aggravated by inflation. Re strictionist policies and emphasis on production for the home market have dis couraged exports, thus making it difficult to import goods which cannot b produced domestically. Some industries have been forced into idleness throug a lack of foreign exchange to purchase replacement parts for machinery an even a lack of raw materials.

Although the analysis presented has few flaws, this book does little mor than pull together arguments that have been stated elsewhere, and I. K Chatterjee does not always restate the arguments with clarity. There is a dis tressing lack of attention paid to the peculiar circumstances of underdevel oped countries. To illustrate points in the argument, the author frequentl refers to the German recovery from World War II. It may be that the devel opment problems of Germany and India are essentially the same, but there i a presumption to the contrary in much of the current literature on develop ment.

Chatterjee seems to favor free markets although his point of view is fre quently unclear. But free markets cannot produce development without ente prise and individual initiative. Free markets will not produce developmen when the savings of the populace go into gold hoards, temples, and monu ments rather than into educational facilities, irrigation projects, and machin ery. Neither is planning necessarily the solution. Planners may not demor strate any more enterprise than private individuals. Furthermore, controls, a Chatterjee points out, may not be effective and may promote corruption i government.

The convertibility vs. restriction issue is clearly a part of the larger issue (free vs. planned economies. While there are important issues concerning th effects of planning on individual freedom and state power, the purely econon ic issue is an empirical one: under what circumstances will planning produc more rapid growth than will free competition? It is surely possible that plar ning is more appropriate for some countries than others, and for some indu tries than others. Chatterjee seems to recognize this point since on the fir page of the book he says (p. 11) that "freedom from ideological bias and . . pragmatism" mark the economic thinking in underdeveloped countries. Th job of the economist, then, would seem to be to specify the conditions und which planning is preferable to free competition, and vice versa, and to devi tests to identify the existence or nonexistence of the specified conditions. Bu after espousing pragmatism, Chatterjee says that "economics does not perm of any experimental verification and hence the position of neither school ca be decisively proved or disproved" (p. 27). Throughout the book statemen appear to the effect that empirical investigation is impossible. Empirical i vestigations are certainly not easy, but I suggest that empirical work will, this stage, prove more valuable than another book on the theory of the rel tive merits of planning and free markets for economic development.

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The Dollar in World Affairs: An Essay in International Financial Policy. I Henry G. Aubrey, New York: Frederick A. Praeger, 1964. Pp. xii, 29 $2.25.

Dr. Aubrey's *The Dollar in World Affairs* provides a panoramic view U.S. domestic and foreign affairs and the role of the dollar within this bro setting. Ways of maintaining the strength of the dollar and an acceptable ba ance-of-payments position are considered within the closely interrelated co

text of foreign aid to underdeveloped areas abroad, the U.S. money and capital markets, domestic unemployment, growth and price stability.

The book is divided into five major parts. Part I, "Financial Policy and Foreign Relations," presents a general background to the subject matter, indicating America's increasing and irreversible involvement in world financial affairs. Part II, "The Strength of the Dollar," focuses attention on the nature of the external deficit as related to various components of the balance of payments. Part III, "The Dollar as International Money," considers the role and problems of the dollar as a key currency, as well as current proposals for strengthening the international monetary order.

The credit function of the dollar is analyzed in Part IV, "The United States as an International Capital Market." As the major financial center of the world, the United States is restricted in its possible action to alleviate the balance-of-payments deficit. In Part V, "The Outlook for American Foreign Financial Policy," the author integrates the many facets of U.S. foreign financial involvements in an effort to suggest policy measures within a broad frame of reference.

Aubrey is critical of a number of prevalent attitudes and proposed actions. A basic inadequacy he finds is the failure on the part of many persons to make the distinction between the credit function and the payment function of the dollar, each of which requires a different order of policies. He stresses the unreliability of the conventional "liquidity ratio" (gold and foreign exchange in relation to world trade) when used in the absence of an investigation of such factors as the stability of world trade and the distribution of reserves. He opposes U.S. policy on tied aid and an interest equalization tax. On a broader scale, he dismisses as impractical solutions such policies as increasing the price of gold or adopting floating exchange rates.

The author concludes that the international monetary structure should not be subjected to sweeping schemes of reform; instead, an evolutionary approach, with a series of small changes, should be taken to secure gradually a monetary order capable of meeting existing and new needs in the world economy. For example, he does not believe that an assessment of the ultimate merits of centralized monetary reserves and policy is adequate; equally important is the *rhythm* of change in pursuit of such an objective. A radical departure is out of the question; a gradualistic approach with an eye to political acceptability may be an alternative.

In terms of specifics, Aubrey notes the need to recognize that a small and undramatic increase in the trade balance would eliminate the U.S. balance-of-payments deficit within a few years. As a short-run measure, the U.S. government might seek to fund some of its externally held liabilities into longer-term instruments. The IMF could accept, on a limited basis, deposits of national currencies in exchange for a type of international script. Existing reserves should be used in a routine fashion, rather than as a last resort. Finally, any limited international central banking—as might develop with the European Common Market—should be watched as a guide to what might be done eventually on a global basis. In general, long-run objectives must be sought on a cooperative basis with the governments of foreign countries.

This essay is valuable in at least two ways. It presents a complex set of issues in a lucid fashion; the author has undoubtedly succeeded in his goal of making financial issues intelligible to those who are not specialists in international finance. Second, it analyzes the issues within a broad framework of over-all foreign policy. One result of his approach is that certain sections of the book tend to become somewhat repetitious; this is not, however, a serious drawback in view of its success in showing that the technical aspects of international finance are a counterpart of live and tangible foreign affairs issues.

Aubrey's chief contribution is not one of offering novel ideas or solutions, but rather of analyzing, evaluating, and suggesting remedies in international finance against a broad setting of relevant circumstances. It becomes obvious that there are no simple, quick solutions, that a wide range of factors must be taken into consideration in any policy decision, and that measures to strengthen the dollar cannot be pursued in isolation. In this reviewer's opinion, Aubrey's closing statement represents a forceful guide to the policymaker:

> . . . the strength of the dollar in world finance is linked to the role of the United States in world affairs. It cannot be stronger—and need not be weaker—than the purpose this country has set itself in the world; and while these tasks—toward the less developed countries, the Communist bloc, and within the Western community—are acknowledged as the West's common objectives, the dollar will serve as a common, not just a national, financial instrument (pp. 262-63).

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Trade Relations Between the Common Market and the Eastern Bloc. By JOHN P. DE GARA. Brussels: College of Europe, De Tempel, 1964. Pp. 103.

According to John de Gara, trade between the West and the European satellites helps to encourage greater independence among the latter countries and serves as a means of stimulating a gradual change in them. The author bolsters this hope with a set of statistics which bring into focus the development of trade relations between the European Common Market and Eastern Europe, excluding the Soviet Union.

As a background to East-West trade relations, De Gara shows that the early Communist conviction in the inevitable doom of the capitalistic system and their use of trade to hasten this process and to exploit the inherent contradictions between capitalistic countries became demonstratively shaky in the light of actual experience. The recent trade offensive of the more pragmatic Communist regimes has been conducted on the premise that trade with the West is a prerequisite for preserving peace. In this way assertions of a propaganda nature veil the self-interest which presumably is their real reason for entering into trade relations with the West.

The author's outline of Communist views on European integration is quite interesting. He states that the ideological blinkers worn by the Communists prevented them from perceiving in time the real significance of the economic integration of Europe. Expecting momentarily to become spectators of the last convulsions of capitalism, they had no inkling that unification, instead of

bringing out the inherent contradictions, would bring new strength. Once their oyes opened—as late as 1960—they were quick to decry the unification as highly undesirable. Their arguments rested on the long-standing Communist stress on national sovereignty, which seems to this reviewer an argument used for the sake of its general appeal but not one in which the Communists seriously believe. De Gara himself finds in it an example of Communist "doublethink": While under conditions of capitalism economic integration amounts, according to the Communists, to a surrender of national sovereignty and brings about exploitation, the same economic integration under communism "reflects . . . the real friendly relations, equality and cooperation which exist between the people building a new way of life" (p. 27).

With reference to Western views on trade with the Communist countries, De Gara notes the already seriously eroded American notion that not trading with the Bloc would weaken it, and the Battle Act which is designed to exclude from Western exports the so-called "strategic goods." The author's sympathy rests obviously with the British view, increasingly widely adopted, that insofar as trade with the Communists helps to make them more prosperous, it makes them also more tractable. The wealthier a country, the greater its stake in preserving peace—prosperity becomes a tranquilizer of the revolutionary Communist ardor.

The empirical part of the book consists of a review of trade statistics for the period 1958-61. It is noted that trade with the Eastern bloc represents less than 2 per cent of total foreign trade of the Common Market countries, as compared with about 10 per cent of the Eastern bloc. Germany is the most important trading partner on the Western side, with Italy expanding its trade most rapidly. Poland is the most important trading partner on the Communist side, with Rumania expanding its trade most rapidly, a trend which has continued into 1964. East Germany shows more trade with the West than Poland if intra-German trade estimates are included in the statistics—and it also, like Poland, shows a relative stagnation in its trade turnover with the West. And, not surprisingly, exports of the Common Market to the Eastern bloc consist to a higher degree of industrial goods than its exports to the world as a whole, while imports into the Common Market from the Eastern bloc show a higher proportion of agricultural products than its imports from all foreign countries.

In discussing the prospects for trade with the European satellite countries De Gara correctly asserts that the common external tariff of the Common Market is unlikely to have in itself far-reaching effects on its relations with the Eastern bloc. Abolition of internal tariffs, however, will discriminate against the bloc and thus tend to cut down on trade. Quotas and other quantitative restrictions, used mainly to protect agriculture but applied also to other raw materials, may seriously affect Communist exports, particularly since the latter concentrate on nonindustrial exports to the Common Market. In De Gara's view, however, the replacement of the individual bilateral agreements with the Eastern bloc by a single agreement of the Community seems to offer most promise in influencing mutual trade relations. His conclusion is that the establishment of the Community augurs a new era in East-

West trade relations, in which countries belonging to it will be able to influence those relations to a much greater degree than previously.

Although the author mentions the cost-cutting effect of intra-European specialization and hence a tendency towards cheaper exports which would benefit prospective importers, he fails to take cognizance of the income effect of the Community: With more specialization, higher purchasing power, and more competition the national incomes in the Community receive an added boost. This increases, among others, the demand for imports, which increase is likely to affect also imports from the Eastern bloc. Whether this effect will be stronger than the discrimination effect of abolition of internal tariffs is hard to say.

Omitted also is the short-run effect on trade of changing Communist policies. This reviewer finds no quarrel, however, with the stress put by the author on gradualism in East-West relations and on the hope that closer economic links encourage national independence within the Communist bloc, so interestingly demonstrated recently by Rumania. Perhaps the Community will pave the way for an eventual economic unification of Europe, as envisaged by Z. Brzezinski.

The shortness of the period covered by the empirical study is its major drawback. It seems likely that a coverage starting with 1954 or thereabouts would reveal a substantial increase in trade, independent of the Common Market, which did not yet exist. In other words, it is difficult to attribute changes in the pattern of trade from the establishment of the Common Market to 1961 to that institution alone, and it would be rash to try to draw any connection between the political changes in the European satellites and the Community. Such a connection may be established over a longer period of years. Over the short run the dominant influence seems to be, from the Community's point of view, fortuitously political in nature. Consequently, no functional connection can be established as yet between the volume of East-West trade and the establishment of the Common Market.

Several minor blemishes can be found in the book under review: There is no indication whether trade statistics are in current or constant dollars. If the former are used, which is more likely, no reference is made to the impact of changing prices, e.g., in the striking case of Polish coal. A reference is made to "the Wests," instead of the Westerners (p. 66). An obvious redundancy is found at the top of page 45. E. Khmelnizkaia is referred to as a man (p. 24). On the whole, however, the technical editorship of the study is good and its scholarship sound.

De Gara's book is a valuable contribution to the study of a problem which ought to be raised more often and discussed from more points of view. It contains many penetrating remarks, omitted from this review in order to make the reader more interested in finding them out for himself. In the present reviewer's experience the search is a rewarding one.

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Business Organization; Managerial Economics; Marketing; Accounting

Economic Analysis and Industrial Management. By JACQUES LESOURNE. Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1963. Pp. xxii, 631. $10.00.

This is a translation by *Scripta Technica, Inc.*, of the second edition of the author's text, *Technique économique et gestion industrielle*. The latter has enjoyed a great success in France, and it is not difficult to see why. It reflects the phenomenal growth of what is variously called "operations research" and "management science" that has been taking place in France since World War II.

The most impressive feature about the book is its enormous scope, combined with a rich array of applications. It contains treatments of the axiomatic basis of measurable utility and behavior under risk, the Hicks-Samuelson theory of the firm and the consumer, some regression theory and the identification problem, interest and investment theory, inventory control, linear programming, and many other topics. No attempt is made to treat these topics in a completely rigorous way, but on the other hand they are handled with clarity in the statements of assumptions and conclusions, and with the aim of giving the reader an understanding of what is involved.

The philosophy of the book is presented in the following words (p. 3): "The economist serving management, unlike the analyst working in pure research, cannot afford to consider problems other than those of interest to management. An economic study is an investment which must yield return. It is of little importance that it is very difficult to predict the possible results of a study at the outset. Repeated evaluations, though lacking in precision, make it possible to avoid the more serious errors. Timeliness is an important factor because executives are often called upon to make a decision within a given period of time; a brief study, however imperfect, prepared in time to meet a deadline is more valuable than a voluminous and complete document presented six months too late." For the academic mind this attitude goes very much against the grain, and I wonder if the point is not overstated. The question of where to draw the deadline is surely a major problem in itself, and by its very nature one which executives are not entirely competent to pass on by themselves.

Of particular interest to U.S. readers is the exposition of some noteworthy contributions by French authors, including Maurice Allais, Marcel Boiteux, Robert Gibrat, Paul Massé, Jean Ullmo, and several others. This includes an illuminating discussion of "economic pseudo problems in accounting" (pp. 233 *et seq.*), in which cost accounting and depreciation policies are formulated from a decision-making point of view; much of this is based on work by Boiteux. The chapters on cost, amortization, production control and investment criteria, in Parts II and III of the book, are very detailed, combining the mathematical analysis, engineering data, and personal judgment and interpretation in an unusually well-integrated manner. They reflect the wide knowl-

edge and experience of the author and give the book a flavor all of its own.

Part I of the book, entitled "Econometrics in the Service of Business," does not, in my opinion, quite come up to the rest of the volume in quality. The treatment of each topic is necessarily sketchy and insufficient to provide a basis for intelligent use of econometric methods. The section on regression analysis (pp. 125-32) appears to confuse two quite different principles, that of the Markov theorem on least squares and that of Cramér's criterion of minimum mean-square linear regression. The discussion of behavior under risk (pp. 23-31) includes Allais' well-known objection to the von Neumann-Morgenstern axioms (which in my opinion is not so much an objection as a misinterpretation—though the point is a subtle one) and also an axiom due to Massé and Morlat justifying risk-aversion. The section on the theory of the firm (pp. 90-91) is too sketchy to be of much use; and in defining input demand functions in terms of output price as well as input prices, Lesourne fails to point out that this analysis cannot be applied under conditions of constant or increasing returns to scale.

Even though some topics are barely sketched, in all cases there is enough information to enable the student to find out what the subject is about, and there are ample references to original sources. Together with the many illustrations and examples, this makes the text an excellent introduction to what is becoming a vast and complex subject.

It is interesting to note the difference in emphasis between Lesourne's text and the kind of emphasis to be found in comparable texts in the United States. There is no discussion of queuing theory and hardly any treatment of statistical decision theory. The focus of attention is on production and costs; marketing, merchandising, and advertising are not dealt with at all. (I find this a refreshing contrast.) There is a strong emphasis on the integration of business objectives with social objectives, reflecting the close association between business and government to be found in France. In fact, most of the examples relate to problems of nationalized industries such as gas, electric power, and coal mines.

The translation is, on the whole, excellent. There are only a few awkward phrases. The "rainfall" of Koopmans' well-known example of the identification problem is translated "a downpour" (pp. 135-36); the phrase "corrected for systematic error" (p. 128) corresponds, of course, to "unbiased." And Robbins' famous definition, "Economics is the science which studies human behavior as a relationship between ends and scarce means which have alternative uses," comes out after double translation as "The science of economics is that science which makes a study of production and of the widespread use of unconventional means on an alternating or reciprocal basis" (p. xvii).

In giving advice to the student (pp. 9-10), Lesourne proves himself to be not only a scientist but also a diplomat: "The economic staff, on the strength of a relatively short-run study, must sometimes advise men with many years of experience that their procedures or products can be improved. These men will alternate between defending themselves and attacking the study. Both

positions must be taken seriously. First of all, the cooperation of these people is essential to the successful implementation of the results of the study. And, second, they may be right."

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Industrial Organization; Government and Business; Industry Studies

The American Petroleum Industry—The Age of Energy 1899-1959. By HAROLD F. WILLIAMSON, RALPH L. ADREANO, ARNOLD R. DAUM, AND GILBERT C. KLOSE. Evanston: Northwestern University Press, 1963. Pp. xx, 928. $7.50.

This massive volume will serve for years as an excellent introduction to many aspects of the petroleum industry. As the second volume of a two-volume work, it covers many interesting aspects of the industry for economists, industry people, and citizens interested in the public problems of the industry. The 60-year period is broken into four subperiods (1899-1919; 1920-29; 1930-41; 1942-59), and the pattern of the book is to provide for each period a chapter on production, transportation, refining, product demand, domestic marketing, and foreign trade. Unfortunately, the authors make no reference to much more comprehensive biographies, and references to leading economists who have analyzed key petroleum issues are unsystematic and incomplete.

There is a nice, even tone of skeptical reasonableness used in reviewing many of the heated issues. The petroleum industry has generated torrid debates, and the fires of controversy are certainly not dying down, even though there is some shift of battlelines.

The postwar period is dealt with only summarily, despite the fact that many would find it the most interesting and relevant era. There is almost nothing said, therefore, about domestic competition in the postwar era; the leveling-off of the rising share of crude petroleum in U.S. energy consumption; petroleum import quotas (and the imputed income to lucky importers of over $500 million annually from the foregone equivalent duty); percentage depletion; the effects of well-spacing on U.S. domestic crude production costs, output, and employment; the effects of refinery productivity improvement upon refinery employment; the effects upon world reserves of new discoveries and technological improvements in the last decade; the very high tax and royalty revenues per barrel enjoyed by producing countries; the restrictive excises and duties by European importing countries on crude oil and products; and the roles of postwar U.S. petroleum international investment.

The great strength of this book is its description of the eras before the current postwar period. The first two decades of the century were crucial in the transition from an Eastern kerosine industry to a Southwestern gasoline industry which has typified petroleum ever since. The shifts to rapid expansion of Gulf Coast and Oklahoma bonanza fields broke up the production dominance of Standard Oil far more than did the 1911 decision. In fact, it is

doubtful that the decision would have been economically effective if it had not been for the dramatic Southwestern discoveries and their rapid development. (The legal evolutionary effect of the decision might also have been significantly different.) The Southwestern discoveries were also very important in building the economies of these states, including California.

The shift of product dominance from kerosine to gasoline was, of course, caused by the automobile industry, not the petroleum industry. The shift, however, also served to shake up the refining segment of the industry and provide new opportunities for developing technical and marketing competition from aggressive independent companies. Again, the Standard Oil Companies failed to move with maximum vigor and were outflanked. The period of the 'twenties essentially consolidated and exploited the opportunities clarified in the previous decade.

The depression of the 'thirties exposed the instability of field-production competition under grossly inadequate State control. Crude oil production lent itself uniquely to cut-throat competition and underground theft. The weakness of prevailing regulations helped frame the widespread acceptance of State prorating, enforced federally by the Connally Hot Oil Act. The resulting system since the 1930's has been typified as operating as an interstate cartel which restricts oil production and keeps crude oil prices higher than they would have been in a more competitive industry.

How much more competitive should crude petroleum have been since the last half of the 'thirties to satisfy the national interest? There is no simple or legal answer to this question, either in terms of a clear-cut definition of some happy state of theoretical optimal competition or in terms of shibboleths of States' rights. The whole system is enforced federally, but there has not been a serious federal acceptance of the responsibility of determining a proper degree of competition (or regulation of crude oil output). The State Commissions have adroitly avoided really explaining how they set the quotas and have put off Congressional Committees with phrases about maintaining the "health" of the industry. Historically, this has meant maintaining a state of health and well-being considerably above that of the rest of U.S. industries—a fact that stirred almost no group except the Federal Trade Commission late in the 1930's.

The tax history of percentage depletion, expensing of intangible drilling expenses, and foreign taxes and royalties are glossed over. The economic consequences of the two major U.S. forms of tax assistance are not evaluated, despite reams of Congressional evidence on the subject and repeated official estimates of revenue losses. (For most years, revenue losses were greater from expensing than from percentage depletion.) The two forms of tax assistance have subsidized the replacement of bituminous coal by oil and gas as dominant fuels, but if the coal industry does not complain, it is not surprising that economists generally ignore this major distortion of resource allocation.

The fourth basic determinant of U.S. crude oil production which is not evaluated is prorationing by *wells* rather than by *fields*. Unlike every other major petroleum-producing nation, this puts a premium on drilling an excessive number of wells. Certain experts judge that between one-third and two-

thirds of the U.S. wells need not have been drilled and would not have l
except for State well-spacing laws which permit wells to be drilled too c
together compared to the economic engineering optimum for least-cost
exploitation. It has been estimated that over $10 billion were wasted on u
cessary drilling between 1948 and 1959, and that U.S. crude oil total c
could have been $1.50 a barrel rather than nearly twice that. This w
have meant that the United States could still have been a net exporte
1950 (and since) rather than a net importer. Perhaps our long-run conse
tion interest, however, was that we became a net importer when we did.
haps these questions are too detailed for economic history.

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Labor Economics

Employment and Economic Growth. Geneva: International Labour Of
1964. Pp. vi, 217. Paper, $2.25.

The title of this lucidly written volume suggests its principal thesis—
there is an intimate relationship between sound employment policies and p
cies for economic development and growth. The reduction of unemployn
and underemployment is an important goal not only for its own sake, but
because it generally may be expected to contribute to economic growth.
there are alternative routes to development and growth, and some of these
more productive of employment opportunities than others. Consequei
while employment policy must be formulated in the light of other econc
and social objectives, so must policies for development or growth be cogni:
of their employment effects. As a value judgment, it is suggested that an '
ceptable" growth rate with maximum employment opportunities may be p
erable to a maximum growth rate that leaves a portion of the popula
without adequate employment (pp. 43, 141).

Prepared originally for a 1963 Technical Conference on Employment P
cy, and revised and updated in the light of discussions at that conference,
report examines the dimensions of unemployment and underemploymen
both advanced and developing economies and describes and evaluates a v
variety of policy measures for dealing with the problem.

The book opens with a discussion of the concepts of unemployment
underemployment and an analysis of labor force and unemployment trend
a large number of countries of the world. The United States, Poland,
India are singled out to illustrate the diversity of manpower problems
countered by countries at different levels of development and with diffe
institutional frameworks. This is followed by two chapters which deal
turn, with the "objectives" and the "general principles" of employment p
cy. The three-fold goal of *full, productive,* and *free* employment is sugges
and there is an exploration of the general character of an active labor-ma
policy designed to achieve economic growth through the development

human capacities and the full utilization and effective deployment of man power.

The heart of the volume is in Chapters 4-6, which explore the policies relevant to dealing with three different types of unemployment problems—those arising from fluctuations in level of economic activity (cyclical and seasonal), those created by structural changes, and those characteristic of underdeveloped countries. Two relatively short final chapters deal with the contribution that can be made to solving or mitigating unemployment problems by unions and employers (principally in advanced countries) and by international aid and trade policies (principally with respect to developing countries).

The discussion of cyclical, seasonal, and structural unemployment problems is along the conventional lines of good labor-economics textbooks, but more elaborated and considerably enriched by a wealth of illustrative material from countries all over the world. From the description of the arsenal of policy weapons that have been and are being used elsewhere to combat unemployment, it is difficult to avoid the conclusion that the United States has failed to manifest the same will to come to grips with the problem that other countries have displayed.

The reviewer has some reservations about the conceptual framework in which cyclical and seasonal unemployment are analyzed. Both of these are treated in a chapter dealing with "short term fluctuations in output." Such a specification of the problem seems to be at once too broad and too narrow: too broad because it embraces both seasonal and cyclical variations for which the appropriate policy measures are of course quite different (which the report recognizes), and too narrow because it fails to include the problem of inadequate growth, for which many of the appropriate policy measures are identical with those for cyclical declines. It is rather surprising that "inadequate demand" unemployment was not used as a general type, particularly in view of the fact that the report in an earlier chapter acknowledges that the recent upward trend in unemployment in the United States has been attributable primarily to inadequate economic growth.

The longest and most interesting chapter is that devoted to the employment problems of underdeveloped economies, where underemployment rather than overt unemployment is the more pervasive phenomenon. The report explores measures for generating growth and expanded employment opportunities in both the traditional and modern sectors of such economies. A simple yet sophisticated analysis is presented of the advantages of exploring possibilities for labor-intensive techniques and of the use of "shadow prices" for planning purposes where market prices of labor and capital fail to reflect their real relative costs. Subsidizing employment where money wages exceed marginal productivity (provided it is greater than zero) is justified as a means of increasing both employment and growth, so long as the subsidy is at the expense of (other people's) consumption rather than investment.

The unidentified staff member(s) of the I.L.O. Secretariat responsible for the publication are to be commended for a valuable contribution to the literature on both unemployment and economic development. The book combines

wealth of descriptive material with sound economic analysis. Not the least of its virtues are the ample footnote references to relevant recent literature in all parts of the world.

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Disabled Workers in the Labor Market. By A. J. JAFFE, LINCOLN H. DAY, AND WALTER ADAMS. Totowa, N.J.: The Bedminster Press, 1964. Pp. 191. $5.00.

This study is an analysis of the experience of approximately 1300 male employees in re-entering the labor market after having been seriously and permanently injured on the job. The major part of the work is concerned with an examination of the variables associated with differing degrees of success, and the identification of the factors which are likely to be good predictors of success and failure in securing re-employment. The ability to identify such factors is of great importance to vocational rehabilitation agencies seeking to make optimum use of their limited resources. Among the additional related topics treated are the job-seeking and job-training efforts of the disabled employees, and their means of support while unemployed because of disability.

The subjects of study were men under the age of 60 in the New York City metropolitan area who were recipients of benefits for serious and permanent disabilities or who were determined to have incurred "lost earning capacity" under the workmen's compensation systems of either New York, New Jersey, or the federal government programs for federal employees and longshore and harbor workers. The injuries occurred between 1950 and 1957 and the men were interviewed in 1960.

At the time of interview 41 per cent of the men held jobs which were classified as "better" than the ones held at the time of injury, 39 per cent were in jobs which were the "same or somewhat poorer," and 20 per cent had "very poor or no" jobs. The factors deemed to be the best predictors of "job failure" and of only partially successful adjustment were: (1) the occurrence of one or more additional injuries, separated in time from, but related to, the one which brought the individual into the study; (2) the failure to return to work in the *same firm* in which the individual was employed at the time of injury; and (3) being 45 years of age or over with a maximum of eight years of schooling. As the authors point out, the "failures" among the disabled men studied resemble the unemployed in general, "only more so, and, in addition, have physical problems which diminish further their employment prospects."

The minimum degree of severity of the injury which was established to qualify the employee for inclusion in the study was that equal to the loss of a thumb—a loss which entitled the employee to 75 weeks of compensation under each of the systems from which the cases were drawn. However, except for differentiating the original injuries as to the parts of the body where the injury had occurred (i.e., trunk, upper or lower extremities, or internal), the authors do not utilize a scale of the relative severity of the injury at the time it occurred, although they do develop a rough index of the individual's physical condition at the time of interview. If, however, one desires to study the

relationship of disability to ultimate success in re-entering the labor market, and to identify *as soon as possible* the individuals most likely to be in need of vocational rehabilitation, it is rather important that the degree of severity of disability at the time of injury be differentiated. Thus, for example, the combined loss of an arm and leg may make considerably more difference than the loss of a thumb as concerns the ability of an individual to obtain re-employment, or as concerns the relative importance of his other personal characteristics. Such relationships need to be investigated. The authors had access to workmen's compensation records, information from personal interviews with the disabled men, and the opportunity for discussion and consultation with the personnel administering the workmen's compensation programs and with other experts in the fields of disability evaluation and physical medicine. The use of a scale of relative severity, or of a more refined typology of injuries in relation to the subsequent labor force experience and personal characteristics of the individuals studied would likely have yielded considerably more insight concerning the labor force salvability of seriously injured employees.

Of the men who failed to make a satisfactory labor market adjustment only one in ten claimed to have learned a new skill or trade, or to have undertaken any additional schooling—two of the principal types of activities included in vocational rehabilitation programs. Information as to why more in this group did not seek or undertake vocational rehabilitation is vital, and merits considerably more attention than that given to it in the study. Having had the opportunity for personal interviews with the disabled employees one wishes that the authors had probed more deeply in this area.

Despite these limitations the study is a good one. The analysis and presentation of the data are rigorous, the methodology is sound, and the findings are interesting. It is a valuable addition to the sparse literature on the relationship of disability to labor force participation.

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Hiring of Dock Workers and Employment Practices in the Ports of New York, Liverpool, London, Rotterdam, and Marseilles. By V. H. JENSEN. Cambridge: Harvard University Press, 1964. Pp. xiii, 317. $7.50.

Maritime industry employment has traditionally been viewed as the prototype of casual employment relationships. Increasingly, however, and through varied approaches in different ports, efforts are being made to overcome the disorganization of the longshore labor market and its concomitants of uncertain employment opportunities, inadequate earnings, excessive work loads, and the frequently illicit practices which derive from these conditions. Through a study of the port of New York juxtaposed to the varied developments in the ports of London, Liverpool, Rotterdam, and Marseilles, Dr. Jensen seeks to separate out the underlying common denominators required for achieving regularization of longshore employment and increased economic security, and the roles of labor, management, and government in achieving these. The appearance of the study is particularly timely in view of the pending protracted consideration of Atlantic and Gulf longshoring problems. How-

ever, while these have involved consideration of both manpower utilization and job security, the scope of the Jensen study is focused on job security.

The analysis of the development of a measure of organization in the longshore labor market in the port of New York is incisive and enlightening. It is based on a four-year study of New York longshore employment and labor relations seeking to explore charges and countercharges and inadequate information, particularly on the Bi-State Waterfront Commission established in 1953. An important clarification is revealed in the study regarding the employment rights on specific piers which many longshoremen had acquired through local custom and bargaining during the period of the shape-up, prior to the Waterfront Commission. These job rights are important to an understanding of the efforts to achieve mobility associated with an effectively organized labor force. Treated in detail is the decasualization impact of the efforts of the Waterfront Commission and the development of a seniority system by the New York Shipping Association and the International Longshoremen's Association. The decline in registered longshoremen from over 40,000 before 1954 to 22,000 in 1961 has meant increased work opportunities and earnings for those remaining, as the level of work hours has remained virtually unchanged. Decasualization has been incomplete, however, for there is still a fourth of the labor force working relatively short hours, the product in part of the Waterfront Commission's stated statutory inability to close the register. This is one of the bases for the continuing ILA objection to the Commission's activities.

This study makes a second and largely novel contribution, through the comparative treatment accorded the hiring systems of the five ports studied. For, as Professor John T. Dunlop states in the Foreword, few studies, outside of specialized ILO reports, have treated industry situations in varying national environments. This approach can serve as a tool to determine common features related to the industry, and variations which may be attributed to national or local conditions.

Following are some of the national variations distilled by Jensen's application of the comparative approach. Present British arrangements follow over 75 years of efforts to cope with casual longshore employment. The control of the size of the labor force in the ports and the determination of daily and weekly guarantees through the tripartite National Dock Labor Board are the result of the wartime control experience. Details of port labor-management relationships are maintained through labor-management port committees, so that there are important differences in employment practices and relationships in the ports of London and Liverpool. The greatest stabilization of earnings has developed in the port of Rotterdam. This is in large measure the reaction of employers both to labor shortages and possible union growth and influence. In Rotterdam, two-thirds of the workers are employed steadily by individual companies, while the remainder receive guarantees as part of a reserve maintained out of employer contributions. The role of government here is important particularly through the labor code which restricts the conditions under which workers may be laid off. The port of Marseilles is owned by the nation-

al government. While employment is private, the hiring system is set up under national law and administered by government agents. Unlike the other European ports covered, only a portion, albeit three-fourths of the force, is covered by reporting pay, but there are no guarantees.

Sifting through national variations, Jensen finds the common residue in three features essential to any decasualization plan. The first two factors he finds to be fast ones. These are, first, restriction of the supply of men, which requires compulsory registration and a closed, administered register. Centralized hiring is the second factor, intended to assure that men get to the places where their services are to be used. The third element, that of "predetermined selection," is left open to such influences as national or local preference as to whether equalization should be achieved through rotation of assignments, the equalization of work and earnings, or a system of priorities.

The study has an adequate area of concern despite the absence of direct treatment of such matters as the relationship of job rights and hiring arrangements to gang size, work rules, and the flexibility of manpower utilization. The mechanization and modernization agreement on the West Coast has met these concerns in one way. These subjects are under consideration by the parties on the Atlantic and Gulf Coasts. Dr. Jensen has made clear the complexity of organizing the longshore labor force. When efforts to achieve such organization are combined with work rule adjustments to changing technology and work methods, a task of infinite magnitude is involved, requiring boundless exercise of wisdom and fortitude on the part of labor, of management, and frequently in situations of this complexity, of government.

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Population; Welfare Programs; Consumer Economics

Federal Aid to Depressed Areas—An Evaluation of the Area Redevelopment Administration. By S. A. LEVITAN. Baltimore: The Johns Hopkins Press, 1964. Pp. xv, 268. $6.95.

S. A. Levitan has written the best study yet available on the economic problems of depressed areas. The work under review is thorough, lucidly presented, and effective in bringing out the deficiencies of the legislation and of this Administration of the Area Redevelopment Act. All students of unemployment will want to examine it.

The author is especially helpful in his survey of the legislative history and of the administrative machinery set up by the Area Redevelopment Administration, in his careful estimates of the funds required to do an adequate job, in his unusually thorough survey of the economic characteristics of depressed and nondepressed areas—their migration and population trends, their per capita income, their expenditures on education, their public expenditures generally, etc., etc.

No one conversant with this experience would deny many of the criticisms

of the enabling legislation: the award of money voted was most inadequate; the areas and population to be covered far too extensive; and the failure to provide guidelines for help was unfortunate.

In operation, the ARA suffered greatly from the weaknesses of the legislation. This clearly was not the fault of the prime mover of this legislation, Senator Douglas, nor of the other chairman, Senator Kennedy. It was not possible to get more cash for the program; and in the light of opposition of those who opposed public spending on all counts, of those who preferred other spending programs, and in view of the fact that many congressmen saw in ARA a dangerous precedent where the government would seek to determine the extent of migration of men and capital—in the light of these blocks, the legislation had to be inadequate and faulty. A few hundred millions just could not do the required job.

Levitan seems to be excessively critical of the ARA, and in particular for designating an excessive number of areas for eligibility for help from the ARA. Indeed the effect is spreading the limited funds too thin. But when one considers the incapacity of the congressional sponsors to withstand similar pressures, it does seem unfair to blame Mr. Batt, the Administrator, and his lieutenants. As Levitan himself shows, the ARA was not provided with an economic staff of required proportions, the pressure for haste was great, the failure to yield to political pressures would have been disastrous. Even with these concessions, the ARA still has not been able to get a needed appropriation for 1965. Given the difficulties confronting the Administration, the only way out seemed to be generous designations of areas eligible for help even if—as Levitan so well shows—a substantial proportion are not really in need of subsidies. Incidentally, the progress of ARA since the period covered by the author has been substantial. The statistical material needed to appraise needs of areas with relatively small numbers of workers was not available. The program suffered from these statistical deficiencies as well as from the absence of the best kind of cooperation from other agencies. The simplest and apparently only practical approach was to be generous in designating eligible areas. These were not the only obstacles confronting the ARA. Levitan is very helpful in showing how inadequate the training facilities were for retraining workers under the ARA program and a fortiori after the Manpower Training Program further increased the pressures on the facilities for vocational training.

It is not easy to estimate the net gains of jobs related to the activities of the ARA. One of the unknowns is the multiplier. In the days of discussion of the tax cut, estimates of the multiplier by knowledgeable Washington economists varied from 2 to 5. Another unknown is the extent to which the jobs are additional or diversionary. Levitan does at least estimate the number of additional jobs that would be required to bring unemployment down to 4 per cent and the amount of government money required to achieve this outcome. The costs may be even larger than he estimates and in part because one must allow for the rise in the numbers in the labor market in response to the improved economic situation.

The cost of the programs per job will be less, the smaller the investment required per job, a fact well known to the ARA. For this reason, among others, as Levitan was aware, investments in motels or hotels were difficult to support. Yet, these employments are on the increase and, therefore, have a special appeal to the ARA. The argument of existing excess capacity is not so relevant.

In one respect the Levitan volume is disappointing. One would have expected a discussion of the ARA and alternative approaches to full employment —the *direct* approach as against the general or indirect approach, that is, the recourse to fiscal and monetary policies. The author limits himself to a page or two on these issues, in which he is critical of the Joint Economic Committee of the Congress for not emphasizing more the direct approach.

But the relative contribution of the direct and indirect approaches to unemployment has been one of the hottest issues in Washington, with views divided among government economists and departments and also among academic economists. Whereas the Council of Economic Advisers (CEA) has tended to emphasize the indirect or general approach, the Department of Labor has been more sympathetic with the direct approach. In the early period of the Kennedy Administration, the CEA tended to understate the case for the direct approach.

The direct approach can be helpful, but the indirect attack is crucial. With unemployment at 4 million or more, the general attacks will account for the major gains of employment. This is suggested by the fact that for eight declining industries losing 2½ millon of 7½ million jobs in 13 postwar years, the jobs lost were eight times as large per year in the four relatively depressed years as in the nine prosperous years. The extent of help to be expected from the direct approach—area redevelopment, increased government contracts, reduced imports—is limited by the number of unfilled vacancies. But many of the unfilled vacancies will not be filled because to do so may require moving workers from West Virginia to Los Angeles, or training a coal miner or a textile worker to be a professional or semiprofessional worker. There may well be a million unfilled vacancies in the professional or semiprofessional fields. But it will take many years before the requisite training is had to provide a million additional jobs from the million unfilled vacancies in this area.

In summary, Levitan has made an important contribution. But he expected too much of the Administrator of the ARA and his staff who have coped admirably with very difficult problems and who, if they had followed the lines proposed by Levitan, would undoubtedly have been even more untenable politically than they actually are. In operating a highly sensitive program, one has to be satisfied with much less than the ideal.

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Anticipations and Purchases—An Analysis of Consumer Behavior. By F. THOMAS JUSTER. Princeton: Princeton University Press for the National Bureau of Economic Research, 1964. Pp. xviii, 303. $6.50.

It is a well-known fact that cyclical fluctuations in major demand sectors of the economy are of different magnitude and duration. In the broad consumer sector, purchases of durable goods—automobiles, appliances, and furniture—show the most violent fluctuations over the course of the business cycle, and forecasters have long searched for better ways to predict these fluctuations. In recent years, an increasingly popular forecasting device has been the utilization of data on consumer purchase anticipations which are gathered, for example, in the well-known Survey Research Center and Federal Reserve Board surveys.

F. T. Juster's book reports the results of one major effort to use and evaluate a special and different set of durable goods purchase anticipation data; a series of mail interviews and reinterviews of member-subscribers to Consumer Union of the United States, publisher of the popular monthly magazine, *Consumer Reports*. The focus of his study is a detailed analysis of the meaning of these anticipation data and their relationship to actual purchases of durable goods. Within this focus Juster discusses many provocative and interesting problems which can conveniently be grouped into three topics.

The first topic deals with problems associated with questionnaire design and the phrasing of specific questions pertaining to the respondents' purchase expectations. Having a relatively free hand in the design, Juster was put in a fortunate and unusual position, and he experimented with a number of differently formulated questions concerning anticipated purchases. These different questions were then used in five randomly selected subsamples in order to determine which were the best predictors of subsequent purchase behavior. Juster concludes that the preferred wording is one "that explicitly suggests the complicated nature of a probability judgment, rather than questions with undefined phrases that the respondent must interpret as best as he can." Juster also states that better results are obtained when intention questions are asked only after those dealing with past and expected financial variables, durable stock position, and other questions that help to clarify the respondent's thoughts about his future durable-goods needs, wants, and ability to pay.

Juster's conclusions are useful, and any criticism may sound ungrateful, considering the rarity of rigorous experimentation in questionnaire design, but it seems to me that the evaluation of the questions in terms of their accuracy in forecasting aggregate durable-goods purchases (the critical test from a business-cycle-forecasting point of view) was less than complete. Also, the author's suggestion that an interview should deal with durable-goods purchase expectations only after many other questions have been asked may have limited applicability. Anticipation data are usually collected within a framework of a broad interview with many aims. Therefore, Juster's suggestion may be in conflict with, for example, the objective of obtaining the best financial information from the respondent.

The second topic of the book is Juster's most important contribution and

one that underlies his entire analysis. Juster proposes the hypothesis that the responses to questions concerning purchase plans are "essentially a reflection of the respondent's subjective estimate of his purchase probability. . . ." Affirmative purchase-plan answers are given by respondents who believe that their chances of becoming purchasers are such that a negative reply would be less accurate. Moreover, Juster argues that there is a continuous distribution of these subjective purchase probabilities and that the anticipation surveys yield an estimate of "the proportion of the population with buying plans above some unknown cut off point." It follows that survey responses on the proportion of planners may not be neatly related to the mean *ex ante* probability of purchasing or to the actual purchases. Despite numerous ingenious ways designed to test these propositions and many impressive statstical manipulations, Juster's conclusions are only mildly encouraging. "On the whole, the evidence tends to support the hypothesis that questions about intentions to buy elicit responses drawn from a continuous distribution of purchase probabilities. No convincing evidence is available on the shape or the stability of the distribution functions."

The third major topic is an analysis of the relationship of purchase expectations to actual purchases in conjunction with other explanatory variables. An especially interesting test (Chapter 6) involves the use of a "surprise" variable in order to capture the possible effect of events that the respondent did not expect at the time of the first interview but which did occur, e.g., an unexpected income increase or decrease.

In Chapter 7, Juster reports the results of a large-scale multivariate regression analysis using a number of independent variables of a financial and demographic nature to study two different dependent variables—actual durable-goods purchases and intentions to buy durable goods. The latter dependent variable, of course, appears as an independent variable in the multivariate analysis of actual purchases. His conclusions are that these analyses "lend additional support to the hypothesis that consumer responses to questions about intention to buy durable goods are basically a reflection of purchase probability," and that in comparison to other variables "intentions to buy durable goods have by far the strongest net relations to durable goods purchases. . . ." The latter conclusion, incidentally, is also reached by most studies of cross-section data, but it is one that runs counter to results obtained in time-series analyses.

Anticipations and Purchases, in some respects, is an incomplete book. The purpose of studying consumer purchase expectations can be either to improve our knowledge of consumer behavior with regard to the process of decision-making, the planning period, etc., or simply to make our aggregate forecasts of consumer durable purchases more precise. In both respects, important questions remain unanswered. Juster fails to spell out his views concerning the conceptual framework underlying consumer behavior and how it relates to the empirical work. The implications of Juster's findings for the forecasting of aggregate consumer durable-goods spending are also not made clear. The few pages devoted to the relationship of cross-section and time-series analyses are much too brief, and economists who look for some help in forecasting con-

sumer durable-goods outlays will benefit little from the book.

Despite these criticisms, however, there is little doubt that Juster, in his research, has demonstrated considerable ingenuity and imagination, and that within the narrow focus of the project the reader has been given many new insights and ideas.

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NOTES

Experimental Courses in Elementary Economics

The Association's Committee on Economic Education has undertaken to arrange for publication of a small sample of experimental courses in college-university level elementary economics. The primary purpose is to make information of such experiments widely available to other institutions and individual members of the profession.

Some four to six courses which may be of widespread interest will be reported in a special small volume, whose publication has been subsidized by grants of funds and services from the Kazanjian Foundation and the Joint Council on Economic Education. The committee hopes to select a sample which will include different types of courses that may be of particular interest to different types of colleges and universities. Complete descriptions of the courses selected will be reported, including syllabi, references to reading materials, special student projects or assignments, teaching approaches, and the like. Wherever possible, evaluations indicating the successes and weaknesses of the course in achieving its objectives will also be included, so as to increase the potential usefulness of the course information to other schools.

Departments and individual teachers are invited to send information on courses at their own, or other, institutions which they believe should be considered for inclusion in this volume. The committee has established no firm rule as to how "experimental" a course must be to be suitable for submission, but its hope is to help disseminate information on basic courses which may provide insights or information that will be new to many economists planning courses and teaching at the elementary level.

Please send information to the chairman of the committee (Professor G. L. Bach, Department of Economics, Carnegie Institute of Technology, Pittsburgh, Pennsylvania 15213), in sufficient detail to give the committee a basis for deciding whether more detailed information on the course should be requested for use in a final screening process. You are invited to discuss the undertaking and criteria for selection of courses to be reported with any member of the committee. Prompt submission of nominations will help the committee to meet its tentative deadline, which is to select the courses to be reported this year by mid-summer and have the volume ready for circulation by mid-winter. If a large number of interesting experiments are submitted, the committee will attempt to arrange a second such volume for 1966.

Committee on Economic Education

New Publication

A new quarterly journal, *Water Resources Research,* sponsored by the American Geophysical Union, will begin publication this year. Invitation is extended for original scientific contributions that present results of basic or applied research or offer an examination of the principles governing water economics and management. Papers in the physical, chemical, or biological sciences should be sent to: Walter B. Langbein, U.S. Geological Survey, Washington, D.C. 20242. Papers in the social sciences, including economics and law, should be sent to: Allen V. Kneese, Resources for the Future, 1755 Massachusetts Avenue, N.W., Washington, D.C. 20036.

Research Grants

The Agricultural Development Council is continuing in 1965 a program of grants to facilitate research in international agricultural development by professors of American universities. Applications for research grants will be considered only from full-time faculty members of American universities. While projects must be supervised by grant recipients, advanced graduate students may collaborate in studies. Proposed studies should relate to

the economic and human problems of agricultural development in the developing nations. Preference is given to proposals that promise to increase understanding of the process of agricultural development. Most studies involve field work in Asia, Africa, or Latin America.

Applications for grants of up to $2,500 may be submitted to the Council at any time. For grants of more than $2,500, applications are considered three times each academic year. Requests for research grant application forms or correspondence relating to the program may be addressed to Dr. Clifton R. Wharton, Jr., Director, American Universities Research Program, The Agricultural Development Council, Inc., 630 Fifth Avenue, New York 20, N.Y.

Announcements

For the eighteenth consecutive year, the Survey Research Center of the University of Michigan will hold a Summer Institute in Survey Research Techniques. The Institute is designed to meet some of the educational and training needs of men and women engaged in business and governmental research and other statistical work, and graduate students and university instructors interested in quantitative research in the social sciences.

The 1965 Institute will be presented in two four-week sessions, the first from June 28 to July 24 and the second from July 26 to August 21. These two sessions may be taken independently or successively. For further information please write to the Survey Research Center, University of Michigan, P.O. Box 1248, Ann Arbor, Michigan 48106.

The Economic Institute, which provides a summer training program for selected foreign students who have been admitted to regular graduate study in economics or agricultural economics at universities and colleges in the United States, announces its eighth session for the period June 24 to August 25, 1965 at the University of Colorado, Boulder, Colorado. Application forms and scholarship information can be obtained from the Institute of International Education, 809 United Nations Plaza, New York, New York 10017, or from the director of the Economics Institute, Professor Wyn F. Owen, University of Colorado, Boulder, Colorado.

Professor Albert A. Blum is compiling, for future publication in the journal, *Labor History*, a list of research projects in progress in the labor history field. He requests students and faculty who are doing such research to send their names, the subjects of their research, and their affiliations to him at the following address: School of Labor and Industrial Relations, Michigan State University, East Lansing, Michigan.

Deaths

Viva Boothe, October 8, 1964.

Albert J. Raebeck, November 27, 1964.

Charles Winston, lecturer in economics, Washington University, October 1964.

Retirements

John C. Fetzer, professor of economics, University of Miami, June 1964.

James W. Martin, director, Bureau of Business Research, University of Kentucky, July 1, 1964.

Margaret Myers, professor of economics, Vassar College, June 1964.

Ruth Edith Thomas, professor of commerce, University of Kentucky, July 1, 1964.

Visiting Foreign Scholars

R. D. C. Black, The Queen's University of Belfast: visiting professor of economics, Yale University.

Kenan Bulutoglu, University of Istanbul: visiting foreign scholar, Columbia University, 1964-65.

Guy T. de Ghellinck, Université Catholique de Louvain: visiting associate professor of applied mathematics, Graduate School of Business, University of Chicago, January 1, 1965 to August 31, 1965.

Barry Supple, University of Sussex, England: visiting professor, department of economics, Harvard University, fall term 1964-65.

Sabri F. Ulgener, Istanbul University: visiting professor, department of economics, Columbia University, academic year 1964-65.

Henry Y. Wan, Jr., University of New South Wales: visiting assistant professor, department of economics, University of Washington, academic year 1964-65.

Donald N. Winch, University of Sussex, England: visiting lecturer, department of economics, Rice University, spring semester, 1964-65.

Pieter de Wolff, University of Amsterdam: visiting professor, department of economics, Harvard University, academic year 1964-65.

A. J. Youngson, University of Edinburgh: visiting research professor of economics, Yale University, January to June 1965.

Promotions

Wallace Barr: associate professor of agricultural economics, Ohio State University.

James H. Bearden: professor of marketing, East Carolina College.

Carolyn S. Bell: professor of economics, Wellesley College.

Edward L. Claiborn: associate professor of economics, U. S. Air Force Academy.

Albert R. Conley: professor of economics, East Carolina College.

Bruce Davie: assistant professor of economics, Georgetown University.

John E. Drotning: assistant professor, department of industrial relations, School of Business Administration, State Universtiy of New York, Buffalo.

I. M. Drummond: associate professor, department of political economy, University of Toronto.

Bruce Duncombe: assistant professor of economics, Georgetown University.

Gary D. Eppen: assistant professor of industrial administration, Graduate School of Business, University of Chicago.

Sigfried Garbuny: associate professor of economics, Georgetown University.

John D. Guilfoil: assistant professor of economics, School of Commerce, New York University.

Michel E. A. Hervé: assistant professor of economics, Yale University.

Winfield Hutton: associate professor of economics, Hunter College.

Charles H. Ingraham: assistant professor of agricultural economics, Ohio State University.

Harold L. Johnson: professor, School of Business Administration, Emory University.

Benjamin F. King, Jr.: assistant professor of statistics, Graduate School of Business, University of Chicago.

Philip Kotler: associate professor of marketing, Northwestern University.

Richard A. Ladd: associate professor of economics, U. S. Coast Guard Academy.

Robert D. Leiter: professor of economics, City College of the City University of New York.

Harvey J. Levin: Augustus B. Weller Chair in Economics, Hofstra University.

Ta-Chung Liu: Goldwin Smith Professor of Economics (Distinguished Chair), Cornell University.

Francis B. McCormick: professor of agricultural economics, Ohio State University.

Lionel McKenzie: John Munro Professor of Economics, University of Rochester.

Feng-hwa Mah: associate professor of economics, University of Washington.

Peter M. Mieszkowski: assistant professor of economics, Yale University.

John M. Montias: professor of economics, Yale University.

Oscar K. Moore: professor of economics, East Carolina College.

Van Doorn Ooms: research staff economist and lecturer in economics, Yale University.

Hugh T. Patrick: associate professor of Far Eastern economics, Yale University.

James L. Pierce: assistant professor of economics, Yale University.

Gustav Ranis: professor of economics, Yale University.

Marvin E. Rozen: professor of economics, Pennsylvania State University.

Leonard G. Schifrin: assistant professor of economics, Yale University.

Irwin H. Silberman: assistant professor of finance, School of Commerce, New York University.

Kenneth H. Smith: assistant professor of economics, Hunter College.

Robert E. Strain: professor of economics, California State College at Long Beach.

Donald G. Tailby: associate professor, department of economics, University of Georgia.

J. Robert Tompkin: professor of agricultural economics, Ohio State University.

Vladimir G. Treml: associate professor, department of economics, Franklin and Marshall College.

Vern Vandemark: assistant professor of agricultural economics, Ohio State University.

George Viksnins: assistant professor of economics, Georgetown University.

Francis E. Walker: associate professor of agricultural economics, Ohio State University.

Max S. Wortman, Jr.: associate professor of industrial relations, department of business administration, State University of Iowa.

Richard Zock: assistant professor of economics, U. S. Air Force Academy.

Administrative Appointments

August C. Bolino, Division of Economic Analysis, Office of Manpower, Department of Labor: director, Evaluation of Manpower Development Programs, U.S. Office of Education.

Harvey C. Bunke, University of Iowa: president, Western Washington State College, February 1, 1965.

Alfred B. Carlip: associate dean for graduate studies, Harpur College, State University of New York.

H. C. Eastman: associate dean, School of Graduate Studies, University of Toronto.

Robert Eisner: chairman, department of economics, Northwestern University.

Padraic P. Frucht, University of Florida: assistant administrator for economics, Small Business Administration.

John L. Fulmer: professor of economics and director, Bureau of Business Research, University of Kentucky.

Paul T. Hendershot: assistant dean, School of Business, East Carolina College.

Howard H. Hines: division director for social sciences, National Science Foundation.

Linwood L. Hodgdon: director, Office of International Programs, Colorado State University.

Charles Hoffmann: acting chairman, department of economics, State University of New York, Stony Brook.

David M. G. Huntington: executive assistant to vice president, Planning and Development, University of Chicago.

Benjamin J. Klebaner: acting assistant dean—personnel, College of Liberal Arts and Science, City College of New York.

Paul L. Kleinsorge: director, Institute of Labor and Industrial Relations, University of Oregon.

Solomon B. Levine: director, Center for Asian Studies, University of Illinois.

H. Gregg Lewis: director, graduate program, department of economics, University of Chicago.

Vito Natrella, Securities and Exchange Commission: director, Statistics Division, Internal Revenue Service.

E. Dale Peterson: chairman, department of economics, School of Business, Mankato State College.

Henry Ponder, Virginia State College: chairman, department of business and professor of economics, Fort Valley State College.

Raymond P. Powell: chairman, department of economics, Yale University.

Kenneth D. Roose: professor of economics and dean, College of the Liberal Arts, Pennsylvania State University.

James S. Schindler: dean, School of Business Administration, State University of New York, Buffalo.

Jack W. Skeels: acting head, department of economics, Northern Illinois University, academic year 1964-65.

Robert S. Smith: chairman, department of economics and business administration, Duke University.

William C. Spencer: associate dean, Graduate School of Business, Columbia University.

Norman W. Taylor: chairman, department of economics, Franklin and Marshall College.

Richard J. Thain: associate dean of students and director of placement, Graduate School of Business, University of Chicago.

Raymond R. Toledo: professor and chairman, economics department, Brescia College, Owensboro, Kentucky.

Jack T. Turner: professor of business administration and associate dean, College of Business Administration, University of Illinois, Chicago.

Peter N. Vukasin: chairman, department of economics, Harpur College, State University, of New York.

Jacob Weissman: chairman, department of economics, Hofstra University.

Howard C. Williams, Ohio State University: appointed by President Lyndon B. Johnson to serve a three-year term on the National Agricultural Advisory Commission.

Appointments

Norman D. Aitken: instructor in economics, University of Massachusetts.

Shirley Almon: lecturer in economics, Wellesley College, 1964-65.

Takeshi Amemiya: acting assistant professor, department of economics, Stanford University, 1964-65.

Richard E. Attiyeh: research staff economist and lecturer in economics, Yale University.

Samuel Battaglia: instructor, department of management science, School of Business Administration, State University of New York, Buffalo.

Haskel Benishay: associate professor, department of management science, School of Business Administration, State University of New York, Buffalo.

Heinz Biesdorf: assistant professor, College of Home Economics, New York State College of Home Economics, Cornell University.

Richard G. Blackhurst: lecturer in economics, University of Chicago, and director, Economic Research Center, University of Cuyo, Mendoza, Argentina, February 1965.

Aryeh Blumberg: instructor in economics, Graduate School of Business, University of Chicago.

John C. G. Boot: associate professor, department of management science, School of Business Administration, State University of New York, Buffalo.

Robert D. Bowers: instructor, department of economics, Western Reserve University, 1964-65.

Malcolm C. Brown: instructor in economics, University of Saskatchewan, academic year 1964-65.

John F. Burton, Jr.: lecturer in economics, Yale University.

Adhemar Byl: instructor in economics, Georgetown University.

D. R. Campbell: professor of economics, University of Toronto.

David Cass: research staff economist, Yale University, September 1964 to May 1965.

William H. Chartener, senior economist, Stanford Research Institute: economist, Goldman, Sachs & Co., New York.

Kuk Soo Chung: instructor in economics, Vassar College.

Ronald H. Coase: professor of economics, Graduate School of Business and Law School, University of Chicago.

Harold A. Cohen, Cornell University: assistant professor, department of economics, University of Georgia.

Natalie Z. Davis: lecturer in economics, University of Toronto.

Robert Davis: lecturer in economics, Georgetown University.

Edwin R. Dean: assistant professor, department of economics, Columbia University.

Eberhard W. Dinkelacker: instructor in economics, Georgetown University.

R. Conrad Doenges: assistant professor of finance, University of Texas.

Leo M. Egand: senior management consultant (economic & fiscal), Office of the Mayor, Office of Administration, City of New York.

Bert E. Elwert, Indiana University: assistant professor of business administration, College of Business Administration, University of Illinois, Chicago.

John R. Eriksson: assistant professor of economics, Williams College.

Allen H. Fenichel: assistant professor of economics, McGill University.

David Funk: assistant professor, department of economics, Wellesley College, academic year 1964-65.

Ved. P. Gandhi: assistant professor of economics, University of Massachusetts.

Marvin Gelfand: instructor in economics, University of Massachusetts.

James A. Gherity: associate professor, department of economics, Northern Illinois University.

Henry N. Goldstein: Board of Governors of the Federal Reserve System: assistant professor of economics, Washington State University.

Richard L. Gordon: assistant professor of mineral economics, College of Mineral Industries, Pennsylvania State University.

J. A. G. Grant: special lecturer in economics, University of Toronto.

W. Lee Hansen: department of economics, University of Wisconsin.

Neil E. Harl: associate professor, department of economics, Iowa State University.

Mostafa F. Hassan: assistant professor of economics, University of Miami.

Edward L. Hauswald, Evansville College: research associate, Bureau of Business Research, College of Business Administration, University of Nebraska.

Glenn C. Himes: assistant professor, department of agricultural economics, Ohio State University.

Samuel P. S. Ho: research staff economist and lecturer in economics, Yale University.

William E. Hoehn: research staff, economics department, RAND Corporation.

Robert Jacobson: associate professor, department of agricultural economics, Ohio State University.

Frank C. Jen: assistant professor, department of management science, School of Business Administration, State University of New York, Buffalo.

Arthur M. Johnson: assistant professor of economics, U.S. Coast Guard Academy.

Ronald L. Johnson: wage and hour investigator, U.S. Department of Labor, Wage & Hour & Public Contracts Divisions, Los Angeles.

Ted Lee Jones: associate professor, department of agricultural economics, Ohio State University.

R. W. Judy, Digital Computation Laboratory, Air University of U.S. Air Force: associate professor, department of political economy and Institute of Computer Science, University of Toronto.

Hugo M. Kaufmann: assistant professor, department of economics, Franklin and Marshall College.

Woo Sik Kee: assistant professor of economics, State University of New York, Stony Brook.

Donald B. Keesing: assistant professor, department of economics, Columbia University.

Boyd King: instructor in economics, Georgetown University.

Y. Kotowitz, Johns Hopkins University: associate professor of economics, University of Toronto.

Richard Kozelka: visiting professor of economics, College of Business Administration, University of Florida, winter trimester.

Joseph Krislov: professor of economics, University of Kentucky.

Ralf Kuehnel: instructor, department of management science, School of Business Administration, State University of New York, Buffalo.

W. H. Lankford: instructor, department of economics, University of Georgia.

Richard I. Leighton: assistant professor, department of economics, Harpur College, State University of New York.

Anthony J. Lerro: associate professor of economics, East Carolina College.

Lester S. Levy: professor, department of economics, Northern Illinois University.

J. C. Liu: assistant professor of economics, McGill University.

Gary A. Luoma: assistant professor, School of Business Administration, Emory University.

Donald W. Lybecker: instructor in the department of economics, Iowa State University.

Harald Malmgren: lecturer in economics, Georgetown University.

Andrea E. Maneschi: research staff economist, Yale University, September 1964 to August 1965.

Julius Margolis: professor of engineering-economics, Stanford University.

John F. Mead: instructor in economics, University of Kentucky.

Jack Melitz, Rice University: assistant professor of economics, Tulane University.

N. M. Meltz: assistant professor of economics in the department of political economy and Scarboro College, University of Toronto.

Jan Michal: associate professor, department of economics, Harpur College, State University of New York.

J. C. Mills, U.N. Economic Commission for Africa: associate professor of economics, Memorial University, Canada.

Joseph D. Mooney: assistant professor of economics, Princeton University, September 1965-June 1968.

John C. Murdock: research professor of economics, department of economics, University of Missouri.

Edward Murphy: instructor in economics, Georgetown University.

Lawrence B. Myers, Agency for International Development: principal economist, Cornell Aeronautical Laboratory.

James Nakamura: assistant professor, department of economics, Columbia University.

Wladimir Naleszkiewicz: associate professor of economics, University of Notre Dame.

Mohammed I. Nadiri: assistant professor, department of economics, Northwestern University.

Myron Bill Neace: assistant professor, School of Business Administration, Emory University.

Jeffrey Nugent: assistant professor of economics, University of Southern California.

Clifford Owen: lecturer in economics, Georgetown University.

David L. Paden: senior economist and manager of transportation research, Midwest Research Institute.

Josephus O. Parr: assistant professor of economics, Vanderbilt University.

Pierre-Paul Proulx: assistant professor of economics, McGill University.

Frederic L. Pryor: research staff economist, Yale University, September 1964 to August 1965.

Robert H. Renshaw: assistant professor of economics, School of Business Administration, Wake Forest College.

Idrian N. Resnick, Howard University: lecturer in economics, University College, Dar es Salaam, Tanganyika.

Thyrele Robertson: research associate, Iowa State University, and with AID in Peru, December 1964 for two years.

Donald C. Rocke: associate professor of economics, East Carolina College.

Thomas J. Rothenberg: research staff economist and visiting lecturer in economics, Yale University, January to June 1965.

Edward Saraydar: assistant professor of economics, California State College, Long Beach.

Anthony E. Scaperlanda: assistant professor, department of economics, Northern Illinois University.

Albert Schepanski: instructor in economics, University of Saskatchewan, academic year 1964-65.

Elbert W. Segelhorst: assistant professor of economics, California State College, Long Beach.

Hale T. Shenefield: visiting professor of economics, University of Southern California.

James J. Sherman: instructor, department of industrial relations, School of Business Administration, State University of New York, Buffalo.

Gary Singer: instructor, department of financial accounting, School of Business Administration, State University of New York, Buffalo.

J. Graham Smith: instructor, department of economics, Wellesley College, academic year 1964-65.

James B. Spalding, Jr.: assistant professor of business, Western Illinois University.

D. A. A. Stager: lecturer in economics and dean of men, University of Toronto.

Howard Steele: research and teaching position, department of agricultural economics, Ohio State University; two-year term with Ohio State Contract team in Brazil developing agricultural economics graduate programs.

James A. Stephenson: assistant professor, department of economics, Iowa State University.

Henri Theil, Netherlands School of Economics: professor of economics and business and executive director, Center for Mathematical Studies in Business and Economics, University of Chicago, September 1, 1965.

Richard S. Thorn, University of the City of New York: associate professor of economics, University of Pittsburgh, September 1965.

Yien-I Tu: assistant professor of economics, University of Kentucky.

Edward Van Roy: instructor in economics, State University of New York, Stony Brook.

J. Van Wagstaff: assistant professor of economics, School of Business Administration, Wake Forest College.

Roger N. Waud: instructor in business economics, Graduate School of Business, University of Chicago.

M. L. Weidenbaum, Stanford Research Institute: associate professor of economics and director, NASA Economic Research Project, Washington University.

Stanislaw Wellisz: professor, department of economics, Columbia University.

Robert H. West: associate professor of business administration, East Carolina College.

Byron White: professor of economics, Universidad Catolica "Madre y Maestra," Santiago de los Caballeros, Dominican Republic.

T. David Williams: lecturer, department of economics, Northwestern University.

John S. Wright, Northwestern University: professor of marketing, College of Business Administration, University of Illinois, Chicago.

Russell Yerkes: instructor, department of financial accounting, School of Business Administration, State University of New York, Buffalo.

Yi-Change Yin: visiting assistant professor of economics, University of Southern California.

Leaves for Special Appointments

Henry G. Aubrey, New School for Social Research and the Council on Foreign Relations: visiting professor, department of economics, Columbia University, spring term 1965.

Clarence L. Barber, University of Manitoba: visiting professor of economcis, McGill University, 1964-65.

George F. Break, University of California, Berkeley: visiting professor, department of economics, Harvard University, academic year 1964-65.

Benjamin Chinitz, University of Pittsburgh: Fulbright research scholar, University of Glasgow, April through June 1965.

Robert W. Clower, Northwestern University: Keynes visiting professor, University of Essex, 1965-66.

Dorothy Douglas, Hofstra College: visiting professor of economics, University College, Nairobi, Kenya, East Africa.

James R. Elliott, Northern Illinois University: visiting professor of comparative economics, Goettingen University, Goettingen, Germany, academic year 1964-65.

Eirik G. Furubotn, Harpur College, State University of New York: visiting associate professor, department of economics, Tulane University.

C. Lowell Harriss, Columbia University: visiting professor of economics, Yale University, January to June 1964.

Bert G. Hickman, Brookings Institution: visiting scholar, Econometric Institute of the Netherlands School of Economics, 1964-65.

Herbert S. Levine, University of Pennsylvania: visiting associate professor, department of economics, Columbia University, spring term 1965.

Paul M. Mazur, Lehman Brothers Investment Banking: visiting professor of investment banking, Wharton School, University of Pennsylvania, 1964-65.

Wallace C. Peterson, University of Nebraska: Fulbright lecturer at Athens School of Economics and Commercial Science.

Ralph B. Price, Western Maryland College: Fellow, American Institute of Indian Studies, Deccan College, Poona, India, academic year 1964-65.

Warren C. Robinson, Pennsylvania State University: director, Demographic Research Section, Institute for Development Economics, Karachi, Pakistan.

James N. Tattersall, University of Oregon: special assignment 1964-65 with the Facultad de Economia, Universidad de los Andes, Merida, Venezuela.

Paul N. Taylor, University of Connecticut: manpower planning in Saudi Arabia for Ford Foundation, spring semester, 1965.

Paul L. Van Moeseke, Iowa State University: visiting professor of economics, University of Louvain, one year beginning January 1965.

Stanislaw Wasowski, Georgetown University: AID research work on commodity agreements, Washington, D.C., academic year 1964-65.

Clifton R. Wharton, Jr., Agricultural Development Council: visiting professor, department of economics and Research Center in Economic Growth, Stanford University, 1964-65.

Resignations

Byung Tack Cho, East Carolina College.

Stanley L. Engerman, Yale University.

Franklin B. Evans, Graduate School of Business, University of Chicago, September 30, 1964.

William L. Furlong, School of Business Administration, Emory University, August 31, 1964.

Richard C. Porter, Yale University.

Howard G. Schaller, Tulane University.

Donald R. Snodgrass, Yale University.

T. N. Srinivasan, Yale University.

Jan V. Tumlir, Yale University.

Andrew B. Whinston, Yale University.

AMERICAN ECONOMIC REVIEW

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PAPERS AND PROCEEDINGS

OF THE

Seventy-seventh Annual Meeting

OF THE

AMERICAN ECONOMIC ASSOCIATION

Chicago, Illinois, December 28-30, 1964

Edited by the Secretary of the Association

AMERICAN ECONOMIC ASSOCIATION

Organized at Saratoga, New York, September 9, 1885

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49. Paul H. Douglas, Chicago, 1947
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51. Howard S. Ellis, California, 1949
52. Frank H. Knight, Chicago, 1950
53. John H. Williams, Harvard, 1951
54. Harold A. Innis,* Toronto, 1952
55. Calvin B. Hoover, Duke, 1953
56. Simon Kuznets, Pennsylvania, 1954
57. John D. Black,* Harvard, 1955
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62. Theodore W. Schultz, Chicago, 1960
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65. Gottfried Haberler, Harvard, 1963
66. George J. Stigler, Chicago, 1964

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Paul T. Homan, 1941-51
Bernard F. Haley, 1952-62

* Deceased.

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Gottfried Haberler, Harvard
George J. Stigler, Chicago

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Abe Fortas, Washington, D.C.

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Edited by HAROLD F. WILLIAMSON, *Secretary of the Association*

and

GERTRUDE TAIT, *Executive Assistant*

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PROCEEDINGS

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PROGRAM OF THE SEVENTY-SEVENTH ANNUAL MEETING OF THE AMERICAN ECONOMIC ASSOCIATION

Chicago, Illinois, December 28-30, 1964

While no general theme underlay the sessions, a major concern was the extension of the applicability of modes of economic analysis.

In the preparation of the program, the President received substantial assistance from the following members of the Association: William R. Allen, G. L. Bach, James Buchanan, Geza Feketekuty, C. E. Ferguson, Franklin M. Fisher, R. W. Harbeson, C. B. Hoover, D. Gale Johnson, Harry G. Johnson, John W. Kendrick, John W. Lehman, Fritz Machlup, Edwin Mansfield, Franco Modigliani, James Morgan, William N. Parker, R. W. Pfouts, George J. Stigler, and James Tobin.

Sunday, December 27, 1964
3:00 P.M.

Executive Committee Meeting

6:00 P.M.

Executive Committee Dinner

Monday, December 28, 1964
8:30 A.M.

Economic History: Its Contribution to Economic Education, Research, and Policy

Chairman: WILLIAM N. PARKER, Yale University

Papers: DOUGLASS C. NORTH, University of Washington; ROBERT W. FOGEL, University of Chicago; BARRY SUPPLE, University of Sussex; RICHARD A. EASTERLIN, University of Pennsylvania; ROBERT E. GALLMAN, University of North Carolina; RONDO E. CAMERON, University of Wisconsin

Discussants: EVSEY DOMAR, Massachusetts Institute of Technology; R. A. GORDON, University of California, Berkeley

Nonmarket Decision Making

Chairman: JAMES BUCHANAN, University of Virginia

Papers: ANTHONY DOWNS, The RAND Corporation; JOHN HARSANYI, Wayne State University; GORDON TULLOCK, University of Virginia

Dicussants: JAMES MARCH,[1] University of California, Irvine; ROBERT L. BISHOP, Massachusetts Institute of Technology; JESSE MARKHAM, Princeton University

10:30 A.M.

Survey Research: Three Surveys—Findings and Implications for Theory and Policy

Chairman: DAN THROOP SMITH, Harvard University

Papers: DOROTHY PROJECTOR, Board of Governors of the Federal Reserve System; JAMES MORGAN, HARVEY BRAZER, and ROBIN BARLOW, University of Michigan; EVA MUELLER, Survey Research Center, and HARLOW OSBORNE, Federal Reserve Board

Discussants: JOHN CULBERTSON, University of Wisconsin; GEORGE F. BREAK, University of California, Berkeley; LAWRENCE E. THOMPSON, Harvard University

Recent Capital and Production Theory

Chairman: HOWARD SCHALLER, Indiana University

Papers: ABBA P. LERNER, Michigan State University; C. E. FERGUSON, Duke University

Discussants: R. W. PFOUTS, University of North Carolina; MARVIN FRANKEL, University of Illinois; ROBERT SOLOW,[1] Massachusetts Institute of Technology

Balance of Payment and Related Problems (Joint session with the American Statistical Association)[2]

Chairman: E. M. BERNSTEIN, E. M. Bernstein, Ltd.

Papers: HELEN B. JUNZ, Federal Reserve Board and RUDOLPH R. RHOMBERG, International Monetary Fund; IRVING B. KRAVIS, ROBERT F. LIPSEY, and PHILIP J. BOURGNE, N.B.E.R. International Project.

Discussants: WALTHER LEDERER, U.S. Department of Commerce; HAL B. LARY, N.B.E.R.

[1] No manuscript received.

[2] Published in the *Proceedings* of the Business and Economic Statistics Section of the A.S.A.

12:30 P.M.

Joint Luncheon with the American Finance Association

Chairman: MARTIN R. GAINSBRUGH, National Industrial Conference Board
Speaker: HENRY C. WALLICH, Yale University

2:00 P.M.

Defense Economics: Applying Economic Criteria

Chairman: DAVID NOVICK, The RAND Corporation

Papers: STEPHEN ENKE, Institute for Defense Analyses; ROBERT N. GROSSE and ARNOLD PROSCHAN, Research Analysis Corporation

Discussants: JAMES R. SCHLESINGER, The RAND Corporation WILLIAM H. MECKLING,[1] Center for Naval Analyses; ROLF PIEKARZ, Institute for Defense Analyses

The Contribution of the History of Economic Thought to the Understanding of Economic Theory, Economic History, and the History of Economic Policy

Chairman: ALFRED F. CHALK, Texas A. and M. University

Papers: DONALD F. GORDON, University of Washington; WILLIAM D. GRAMPP, University of Illinois, Chicago; FRANK W. FETTER, Northwestern University

Discussants: WILLIAM R. ALLEN, University of California, Los Angeles; WARREN J. SAMUELS, University of Miami; WARREN SCOVILLE, University of California, Los Angeles

8:00 P.M.

Richard T. Ely Lecture

Chairman: ROBERT D. CALKINS, Brookings Institution
Speaker: W. ARTHUR LEWIS, Princeton University

Tuesday, December 29, 1964

8:30 A.M.

The Evolving International Monetary Mechanism

Chairman: KARL BOPP, Federal Reserve Bank of Philadelphia

Papers: J. DEWEY DAANE, Board of Governors of the Federal Reserve System; JACQUES J. POLAK, International Monetary Fund; FRITZ MACHLUP, Princeton University

Discussants: MILTON FRIEDMAN, University of Chicago; E. M. BERNSTEIN, E. M. Bernstein, Ltd.; MILTON GILBERT, Bank for International Settlements

Agricultural Structure and Productivity (Joint session with the American Farm Economic Association)[3]

Chairman: GEORGE BRINEGAR, University of Illinois

Papers: WILLIAM H. NICHOLLS, Vanderbilt University, and RUI MILLER PAIVA, Fundăcao Getulio Vargas; BRUCE JOHNSTON, Stanford University

Discussants: D. WOODS THOMAS, Purdue University; CLIFTON R. WHARTON, JR., Agricultural Development Council

10:30 A.M.

Technological Change: Stimuli, Constraints, Returns

Chairman: BURT KLEIN, The RAND Corporation

Papers: EDWIN MANSFIELD, University of Pennsylvania and Harvard University; JESSE MARKHAM, Princeton University; JACOB SCHMOOKLER, University of Minnesota

Discussants: MORRIS ADELMAN, Massachusetts Institute of Technology; ZVI GRILICHES, University of Chicago; RICHARD TYBOUT, Ohio State University

Economic Theory and Nonprofit Enterprise

Chairman: M. A. EGGERS, Syracuse University

Papers: MELVIN REDER, Stanford University; ALLAN CARTTER, American Council on Education; WILLIAM BAUMOL and WILLIAM G. BOWEN, Princeton University

Discussants: MANUEL GOTTLIEB, University of Wisconsin, Milwaukee; SIMEON E. LELAND, Northwestern University; MARTIN BRONFENBRENNER, Carnegie Institute of Technology

Manpower and Welfare Programs: Benefit-Cost Analysis (Joint session with the Industrial Relations Research Association)[4]

Chairman: HERBERT PARNES, Ohio State University

Papers: GERALD SOMERS and ERNST STRONSDORFER, University of Wisconsin; JOHN S. MACDONALD, United Nations

[3] To be published in the May, 1965, issue of the *J. of Farm Econ.*
[4] To be published in the May, 1965, I.R.R.A. *Proceedings.*

Discussants: JUANITA KREPS, Duke University; SELMA MUSHKIN, Advisory Commission on Intergovernmental Relations; BENSON SOFFER, Committee for Economic Development; LELAND S. BURNS, University of California, Los Angeles

2:00 P.M.

Domestic Implications of the Evolving International Monetary Mechanism

Chairman: EMILE DESPRES, Stanford University

Papers: MILTON GILBERT and WARREN MCCLAM, Bank of International Settlements; HERBERT STEIN, Committee for Economic Development; WARREN SMITH, University of Michigan

Discussants: JAMES INGRAM, University of North Carolina; R. A. MUNDELL,[1] McGill University; JAMES TOBIN, Yale University

Economic Growth: International Comparisons

Chairman: WALTER KRAUSE, State University of Iowa

Papers: ARTHUR SMITHIES, Harvard University; SIDNEY KLEIN, Rutgers—The State University

Discussants: W. M. CORDEN, Australian National University; WILFRED MALENBAUM, University of Pennsylvania; M. C. URQUHART, Queen's University

Some Public Policy Issues in Regulated Industries

Chairman: ROBERT W. HARBESON, University of Illinois

Papers: RONALD H. COASE, University of Chicago; MARVIN BARLOON, Western Reserve University; ROBERT A. NELSON, University of Washington

Discussants: H. H. GOLDIN, Federal Communications Commission; W. H. DODGE, University of Wisconsin; W. N. LEONARD, Hofstra University

4:00 P.M.

Utility and Saving (Joint session with the Econometric Society)[5]

Chairman: ROBERT SOLOW, Massachusetts Institute of Technology

Speaker: SIR JOHN HICKS, Oxford University

8:00 P.M.

Presidential Address[6]

Chairman: FRANK H. KNIGHT, University of Chicago

Speaker: GEORGE J. STIGLER, University of Chicago

Wednesday, December 30, 1964

8:30 A.M.

Comparative Economic Systems: Nationalized Industry (Joint session with the Association of Comparative Economics)

Chairman: ROBERT DERNBERGER, University of Chicago

Papers: BEN W. LEWIS, Oberlin College; BENJAMIN WARD, University of California, Berkeley

Discussants: GEORGE MACESICH, Florida State University; HERBERT G. GRUBEL, Stanford University; WILLIAM G. SHEPHERD, University of Michigan; H. EDWARD ENGLISH, Private Planning Association of Canada

The Economics of Poverty

Chairman: MARSHALL COLBERG, Florida State University

Papers: T. W. SCHULTZ, University of Chicago; ROBERT J. LAMPMAN, University of Wisconsin; ALAN B. BATCHELDER, Ohio State University

Discussants: GEORGE H. HILDEBRAND, Cornell University; HARRY G. JOHNSON, University of Chicago; MALCOLM LIGGETT, University of California, Santa Barbara

Graduate Student Session[7]

Chairman: EVSEY DOMAR, Massachusetts Institute of Technology

Papers: G. S. SAHOTA, Vanderbilt University; H. I. GROSSMAN, Johns Hopkins University; M. Z. FABRYCY, City University of New York

Discussants: ROBERT BUSHNELL, Princeton University; LARS SANDBERG, Harvard University; JEROME R. LA PITTUS, Cornell University

Problems of Wealth Estimation (Joint session with the American Statistical Association)[8]

Chairman: RAYMOND T. BOWMAN, U.S. Office of Statistical Standards

[5] To be published in *Econometrica*.
[6] Published in the March, 1965, *A.E.R.*
[7] To be published in *The American Economist*.

Papers: MILTON MOSS, U.S. Office of Statistical Standards; JOHN W. KENDRICK, George Washington University; JOEL POPKIN, U.S. Department of Commerce
Discussants: PATRICK HUNTLEY, George Washington University; RICHARD RUGGLES, Yale University

10:30 A.M.

Invited Student Dissertations
Chairman: JAMES MORRIS, University of South Carolina
Papers: GORDON WINSTON, Williams College; FERDINAND K. LEVY, Stanford University; E. PHILIP HOWREY, Princeton University
Discussants: VINCENT F. BOLAND, University of Arizona; HANS BREMS, University of Illinois; DAVID MCCORD WRIGHT, University of Georgia

The New National Econometric Model: Its Application (Joint session with the Econometric Society)
Chairman: ROBERT H. STROTZ, Northwestern University
Papers: LAWRENCE KLEIN, University of Pennsylvania, and GARY FROMM, Brookings Institution; EDWIN KUH, Massachusetts Institute of Technology
Discussants: MARC NERLOVE, Stanford University; R. A. GORDON, University of California, Berkeley

Economic Education: Experiments in the Teaching of Economics
Chairman: G. L. BACH, Carnegie Institute of Technology
Papers: RICHARD ATTIYEH, Yale University, and KEITH LUMSDEN, Stanford University; MYRON JOSEPH, Carnegie Institute of Technology; SIMON WHITNEY, Rutgers University
Discussants: IRWIN L. HERRNSTADT, Northeastern University; DANIEL FUSFELD, University of Michigan; RENDIGS FELS, Vanderbilt University

2:00 P.M.

Business Meeting

THE purpose of the American Economic Association, according to its charter, is the encouragement of economic research, the issue of publications on economic subjects, and the encouragement of perfect freedom of economic discussion. The Association as such takes no partisan attitude, nor does it commit its members to any position on practical economic questions. It is the organ of no party, sect, or institution. Persons of all shades of economic opinion are found among its members, and widely different issues are given a hearing in its annual meetings and through its publications. The Association, therefore, assumes no responsibility for the opinions expressed by those who participate in its meetings. Needless to say, the papers presented are the personal opinions of the authors and do not commit the organizations or institutions with which they are associated.

HAROLD F. WILLIAMSON
Secretary

RICHARD T. ELY LECTURE

A REVIEW OF ECONOMIC DEVELOPMENT

By W. ARTHUR LEWIS
Princeton University

The underdeveloped countries did reasonably well during the 1950's. According to the United Nations' statisticians,[1] gross domestic product increased at an annual rate of 4.6 percent in Latin America, 4.2 percent in the Far East (excluding Japan and mainland China), 5.2 percent in Southern Asia, and 4.1 percent in Africa. At the beginning of the decade economists were concerned about whether these countries could make the minimum critical effort needed to exceed population growth, then thought to be 2 percent, now accelerating disquietingly to 2½ and 3 percent. Growth rates exceeding 4 percent have shown that capital, entrepreneurship, skill, and foreign trade are not such formidable obstacles as they were thought to be. In reviewing each of these categories, I shall use the occasion to consider some of the concepts which economists have been using to analyse the problems of economic growth.

I. *Capital*

Capital has not been as scarce as expected, first because the capital-output ratio turned out to be unusually low, second, because more foreign aid became available, and, third, because some of these countries are managing to save more.

In 1950, economists thought that the capital-output ratio was around 4 (net); actually in the 1950's it has often been even between 2 and 3, both in developed and in underdeveloped countries. We do not yet know why, or whether the change is permanent. Let us note four possible explanations. First, Western Europe has been exploiting a backlog of technological progress, accumulated during the interwar stagnation. The underdeveloped countries may be reaping a similar harvest—will certainly do so as soon as they tackle their agriculture properly. Second, expenditure on infrastructure may be abnormally low; underdeveloped countries are putting a lot of money into transportation and power, but they are still starving housing and the public services. Third, the high growth rates of industrial production

[1] *World Economic Survey, 1963,* p. 19. The figure for Africa is probably too high. On the other hand an earlier estimate by the Economic Commission for Africa (*Industrial Growth in Africa,* p. 3) that commodity output grew by 2.1 percent per annum is too low, since it assumes that the growth rate of agriculture was only 1.4.

(ranging betw n 6 and 10 percent) and of services have effected considerable transfers from less to more productive sectors of the economy. And, finally, high rates of growth of population, in countries where land is abundant, produce a proportionate growth of agricultural output, using little capital. The question whether in some other countries, such as India or Egypt, the marginal productivity of labor in agriculture is zero seems to arouse fierce passions, though the answer does not seem to be relevant to any practical problem.

When we turn to the transfer of capital from the developed to the underdeveloped world, we are on firmer ground. The United Nations[2] estimates that the net flow increased from about 2 billion dollars in 1950 to about 6 billion dollars in 1960. Six billion dollars was about 3½ percent of the national incomes of the underdeveloped world and was therefore associated with about a quarter of their rate of growth at the end of the decade. This is a very considerable achievement for those of us who have made ceaseless propaganda for foreign aid.

It would be pleasant to be able to report a universal increase in domestic ratios of saving, resulting from these high rates of growth and of aid, but alas only a minority of countries have risen to their opportunity. The best documentation is for Latin America, where if the Economic Commission for Latin America[3] is right, the ratio of gross domestic savings to gross domestic product fell, for the continent as a whole, from 16.8 percent in 1950-54 to 15.6 percent in 1955-61, or 15.2 percent in 1960. Fragmentary evidence suggests that the rest of the underdeveloped world did not do much more than maintain domestic savings ratios (e.g., Philippines from 9.0 percent in 1950 to 9.5 percent in 1960; Nigeria from 8.3 in 1951 to 8.6 in 1957) but there were also spectacular exceptions, such as India, from about 6 percent (net) in 1950 to about 8 percent in 1960, and Jamaica, from 12.2 (gross) in 1950 to 16.1 percent in 1960.

Some of the countries which failed to improve their savings ratios nevertheless increased the take of public expenditure, which can be just as important for growth. The 1950's were a good decade for education, for public health, and for roads. Since both savings and public expenditure come out of the surplus of output over private consumption and improve future productive capacity, they should be considered together when measuring achievement. Availability of foreign capital enabled some governments to put improvement of the public services ahead of improvements in the savings ratio during the 1950's.

[2] *The International Flow of Long-Term Capital and Official Donations, 1959-1961,* and earlier publications in this series.

[3] *The Development of Latin America in the Post-war Period,* p. 10. Domestic saving equals investment minus the surplus on current account in the balance of payments.

We do not have enough evidence to test theories of how income distribution and the propensity to save change as per capita income increases. The fall in export prices relatively to domestic costs was important, since the export sector saves more and is more highly taxed than the rest of the economy. ECLA estimates that adverse terms of trade cost Latin America 3.6 percent of national income, comparing 1960 with 1950, but one would still have expected the savings ratio at least to be maintained, since per capita real output increased by 19 percent. Presumably significant changes in private saving require longer periods and bigger per capita changes.

The slow rate of change of private savings is one reason why more importance is now attached to increasing public saving. This, however, is not easy. First, the marginal rate of taxation is lower than the average in most of these countries; so the percentage share of public revenue falls as national income increases. Radical reforms are required in tax structures if public revenue is even to keep up with public expenditure. Second, where public services are rudimentary, rapidly increasing expenditures on these services are just as important as increased savings and will for some time absorb most of the increase in revenues. Third, the rate of change must inevitably be slow. Attaining self-sustaining growth means reducing the ratio of private consumption to gross domestic product from around 80 to say around 70 percent. Any attempt to reduce the ratio of consumption faster than by about one-half-of-one percent of gross domestic product per year will defeat itself, and also create political unrest. It defeats itself because output cannot be increased without increasing consumption, since growth requires incentives. And it creates unrest because rapid economic growth produces social turbulence, which can be contained only by devoting increased resources to welfare and consumption. My limit of one-half-of-one percent assumes that per capita consumption must grow at least 60 percent as fast as per capita output, in the relevant ranges. Attempts to move faster than this, whether through taxation, inflation, or rationing, are likely to end in riots.

International economic aid is supported by different people for many different reasons. Insofar as its object is to give the big push which creates the opportunity for self-sustaining growth, its success must be measured by the extent to which countries do indeed increase their investment in men and resources faster than current consumption. And if this is the test, then there is something to be said for tying the distribution of foreign aid to actual performance. In another place[4] I have produced a formula for this, making the amount of aid a

[4] "Allocating Foreign Aid to Promote Self-Sustaining Economic Growth," in *Motivations and Methods in Development and Foreign Aid*, Proceedings of the Sixth World Conference of the Society for International Development, 1964.

country receives multiple of

$$\frac{S_{-1}}{GDP_{-1}} - \frac{S_{-4}}{GDP_{-4}}$$

where S = expenditure on gross investment (minus foreign aid) plus current government expenditure (minus defense and welfare transfers), and subscripts refer to years.

Nobody wants to be governed by a rigid formula, but if we want foreign aid to show results in promoting progress towards economic dependence, it should be more closely linked to performance. These issues can no longer be evaded, because the future of foreign aid is now critically in doubt, not only in the United States, but even in France, where aid has hitherto been sacrosanct. Most aid is given for political reasons, and much of the current disillusionment springs only from belated discovery that political aid cannot achieve all that is expected of it. Nevertheless, it has to be admitted that the nonpolitical aid is also distributed haphazardly, whether by bilateral or multilateral agencies; better criteria and better results might win wider support.

II. *Entrepreneurship*

Growth rates exceeding 4 percent suggest that the shortage of entrepreneurship cannot have been the major obstacle it is normally thought to be. It is well known that these countries have no shortage of small-scale entrepreneurship; the desire to make money and the willingness to gamble are endemic. What lacks is the experience of organizing large-scale businesses. Assuming that this lack of experience springs from lack of desire, or from institutional inhibitions, historians have devoted much paper to considering what social and ideological climate spawns successful large-scale enterprise; and the social psychologists have now joined them. The last fifteen years have thrown little light on their theories, except, perhaps, to question the quantitative importance of their problem. Perhaps, too, those politicians were right who said that the end of imperialism and racial subjugation would spark a surprising release of energy.

Meanwhile the shortage of large-scale domestic entrepreneurship has been met to some extent by foreign enterprise. Here the 1950's saw a marked change in the attitudes of governments. During the 1930's and 1940's the air resounded with diatribes against foreign investors, but in the 1960's newspapers in Europe and North America are full of advertisements in which the governments of newly independent states offer foreign investors innumerable incentives, including exemption from taxes. Foreign entrepreneurs have not been allowed to do much

for agriculture or for trade, but their contribution in mining and manufacturing is a major explanation of the high rates of growth of industrial production.

The shortage has also stimulated governments to assume some of the attributes of entrepreneurship themselves. The governments of underdeveloped countries do not command either the capital or the administrative skills necessary to play any significant role as managers of industrial or agricultural enterprises, though some have tried, often for ideological reasons. They can play a more important role in creating a favorable climate for private entrepreneurship, helping with improved infrastructure, market research, feasibility studies, technical advice, and financial aid. Development theory makes a great deal out of external economies, whether in explaining the low level of investment, or assessing the advantages of geographical concentration, or tracing the history of growth through linkages, or arguing the case for the government as a promoter of interdependent investments. The analysis has been influential, though factual evidence remains scarce.

Governments have done better at stimulating the private sector than at controlling it, not surprisingly, since both the statistics and the personnel for efficient control are lacking. Many controls have hindered more than helped, especially by restricting the smaller businessmen, some of whom are the hope for the future. Also, the new states often begin with hostility between politicians and administrators, and need to find a new equilibrium which will reduce corruption and arbitrary decisions. The record is spotty even in the public sector. Most governments have made development plans, but few take their own plans seriously, although planning could undoubtedly help to bring more order into public sector programs. Governments have first to learn to control the public sector before they can hope usefully to control the private sector.

The sector to which government initiatives could contribute most is agriculture, which has, alas, been the most neglected. Everybody talks about the necessary framework for agriculture—the agricultural research stations, the extension agents, farm institutes, animation, water supplies, fertilizers, land reform, and so on—but little gets done. Prime ministers have had their minds on other things: on political questions such as neutralism, Pan Africanism, Afro-Asian unity and the like, or in the economic realm mainly on industrialization; and since most great men can achieve no more than one or two things at a time, agriculture has had to content itself with occasional lip service. Agriculture will have to be seen to be important before any considerable progress will be made.

This change is beginning in countries where agricultural stagnation

is the obvious c ,se of the shortage of foreign exchange. But the crucial role of agriculture is also obvious in the elementary arithmetic of economic growth. For it is easy to show that national income cannot attain a rate of growth of 5 percent in Asian and African conditions. At present none of these countries is able to increase agricultural output faster than 3 percent per annum. Assume optimistically that industrial output increases by 10 percent per annum. Then, if agriculture contributes 50 percent of gross national product and industry 12 percent, the growth of commodity output averages out, initially, at 4.4 percent. Output of services grows slightly faster than output of commodities; so gross domestic product as a whole would grow at a maximum rate of 4.6 percent. Given the failure to reform agriculture, the rates of growth actually achieved in the 1950's are a matter for congratulation. One must assume that the United Nations call for 5 percent in the 1960's is mainly a call for a massive assault on agricultural stagnation, without which such a target is impracticable. Also, the call should not be mixed in with the discussion of foreign aid, since it is with the governments of the underdeveloped countries that the initiative lies in agriculture.

III. *Skill*

Shortage of skills has been even less of a bottleneck than shortage of entrepreneurs. In part this has been due to the large international flow of technical assistance. Despite the break-up of the colonial empires, there are now more European and American technicians in Asia and in Africa than there were in 1950, and the numerous channels through which this movement is organized are an achievement in which the world can take some pride.

Nevertheless, technical assistance is marginal. Leaving aside for the moment the agricultural sector, the amount of skill which these countries can currently absorb is relatively small, mainly because their nonagricultural sectors are relatively small. Where half the population is in agriculture, the number of jobs requiring a secondary education does not exceed 10 percent of the occupied population, nor do the jobs requiring university education exceed 2 percent[5]. The majority of countries in Asia and Latin America (but not all) have as many secondary educated people as they need for strictly economic purposes, and many of them have many more such people than they can absorb in current market conditions. There are shortages of particular types. In general there is too much literary education and not enough technological, but this defect is easily remedied. The number of technical

[5] For amplification of this statement see my articles "Education and Economic Development," *Social and Economic Studies,* June, 1961, and "Secondary Education and Economic Structure," *ibid.,* June, 1964.

institutes and special training institutions has multiplied quite rapidly in underdeveloped countries. What most of the Asian and Latin-American countries need is better quality rather than more quantity.

Africa's situation is quite different. There in 1960 less than 1 percent of the population had received a secondary education and less than 10 percent primary education. This situation is now changing rapidly since these countries have become independent and have made education a first priority.

There is no evidence to suggest that economic development is accelerated by supplying more educated people than the market can absorb. India is one of the best educated underdeveloped countries, but not conspicuously the most successful in economic development. An oversupply of educated people creates great frustrations, stimulates excessive migrations to the towns, and results in political turbulence. All this makes the political situation more exciting; but though the long-run effects of political excitement on development may be positive, the short-term effects seem to be zero, or even negative.

The biggest problem in education is the relation of schooling to agricultural improvement. The problem does not arise in plantation agriculture, since the planters, who are the decision-makers, can adopt the latest techniques whether their workers are literate or not. We cannot doubt that literate farmers are likely to absorb new technology more rapidly than illiterate farmers. Illiterate farmers can be taught the most obvious things and have made important decisions, such as changing from subsistence to commercial crops. But it must be a goal to have as soon as feasible an agricultural system in which every farmer is literate.

The problem is the transition. Putting the children into school, which costs a great deal of money (as much as 3 or 4 percent of national income) does not guarantee that one will have literate farmers. Much depends on the speed with which one moves from 10 percent of rural children in school towards 100 percent, for when the figure is only 10 percent, that 10 percent knows that it will get jobs off the farms, at wages from two to three times as high as the average farmer's income. If one raises the proportion to 60 percent within ten years, as some West African countries have done, the 60 percent also expect to get jobs off the farms at high incomes. They cannot be attracted by three acres and a hoe; only a modernized agriculture capable of high yields per man could hope to hold them. Hence the only way to effect a smooth transition is to keep the rate of modernization of agriculture and the rate of expansion of rural schooling in step with each other. This is not a case for less schooling so much as a case for faster modernization of agriculture—for greater expenditure on research, agricultural credit, water supplies, and so on.

Given the time it takes to organize the right kind of agricultural framework for small farming, expenditure on adult education in the countryside, including short courses for farmers at residential centers and other forms of community development, will probably at this stage prove more productive in Africa than money spent on getting all rural children into primary schools, desirable as that is for innumerable reasons, political and social no less than economic.

IV. *Foreign Trade*

Foreign trade has played its customary role as the engine of growth in most of these countries but not in all. Both the terms of trade and the rate of growth of trade have been high by comparison with prewar statistics.

The terms of trade for primary as against manufactured products averaged higher in the 1950's than at any time in the preceding eighty years. The first half of the 1950's was especially good because of the Korean war and heavy stock-piling in the United States and elsewhere. The terms of trade deteriorated in the second half of the decade and on till 1962, since when they have moved upwards. However, even in 1962 they were 5 percent above 1929, which preceded the Great Depression.

The volume of exports from underdeveloped countries increased at an annual rate of 3.6 percent, which is also much higher than before. But this rate is lower than the growth of national income because of the important part played by import substitution, especially in Latin America.

We are being deluged with literature arguing that the underdeveloped countries cannot grow at an adequate rate if the developed countries increase their demands for the exports of underdeveloped countries at an annual rate as low as 3½ percent. Such literature ignores the important part played by import substitution in economic development. Most of the calculations assume rates of growth of national income, consumption, industrial production, and imports which are not mutually consistent. A little arithmetic shows that if one assumes continuance of the current rate of growth of industrial production, the underdeveloved countries must soon be supplying themselves with nearly all the manufactures they need.[6] They have the necessary minerals

[6] Currently these countries use manufactures equal to about 30 percent (including raw materials) of national income, of which about one-third are imported. If national income grew at 5 percent per annum, use at 6 percent, and production of manufactures at 8 percent, net imports of manufactures would fall to zero in twenty-two years. If the growth rate of production of manufactures is assumed to be lower, so must the growth rate of national income, and therefore of use of manufactures. Considering how slowly agriculture is growing, national income cannot grow by 5 percent if manufacturing is growing by less than 8 percent.

and fuels, and the skills are not hard to learn. It is better that they should not be forced into autarky since comparative advantage demands that, even if net imports be low, this be only because large imports of some manufactures are matched by large exports of others. Recent international discussion has focused rightly on the market for the manufactures of underdeveloped in developed countries; this is likely to be much more important in future than the never ending talks on primary products. Some of us believe even that the time is not far off when the underdeveloped will be net importers of primary products and net exporters of manufactures. This is not only because of high population growth, which the new techniques of family limitation will soon begin to control. More fundamentally, it is arguable that the real competitive advantage of temperate countries is in agriculture, since their temperate climates are more favorable to the retention of soil fertility than the harsher climates of the tropics.

To the individual underdeveloped country, import substitution offers less opportunity for growth with diminishing trade than it does to the group taken as a whole. Only the largest countries (possibly China, India, the U.S.S.R. and the United States) have that wide variety of climates and minerals which is a necessary condition for development as a closed economy. Everywhere else a rise in real income must increase the imports of some commodities. These imports have to be paid for either by expanding exports or by releasing foreign exchange through import substitution. In the nineteenth century, growth was sparked by exports, which generated incomes and so also stimulated production for the home market. In the second half of the twentieth century, import substitution has offered some countries an easy path to growth, without dependence on increasing exports. However, once the limits to import substitution are reached, the rate of increase of exports sets a ceiling to the rate of growth of output, since natural resource limitations make it impossible to have balanced growth for the home market only.

Economists have produced a turgid literature on balanced growth. Most writers do not make clear whether they are concerned with the balance between production for the home market and production for export or are concerned only with production for the home market.

If one assumes a closed economy, the pattern of production must obviously be related to the pattern of domestic demand. The original Rosenstein-Rodan proposition was to the effect that in a closed economy with unemployment, an entrepreneur could employ some of the unemployed with greater confidence if he knew that other employers were simultaneously offering employment in other industries; for his workers would spend some of his outlays on buying their goods, but

their workers would spend some of their incomes on buying his. A parallel proposition in the two-sector model says that if the two sectors trade with each other, the expansion of the one can be brought to an end by the stagnation of the other. These propositions have not been upset by any of the subsequent writing, most of which has merely demonstrated that, while demand and supply are linked, the link is not absolutely rigid. Some production can take place in advance of demand; innovating suppliers create demands which did not previously exist; in other sectors it is safe for capacity to lag behind demand; the availability of entrepreneurial talents will decide where demand may lag, and where it may go in advance; inventions usually bunch in a few industries, in response to shortages, rather than spread all over the economy; and so on. Much controversy has raged around such propositions, not because they are controversial, but because economists like to be controversial.

Maintaining a proper balance between production for the home market and production for export is a much more important subject, because failure to do this has serious consequences for the balance of payments, for prices, and for the growth rate of the economy. Attempts to increase output faster than the growth rate of exports will support give rise to what the Latin Americans now call "structural inflation." This literature, too, is much obscured by irrelevancies. Structural inflation is not due to being underdeveloped, to having a high rate of population growth, to exporting mainly primary products, or to having difficulty in raising taxes; the British economy is the clearest contemporary example of structural inflation, and it has none of these characteristics. Neither is structural inflation due to investing more than is saved, or to running a budget deficit; these cause demand-pull inflations, whereas structural inflation is a cost-push phenomenon.

A simple example illustrates the species. Suppose that a country produces only two commodities, motor cars and wheat, and consumes only these two commodities. Suppose also that through technological progress the output of cars is increased, but structural barriers hold down the output of wheat. The increase in real income increases the demand for wheat. This is met by importing wheat. Thus income generated in the production of cars is used to buy imports; there is a deficit in the balance of payments, and deflation in the home market, represented by a surplus of cars. This structural deflation is turned into structural inflation by the measures taken to right the balance of payments. Devaluation or tariffs or import controls will raise the price of wheat and therefore the cost of living, and so a cost-push inflation spiral will start. Four solutions are possible. One solution would be to reduce the output of cars to the level of home demand. This reduces employment

and damps the rate of growth of national income. We can call this the British solution, since this is what Britain has done regularly over the past fifteen years; it is also what Latin-American economists accuse the International Monetary Fund, rightly or wrongly, of wanting them to do. The second solution would be to go out into the world market and sell more motor cars. This is the Japanese solution. The rate of growth of the economy is then set by the degree of success in exporting. The third solution would be to break the agricultural bottleneck and have balanced growth in the narrow sense of patterning domestic production on domestic demand. This is the Mexican solution. The fourth solution would be to force the public to consume what is being produced; namely, more cars rather than more wheat, whether by subsidizing cars, or by taxing wheat, or by some system of rationing. This was the old Russian solution.

Only two of these solutions appeal to economists; namely, either to sell more exports or to break the bottleneck. Nothing in economic science can tell us a priori which is preferable, but great passions have been aroused by our prejudices. Economists reared in the free-trade tradition tend to look first for the opportunities for increasing trade; whereas a later generation, which learnt in the interwar years to be wary of dependence on exports, prefers to explore first the opportunities for increasing the productivity of home supplies. Inefficient governments chose neither of these solutions. They either damp down growth or try to push ahead despite imbalances. The latter policy has produced its full crop of foreign exchange shortages, devaluations, and inflations. The process is cumulative; persistent cost-inflation discourages both exports and import substitution, so aggravating the foreign exchange shortage, and giving the spiral another push. Whatever merits a policy of persistent inflation may or may not have in a closed economy, in an open economy it can work havoc with employment and growth, by causing a country to price itself persistently out of world markets. A perpetually overvalued currency can be the chief cause of economic stagnation.

One of the advantages of input-output analysis is that it puts these problems into their proper framework. A projected increase in national income results in projected increases in final demands. In balancing demand and supply for each commodity separately, the law of comparative costs is invoked (perhaps in the form of linear or preferably nonlinear programming) to decide the appropriate balance between imports, exports, and production for the home market. Economists are asking whether the new "indicative planning" has really contributed anything to the rate of growth of the French economy. In less developed countries the making of such projections could not but improve

existing plan..ng procedures, so long as good statistics are available and so long as projection is not confused with prophecy. I suspect that if such exercises were done correctly, most of the countries which have been concentrating on the home market would find that a shift to exports would pay, and most of those now concentrating on exports could do better by giving more attention to import substitution.

V. *Unemployment*

One disturbing factor must be set against the high rates of growth of output and investment on which we have been congratulating ourselves; namely, the rising levels of unemployment in the underdeveloped countries[7]. This cannot be documented because there are no reliable statistics of unemployment, but it is everywhere a cause of concern. The phenomenon is unexpected, since rapid growth and high investment ought not to increase unemployment but to reduce it.

This unemployment is not due in the first instance to rising populations. In a well-organized society, surplus population shows itself in disguised unemployment on family farms and in other traditional places, whereas what we are now talking about is unemployment of people working for wages and living in big towns.

The simplest approach to understanding the causes of unemployment is through the model which divides the economy into a growing modern capitalist sector and a traditional subsistence sector which feeds labor to it as required. Unemployment is growing rapidly for two reasons: first, because the traditional sector is expelling labor too rapidly; and, second, because the modern sector is taking in too few because it is too highly capital intensive.

One reason why the traditional sector is discharging labor too rapidly is the unusually large differential between wages in the modern sector and earnings in the traditional sector. We are used to assuming a differential of about 50 percent, and to assuming that the modern sector can grow at a constant wage level. This seems to have happened in the nineteenth century, but in the twentieth century trade-union pressure, nationalistic governmental pressure on foreign enterprises, and the new social conscience of big entrepreneurs are combining to raise wages very sharply in the modern sectors of the developing economies, and it is now not unusual to find some unskilled workers in the modern sector earning three or four times as much as the average small farmer. This causes a sympathetic increase in wages in traditional occupations, and since productivity is very low in these occupations, employers

[7] I have treated this subject more fully in "Unemployment in the Developing Areas" in *Proceedings of the Third Biennial Midwest Research Conference on Underdeveloped Areas* (Chicago, 1965).

get rid of domestic servants and of the surplus clerks and messengers whom their businesses have traditionally sheltered. The high wages in the modern sector also attract people out of the countryside into the towns, where they manage to live by doing a few hours occasional work per week.

Imbalance between the modern and the traditional sectors is not confined to wages. An important factor in Africa is the rapid acceleration in the output of rural schools, which are now producing more primary school graduates than the rural economy is able to absorb at the wages they expect. So young people are flooding into African towns. The excessive rate of growth of a few towns is a problem throughout the underdeveloped world. This is compounded by errors of policy in industrial location and by concentrating development expenditures on these few large towns, making them much more attractive than the villages and the small country towns in terms of water supplies, transportation, schools, hospitals, electric light, and opportunities for unemployment relief. Holding the surplus in the countryside until required has always been a problem in developing economies. The British poor relief authorities gave it much attention at the end of the eighteenth century and the beginning of the nineteenth, and tried to prevent people from coming into the towns if work was not available.

The other aspect of this problem is the high capital intensity of the new investment, not in the sense of the ratio of capital to output, which we have seen is low, but in the sense of the proportion of the national income invested which is required to provide additional employment for one more man. Most of these countries are surprised at how few people have found employment in the growing sectors of the economy, especially manufacturing, mining, and transportation, despite the high investment which has been taking place.

High capital intensity is appropriate when it embodies greatly superior technology, without demanding very high skills. The Ohlin approach to comparative costs puts us against high-capital intensities, but Ohlin's model assumes that countries have the same technology and differ only in the relative scarcity of resources. In comparing developing with underdeveloped countries, it is more appropriate to use the Ricardian version of the law of comparative costs, which stresses instead relative differences in productive efficiency. Now there is no a priori reason for developed countries to have a comparative efficiency advantage in capital intensive industries, and one can easily construct cases where the comparative advantage remains with the underdeveloped country, even when the relatively higher cost of capital is taken into account. Since this result is unfamiliar, I am attaching an arithmetical example as an appendix to this paper.

Economic theory offers no reason why development must increase rather than reduce employment. Capital investment as such must increase employment in a system with an infinitely elastic supply of labor, since it cannot pay in such a system to increase the ratio of capital to labor. Just as important as capital investment, however, is the introduction of new technology. This may operate in either direction, but on balance tends to be laborsaving. Karl Marx asserted that the employment-destroying effects of new technology must more than offset the employment-creating effects of capital investment, but he produced no arguments in favor of this proposition, and proved to be wrong in the nineteenth century. It does not follow that he must be wrong in the twentieth century. The underdeveloped economies lag so far behind in technology, that the opportunities for introducing laborsaving methods are immense, and it could well happen that the new employment created in the factories, in modern forms of transportation, and in modern services could be more than offset by the employment destroyed in handicrafts, traditional forms of transportation, and old-fashioned types of personal service.

This possibility is heightened by the tendency of wages to rise sharply in the modern sector. The higher wages are, the more it pays to import cheap machinery from the developed countries, and therefore the less employment investment creates. Not only are the newest industries highly capital intensive, but some of the well-established older export industries, such as mines and plantations, are finding themselves squeezed by their inability to pay the wages that are being demanded and offered by more profitable enterprises; and the opportunities for import substitution are also diminished. Professors Liebenstein and Galenson have urged upon us the desirability of high capital intensity in underdeveloped countries as a source of profits and therefore of savings and investment. Unfortunately, that policy would be feasible only if the labor surplus remained disguised and could therefore be ignored; it is not practicable when the unemployed are roaming the streets and burgling your houses. Most developing countries have to give the highest priority to providing employment now rather than to maximizing consumption or income or employment in ten years time.

Economists have also developed a theory of the desirability of assessing the real social value of a project by calculating with shadow factor prices differing from the actual factor prices. In the situation described, they would recommend calculations in which the existence of unemployment is recognized by attributing a low wage to labor, below the actual wage. This is arguable on paper, but how does one translate it into practice? Investment decisions are made, not by econ-

omists making calculations in government offices, but by private decision-makers and by civil servants, all of whom are under pressure to produce at minimum costs in money terms, and the government does not possess the resources with which to subsidize labor so as to bring the wage down to the shadow price, or the administrative capacity to substitute an effective licensing system for the price system of the market. The only way to achieve decision making on the basis of a low shadow wage is to have a low actual wage.

Recognition of the connection between wages and employment has opened up a gulf between trade-union leaders and political leaders in new states, especially where the government is the chief employer of labor, or is concerned about the adverse effects of high wages on exports, import substitution and employment, or even prefers high profits to high wages because it can tax profits more easily than wages. Governments have therefore begun to think in terms of an incomes policy. Minimization of unemployment requires that wages should be tied to average agricultural incomes at a level sufficiently high to produce the labor required by the expanding nonagricultural sector, but not so high as to produce a great outflow which the towns cannot yet absorb. If at the same time vigorous measures are raising agricultural productivity, mass consumption will rise sharply, not merely because people are transferring from the lower level of the traditional sector to the higher level of the modern sector, but also because both levels are rising. Without some such policy, development must result in sharply increasing unemployment.

We are back in the political sphere. Trade-unions are not likely to accept an incomes policy from governments in which they have no confidence, whether because the politicians are corrupt, indifferent, reactionary, or inefficient. Economists in the twentieth century usually call upon governments to redress the imperfections of the market, just as their forebears in the nineteenth century looked to the market to replace the imperfections of the government. The last fifteen years have lengthened the list of things which governments can usefully do and improved the statistical and theoretical tools for making decisions, but only a handful of governments show promise of rising to their opportunities. Here the economist must hand the development problem over to his colleagues in the other social sciences.

APPENDIX

1. Assume that before "Libya" adopts new technologies, the position is that one man produces

in "Germany" 20 steel or 3 baskets
in "Libya" 1 steel or 1 basket

Therefore "Libya" specializes in baskets.

2. Now introduce new technology to Libya. Assume that the factor proportions are the same in both countries, in physical terms, namely

in steel 1 man + 1 capital
in baskets 1 man + 0.25 capital

3. Suppose, however, that capital makes much more contribution to steelmaking than to basketmaking. The former is a simple operation, which Libyans can do as well as Germans, given the capital; but the latter (basketmaking) is a skilled occupation, in which the Libyan output is still far short of the German, even with capital. The position is now that one man, with the appropriate capital produces

in Germany 20 steel or 3 baskets
in Libya 16 steel or 1.75 baskets

4. The difference is now smaller in steel than in baskets, but we cannot tell which should specialize in which without the relative prices of labor and capital. Suppose that labor costs one mark in Germany, one franc in Libya. Suppose that a unit of capital costs, on a rental basis, 2 marks in Germany and 3 francs in Libya. The relative cost of capital is higher in Libya because (*a*) it takes more labor to make a unit of capital, (*b*) capital maintenance is more expensive, (*c*) capital has a shorter life, and (*d*) the rate of interest is higher.

5. Costs per unit can now be calculated. They are

Germany, steel 0.15 marks; baskets 0.50 marks
Libya, steel 0.25 francs; baskets 1.00 francs

Hence steel is relatively cheaper in Libya than in Germany.

6. The moral is that the Hecksher-Ohlin test (relative scarcity of factors) gives the answer by itself only if the production functions are the same (i.e., if Ricardian differences are absent). In normal cases one must combine Ricardo and Ohlin to get the right answer.

7. The right answer will favor capital intensity in the countries which are short of capital if capital can be used without skill. As Leontief says, the real superiority of developed countries is not in capital intensive but in skill intensive industries, and though these two categories overlap, they are not identical.

ECONOMIC GROWTH: INTERNATIONAL COMPARISONS

ARGENTINA AND AUSTRALIA

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Introduction

Argentina and Australia are both in the temperate latitudes of the Southern Hemisphere, both are inhabited by populations of European stock, both have endless expanses of agricultural land, but more than a third of their populations are metropolitan. Both have highly ambivalent feelings about the older and more populous Northern Hemisphere. They clearly invite comparison with each other. When I embarked on this paper I had expected to use these obvious similarities as a background for startling contrasts. The paper, in fact, has turned out rather differently. The contrasts come in the preface and the conclusion. The main part of the story records similarities more than differences.

In the spirit of the times, the focus of my attention will be the level and growth of GNP and per capita GNP. But not entirely in that spirit, population increase deserves equal attention as a variable depending on the working of the economic and political system. Immigration has played a vital role in both countries, and whatever may be said about natural rates of increase, immigration has been a matter of conscious policy and has responded to the demand for labor in each country. But more than that, it has also been a factor in stimulating the demand for additional immigrants.

Australia has always been aware of the political need to populate its vast estates lying on the edge of overpopulated Asia. But pursuit of its population objective has been constrained until recently by its insistence that its immigrants should be British. From 1810 on, political leaders in Argentina felt the need to increase population and to dilute the country's Spanish blood as an assertion of its independence. The constitution of 1853 actually gave the foreign born privileges over the native born, such as exemption from military service.

The emphasis on population has meant that the countries have been as much concerned with their total national products as with their per capita achievements. But the latter question has been by no means ignored. Emphasis on high wage rates has limited Australian immigra-

tion throughout, and it has helped to stop migration into Argentina in the postwar period.

I am getting ahead of the story. In telling it, I am largely at the mercy of the statisticians but have refrained from reporting numerical conclusions that do not seem reasonable on more general grounds. My main sources have been the ECLA study on the Economic Development of Argentina, N. G. Butlin's monumental research on Australia, and, of course, Colin Clark's *Conditions of Economic Progress*, together with the official statistics in both countries. The main figures I have extracted from these sources are included in the appendix.

The Beginning of the Century

Since the ECLA figures begin with the period 1900-04, I take that as the starting point. In that period Argentina had a population of nearly 5 million and Australia of a little over 4 million. Colin Clark estimates that in that period the gross product of Australia was 1,370m I.Us. and a per capita output of about 350. Projecting Clark's later estimates back by using the ECLA figures, I estimate that Argentina had a total product of 980m I.Us. and 200 per capita. In short, these indicators suggest that, at the beginning of the century, Australia was ahead in both respects.

Another calculation, equally hazardous, supports this conclusion. ECLA estimates that in 1955 the Argentine gross product amounted to about 11 billion 1950 U.S. dollars. I am inclined to think that in 1955 the Australian pound was still somewhat undervalued. Using an exchange rate of 2.50 instead of the official rate of 2.20, and deflating from 1955 to 1950 by the U.S. implicit prime deflator, I put the Australian gross product at about 10 billions. The growth rates of the countries, to be discussed later, then indicate that the Argentine product was about 70 percent of the Australian at the beginning of the century.

How did this difference come about? One can say with some confidence that in 1788, the year the First Fleet landed in Botany Bay, Argentina was well ahead of Australia, and the early years of the Australian settlement were not auspicious. However, during the next half-century Argentina was torn by internal convulsions, while Australia enjoyed a fabulous gold rush, and, more importantly, its own MacArthur developed the merino sheep. Even Butlin, who is not given to overstatement, is prepared to say that by mid-century Australia had the highest living standard in the world.

During the second half of the nineteenth century, both countries increased their populations, with extensive immigration, three- or fourfold. But, again, Argentine history was marked by economic and politi-

cal instability. Australia, on the other hand, had an extraordinary investment boom, which enabled it not only to provide for its rapidly increasing population but also to raise its per capita product to a modest extent. A substantial part of the investment resources came from Britain. At that time, political conditions elsewhere, including Argentinian, made Australia appear a particularly safe haven for British investment.

One should not get the impression that Australia followed a particularly coherent and well-designed policy at that time. The two major colonies, New South Wales and Victoria, pursued free-trade and protectionist policies, respectively, and both seemed to do about equally well. Much of the public investment, especially in railroads, was ill designed, and much private investment went into speculative building. Nevertheless, progress was made; and without the benefit of statistical comparison, it seems entirely possible that Australia, despite its short history, had accumulated more capital by 1890 than had Argentina.

In fact, the conclusion is borne out by such statistical evidence as there is. According to the ECLA study, the capital output ratio of Argentina at the beginning of the century was about 4.1. Estimates by J. M. Garland and R. W. Goldsmith indicate that the ratio for Australia was about 4.3. In other words, the discrepancy between the capital stocks of the two countries corresponded to the income discrepancy.

Two footnotes on the Australian expansion that are of general interest: First, the expansion took place under conditions of generally declining prices—something that would be regarded as highly improbable under contemporary conditions. According to Butlin's deflators, prices were generally at least 10 percent lower in the 1880's than in the 1860's. Second, increasing export income was not a main propelling factor in the expansion. Increases in primary production were offset by falling export prices. Although export earnings increased rapidly during the 1860's and early 1870's, they showed only a slight upward trend for the remaining twenty years of the period. The main expansionary force was public and private investment in Australia, undertaken by both Australians and foreigners. The main role of export income, in conjunction with capital inflows, was to pay for needed imports. It must be remembered, however, that a large part of the investment undertaken was in the primary industries and in the transportation that served them. This presumably would not have been undertaken but for Australia's prospects as an exporting country.

Returning to comparisons between the two countries, there is no reason to suppose that differences in capital accumulation were the only factor making for income differences. Differences in physical endowments and human abilities may have also played a part. Australia's

pastoral and agricultural resources are clearly greater than Argentina's, but both countries have very low population "densities." It seems doubtful whether either of them has encountered diminishing returns to land. Colin Clark's figures indicate that wheat yields per hectare were about the same in both countries. In the mineral area, however, Australia had a decided advantage. I have no direct evidence on whether Anglo-Saxons are more effective workers or better managers than Latins. An Australian trade-unionist, however, would be vehement in his assertions that Italians—in fact all "foreigners"—work too hard.

There is, however, a piece of indirect evidence that I present for what it is worth. If it is true that the capital-output ratios of the two countries were about equal and if they were on the same (Douglas) production function, then one would expect their total outputs to be proportional to their work forces (populations). In fact, Argentina had a larger population and a smaller total output than Australia. On that basis, one can conclude that labor and capital are less effectively employed in Argentina. This may be due to inferior natural endowments, management or labor skills, or labor utilization, especially in agriculture.

The year 1890 was the end of the century for both countries so far as economic expansion was concerned. The last decade was one of serious depression for both. I have no figures for Argentina, but Butlin's Australian figures suggest that compared with it, the depression of the 1930's was relatively mild. While the depression was worldwide, Butlin maintains that misallocation of investment in Australia left the economy particularly vulnerable. The long expansion was not executed according to a coherent plan. But given time and an expanding economy, misallocation tends to cure itself.

By the beginning of the twentieth century the distribution of production of the two economies was not dissimilar. Primary industries, including mining, accounted for over 30 percent of the gross domestic product, while manufacturing and construction amounted to about 20 percent. Statistics of other production categories are not arranged in a comparable manner.

The major differences appear to have been in the ratios of capital to labor or in productive efficiency. The differing capital-labor ratios also imply that, while the total shares of property and labor may not have been widely different, the level of real wages per man in Australia was considerably higher than in Argentina. The early rise of trade-unionism in Australia compelled it to compromise throughout its development between fostering immigration and maintaining high levels of real wages at home. We shall see that this problem confronted Argentina at a much later stage of the proceedings.

Expansion 1900-29

For the quinquennium 1925-29 the gross domestic product of both countries, estimated in the same way as before, was about equal and stood at 3000m I.Us. The population of Argentina had risen to about 11 million and that of Australia to about 6 million. The per capita product of Argentina was thus about 270 I.Us. and of Australia about 500.

Clark's estimates, however, show a much faster rate of growth for Australia than do Butlin's. Comparing the rates of growth of Butlin's series with the ECLA series, the gross product of Australia grew 91 percent during the period while that of Argentina increased 210 percent. Their populations grew, respectively, 56 percent and 120 percent. Australian per capita output grew by 23 percent, while that of Argentina grew 35 percent over their entire period.

Even though Argentina may well have exceeded Australia in rates of increase, its per capita output remained well below that of Australia.

The Argentine expansion up until World War I must have been one of the most dramatic episodes in the history of development. Immigrants poured into the country at the rate of over 250,000 a year in some years. By 1914, about 30 percent of the population was foreign born. Foreign capital flowed at an equally impressive rate and accounted for over 40 percent of total investments. From 1905 to 1914 total investment amounted to 45 percent of gross product, which in turn grew by about 80 percent in a decade. The investment was widely distributed, but the largest portion went to housing and commerce, with the rest being distributed among transportation, manufacturing, and agriculture. Exports increased by 50 percent during the period but were less important than domestic investment as the demand stimulus.

In comparison, Australia had a sluggish period, especially in the first years of the century. It was an exporter of both capital and people, and net immigration remained modest throughout the period. Why capital emigrated after 1900 but immigrated during the late 1890's I cannot make out. The export of capital, however, took place in the context of increasing export income and less rapidly increasing imports. We have no information about the gross flows; so it is impossible to discuss the attitude of long-term investors. Nevertheless, it is clear that Australian governments were not willing to borrow abroad or foreigners were unwilling to invest in Australia to an extent that would convert the current account surplus into a deficit. There is evidence that the declared intentions of the Australian Labor Party had little appeal to foreign investors. The country developed on its own resources and the impelling factors were increasing export income and probably the new protectionist policy established on a nationwide basis

after the founding of the Commonwealth in 1901. Despite the lack of capital from abroad, the gross product increased 45 percent between 1900 and 1914.

The outbreak of the World War I brought development in both countries to a halt. In both countries gross domestic product was lower in 1915-19 than it had been during the previous five years. In Argentina the cause seems fairly clear. Both foreign investment and immigration fell to very low levels, and these declines could easily account for the decrease in domestic investment. The reasons for the Australian decline are less clear, since those factors were not particularly operative. My guess is that a basic reason may have been the lack of imports of equipment needed for expansion—even though the value of total imports did not decrease. This must also have been a factor in Argentina. Furthermore, neither country was in an industrial position to undertake extensive import replacement, nor apparently were they called upon to supply goods for the war. In Australia even pastoral production declined during the first two years of the war.

After the war both countries resumed expansion, with increasing export income as a main stimulus in the initial postwar years. But prosperity was sustained, particularly in Argentina, to the end of the period by domestic investment, undertaken both by nationals and foreigners. Immigration to Argentina never reached anything like its prewar dimensions, and Australia's efforts to encourage immigrants and settle them on the land were a dismal failure.

Over the period both countries had reduced the importance of primary production in their economies and had increased that of industry. Australia had moved further in that direction, probably as a result of its protectionist policy. Argentina had maintained a liberal import policy throughout.

Looking at the period as a whole and very generally, the dominant impression one gets is one of scale expansion of both economies. In the Argentine case the expansion was dramatic; in the Australian modest. The decisive factors accounting for the difference seem to me to be immigration and foreign investment. Argentina had more of both. I see no reason to believe that had Australia followed the same wage and immigration policies as Argentina, its growth would have been as rapid or even more rapid.

Depression and War, 1930-45

The depression of the 1930's was regarded in both countries as the worst misfortune that had befallen them. Memories were too short to go back to the depression of the 1890's, and looking at the scene with the cold eye of an aggregationist, the Australian gross product decreased

less than 3 percent between 1925-29 and 1930-34 and that of Argentina showed only a slight tendency to decrease. By 1935-39, the Australian had increased by nearly 12 percent and the Argentine by nearly 20 percent over the predepression figure.

Both countries suffered sharp reductions in export income. Immigration ceased or reversed itself. The flow of foreign capital stopped abruptly. The balance of payments of neither country was in a state to permit increased foreign indebtedness. The impact in Australia can be judged from the fact that in the last two years of the twenties, its increase in foreign indebtedness was just about equal to its gross domestic investment.

Argentina turned to an avowedly protectionist policy. Australia continued its high tariffs, and in addition was forced at the outset of the depression to devalue its currency by 25 percent in relation to sterling. Furthermore, by now both countries had acquired an economic base for industrialization, which was not the case when they suffered the impact of World War I.

Shortage of necessary imports, however, retarded industrialization during the actual course of the war, and in Australia, the national war effort placed severe restrictions on expansion of the private economy.

At the end of the war the stage was set for rapid expansion. Both countries ended the war with plentiful foreign exchange views and plentiful internal demand piled up during the war. Another period of parallel advance seemed likely. But a *diabolus ex machina* appeared in Argentina.

The Postwar Period, 1949-64

In the fifteen-year period from 1945-49 to 1959-63, Australia's gross product grew by 90 percent, its population by about 35 percent, and per capita output by about 41 percent. During the same period, Argentina's gross product grew 41 percent and its population by 31 percent and its per capita product by about 8 percent. Australia experienced one of the most successful period of its history, and Argentina one of its most disastrous.

Australia's economic policy since the war has been dominated more by the political mandate to maintain full employment than by the need to carry out an explicit long-term policy. Furthermore, the view has been widely held that a policy of continued full employment from year to year renders a long-run strategy unnecessary.

The 1945 government White Paper stated in its succinct first sentence that "full employment is a fundamental aim of the Commonwealth Government." The general philosophy of the White Paper was that if full employment is continually maintained, high levels of de-

mand will stimulate capital investment, technological progress, rising living standards, and economic growth. While the prescriptions of the White Paper have been somewhat modified, the mandate to maintain full employment has had a decisive influence on government policy. It has evoked a tolerance of booms and an intolerance of even mild recession that persisted until 1960.

Nevertheless, long-term policies were not absent. In 1947, an extensive immigration program was commenced. While its main motivation was probably defense, the government also felt that Australia's economic future could not be ensured with a small and aging population. Even though the original target of 200,000 a year has not been fulfilled, immigration has contributed two-fifths of the total population increase of 2½ million that has occurred since the war.

Other long-term policies have included the traditional ones of resource development by public authorities, increased social services, and protection for secondary industry. Protection was achieved, not only by the tariff, but, more potently, by quantitative restrictions on imports. A further long-term policy has been reflected in the traditional encouragement of residential building either through direct government construction or the provision of cheap credit for private construction.

Within this context, the private economy was allowed to have its head but was pulled up abruptly from time to time when the pace got too hot. In the early postwar years, stimulus was provided by the deferred demands accumulated during the war. Then the Korean wool bonanza, with its direct stimulus to demand and its effects on the liquidity of the economy, generated severe boom conditions, which produced the inevitable balance-of-payments crisis. The boom was quelled by fiscal and monetary restrictions, but the restriction was not carried far enough to produce appreciable unemployment.

Instead, the balance of payments was protected by the imposition of general import restrictions, which had previously been applied only to imports from the United States and Japan.

Direct import controls, while imposed for balance-of-payments purposes, proved to be a more powerful instrument of protection than the tariff. The restriction not only promoted production in Australia of a wide range of restricted imports, but they induced foreign firms and foreign capital to undertake a large part of the import replacement. The consequent inflow of foreign funds has been a major factor in permitting the balance of payments to run a substantial current account deficit amounting on the average to about 20 percent of imports.

Throughout the 1950's the combined effects of import replacement, population growth, and the insatiable demands of the public for automobiles and other durable goods kept the economy at high levels of

production and employment. In fact, boom conditions prevailed from 1953 to 1956, when again the balance of payments compelled the authorities to take restrictive monetary and fiscal action after foreign reserves had fallen to a figure that was apparently considered dangerously low; and imports were rapidly increasing despite the controls. General restrictions, however, were not carried far enough to warrant the removal of import controls.

The economy was also supported by a rising trend in export income over the period. Although protection, inflation, and a fixed exchange rate involved discrimination against exports and although export prices had a marked downward trend, both the quantity and value increased. This was made possible by sustained investment in the export industries and strikingly improved methods, which were the fruits of public, pastoral, and agricultural research.

The subsequent boom, culminating in 1960, ran the familiar course, but the methods of dealing with it were highly unorthodox by previous standards. In February, 1960, import restrictions were completely removed in order to absorb excessive domestic demand and hence curb the boom. The flow of imports was far beyond expectations. The mild fiscal and monetary measures failed to halt the boom as rapidly as desired. This prompted the severe restrictions of November, 1960, which ushered in the first serious lapse from full employment since the war. Expansion has now been resumed, exports have increased, but imports are increasing rapidly. The cyclical pattern of growth is probably continuing.

Prices increased spectacularly by Australian standards up till 1952, particularly in 1951 and 1952, when the consumer price index rose by nearly 40 percent. After that the price increase has averaged about 3 percent annually. In assessing the effects of internal policies, such as those of the Arbitration Commission, on the price level, it is well to bear in mind that a very considerable price increase could only have been avoided had the Australian pound been appreciated before 1951.

Argentina began the postwar period with the possibility and the promise of a period of industrial expansion comparable to that of Australia. It had laid the foundation for industrialization and the world was hungry for its major exports. In fact, from 1943 to 1949 industrial production increased by about 40 percent and industrial employment by about 30 percent.

Peron's policy, however, was not simply industrialization. It was designed more to increase the numbers of the urban masses and to win their political support. A major instrument for pursuing both objectives was to increase real wages. In fact, real wages were doubled between 1943 and 1949.

The doubling of real wages was accomplished by discrimination

against industrial profits and agriculture. Wage payments increased between 1943 and 1949, from 44 percent to 56 percent of the national income. Levies on agriculture were achieved by compulsory purchase of agricultural products by IAPI and by a progressively overvalued currency. Industrial profits were lowered through price controls and by government settlement of labor disputes in favor of unions.

The agricultural policy meant severely declining production and falling exports in face of rapidly increasing imports. By 1949, the substantial export surplus of earlier years had become a persistent import surplus. Needless to say, the balance of payments was not relieved by an inflow of foreign capital. The situation demanded some easing of the exploitation of agriculture, so that the burden of the wage bill fell increasingly on industrial profits and foreign reserves. But neither Peron nor his successors have succeeded in restoring exports to adequate levels. This failure, in conjunction with the absence of foreign investment, has severely limited the imports needed for continued expansion.

Any sophomore could have told Peron that he was raising real wages far above the marginal product of labor at full employment. But unfortunately no sophomore had his ear.

From 1948 till the fall of Peron, industrial production remained virtually constant, with increases centered on machinery and electrical apparatus, stimulated by exchange controls that severely curtailed imports. Production remained constant despite considerable but declining gross investment in industry. However, taking into account the official estimates of depreciation allowances at replacement costs, net investment was considerably smaller.

Since industrial employment failed to increase, increasing employment was provided by public bodies. The most glaring example was the increase in employment in the railroads from 150,000 to 310,000. However, government expenditures did not increase sensationally. In 1943, government consumption and construction accounted for 13 percent of the GNP and in 1955, for 17 percent. Increased government employment was achieved at the expense of the level of government salaries.

Between 1943 and 1955, wholesale prices and the cost of living rose by about 600 percent and 700 percent, respectively. While inflation was open to this considerable extent, it was also suppressed by price controls and overvalued (multiple) exchange rates. Labor policies reduced labor mobility and workers' productivity. The combined effects of inflation and labor policy undoubtedly resulted in widespread inefficiency and help to explain why substantial investment was associated with a stagnant output.

At the time of his ouster in 1955, Peron, thus, had a number of economic achievements to his credit. He had crippled Argentina's export industries; he had established impossible wage rates and widespread inefficiency in industry; and he had deprived Argentina of one of its main sources of support in the past, foreign investment. Under those circumstances there was no need for, or inducement to, immigration to replenish the labor force.

After Peron fell, the revolutionary government removed direct price controls and addressed itself to the export problem by raising the exchange rate applied to exports. Between 1955 and 1958 the volume of exports increased by about 25 percent, but at the same time dollar export prices fell by about 16 percent. However, the exchange rate adjustments meant that export income in terms of pesos increased fourfold over the period.

The government attempted to restrain inflation by resisting wage increases and preventing the quantity of money from increasing. But in 1958, with the advent of the Frondizi government, wage restraint was abandoned. A 60 percent increase was granted in May and open inflation accelerated. By the end of 1958, wages had doubled compared with 1955 and prices had increased somewhat less.

The combination of inflation and declining world prices made a large devaluation imperative. It was undertaken in January, 1959, and export income in pesos increased almost threefold for the year. In the same year the cost of living more than doubled, but the policy of wage restraint held wage increases to less than 60 percent.

At the same time, the policy of monetary restraint produced a decline in industrial production during 1959. Monetary expansion was resumed and open inflation continued. A domestic industrial boom was fostered by a combination of heavy protection and other financial inducements to the automobile industry. With the currency again becoming overvalued, export production failed to increase, and the contraction of imports achieved by the devaluation evaporated despite continuing policies of high protection. Increasing demand for credit led Argentine borrowers to seek large amounts of credit abroad. Wages steadily gained on prices, and by 1961 industrial production was only 5 percent above its 1958 level.

In March, 1962, the 1958 remedy was repeated with greater severity. The exchange rate on the dollar was raised from 82 to 135, and a severe policy of monetary and wage restraint was imposed. The credit restraint operated with particular severity because Argentine borrowers had to raise money at home in order to meet their foreign obligations at the depreciated exchange rate. This drain of credit produced a severe liquidity crisis. Government deficits continued unabated and the

need to finance them by central bank credit seriously curtailed the amount of credit available to the private sector.

The result was severe contraction of production and employment. At the same time, the effects of devaluation in raising prices directly and in increasing export income produced domestic inflation at an annual rate of about 30 percent, with wages lagging considerably behind prices, so that a substantial reduction of real wages took place. The price rise following devaluation was much smaller than in 1958, but the degree of deflation imposed on the economy was much greater. The 1962 devaluation increased exports and reduced imports, so that a current account surplus was achieved in 1963. General recovery was apparent in 1964. How far it can go before the next balance-of-payments crisis is a matter for conjecture.

In my opinion, the country has not yet escaped from the legacy of Peron. It will not fully escape until its export industries recover sufficiently to pay for needed imports and to give confidence to foreign investors.

By the end of the period the wheels of fortune had turned so that Australia was again ahead of Argentina with respect to total product, and further ahead per-capita-wise than it was at the beginning of the century.

General Observations

I conclude with some observations derived from comparison of the two economies over the entire sixty-year period.

First, the periods of most buoyant growth of total product—of Argentina in the first decade and of Australia in the last—were associated with large-scale foreign investment and rapid immigration. The assurance through immigration of an elastic labor supply probably encouraged foreign investment; and domestic savings were inadequate to accommodate rapidly increasing populations. Neither country has shown the capacity to sustain an aggregate rate of growth of more than 4 or 5 percent annually over extended periods. This should be a sobering thought for countries that expect to attain higher aggregate rates under far less favorable conditions.

Second, neither country has been a spectacular performer, over long periods, with respect to per capita output. Capital-labor ratios appear to have remained remarkably constant in both countries throughout the period. In some periods increasing world demand for exports has increased per capita output, but there have been subsequent periods of decline. Foreign investment and immigration may have been important factors contributing to productivity increases. Both countries, when left to themselves, seem to get hampered by restrictive business and labor practices.

Third, exports have played varying roles over the period. In some periods increasing exports have provided a direct stimulus to growth, through their multiplier and accelerator effects. But both countries have shown the capacity to grow without this stimulus. On the basis of the evidence of the whole period, I am inclined to stress the importance of exports as the means of procuring the imports needed for expansion and for giving foreign investors assurance of the credit worthiness of the countries.

Fourth, the policies of the two countries in the postwar period, though very different in outcome, were not so different in conception. Both involved import substitution stimulated by indiscriminate protection and, necessarily, discrimination against exports. By the exercise of a modicum of restraint, Australia managed to maintain exports and therefore its imports, at an adequate, and even increasing, level. It seems likely to be able to continue on its course, even though the easy stages of import substitution may be over. Argentina, on the other hand, carried its policies to extremes, and the damage it did to its exports has deprived it of the imports needed for continued expansion. It has thus reached an impasse which I think its previous history reveals to have been entirely unnecessary. The burden of proof is on those who contend that its present difficulties are not attributable to its government policies, especially in the Peron period.

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STATISTICAL APPENDIX

ARGENTINA
INDEX NUMBERS
(Five-year Averages)

	Gross Domestic Product (Y)	Population (P)	Capital (K)	Y/P	K/P
1900–04......	100	100	100	100	100
1905–09......	148	119	152	124	129
1909–14......	185	146	229	126	157
1915–19......	148	174	247	102	145
1920–24......	238	196	260	122	132
1925–29......	309	229	316	135	138
1930–34......	314	258	359	121	139
1935–39......	370	280	372	131	132
1940–44......	428	305	387	140	127
1945–49......	530	329	421	161	128
1950–54......	586	375	466	156	124
1955–59......	702	412		170	
1960–63......	754	432		174	

SOURCES: ECLA 1900–54 and official statistics 1954–63.

I have used total population rather than labor force employment or man-hours because of the difficulty of obtaining long series of the latter, especially for Argentina. I have no reason to believe that use of population historically affects the comparison between the two countries.

It will be noted that I have not used investment ratios on marginal output capital ratios as a basis for comparison. Where the relative prices of capital goods and consumers goods change over time, both of these ratios depend very much on the choice of weighting factors, and factors that produce a high investment ratio produce a low output-capital ratio. Therefore there does not seem to be much point in attempting to decompose the growth rate into the product of these ratios.

The quantitative effect of different weighting factors in Argentina has been discussed by Alexander Ganz and by Rolf Hayn in the references cited.

AUSTRALIA
INDEX NUMBERS
(Five-year Averages)

	Y	P	Y/P	K/P
1900/1 – 4/5.......	100	100	100	
5/6 – 9/10......	121	109	111	(1903) 460
10/11–14/15......	145	117	124	
15/16–19/20......	140	130	108	(1915) 470
20/21–24/25......	179	141	126	
25/26–29/30......	191	156	123	
30/31–34/35......	186	159	117	(1929) 530
35/36–39/40......	220	174	126	
40/41–44/45......	257	185	139	
45/46–49/50......	304	202	150	(1947) 395
50/51–54/55......	395	223	177	
55/56–59/60......	483	262	184	(1956) 475
60/61–62/63......	574	270	213	

SOURCES: Butlin, 1900/01–1938/39 and official national accounts figures in constant prices 1948/49–1962/63. Butlin's figures linked with official series by deflating official current values by a combination of official retail price index and wholesale price index; with weights 2 and 1. The resulting figures look suspiciously high. However, Colin Clark gets almost identical results but possibly uses similar methods. The Commonwealth statistician tells me there is no valid method of linking the Butlin series with the official postwar series. Capital per capita ratios are estimates by Garland and Goldsmith.

RECENT ECONOMIC EXPERIENCE IN INDIA AND COMMUNIST CHINA: ANOTHER INTERPRETATION

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I. *Introduction*

For the past fifteen years, comparisons of the economic organization and development of India and Communist China have had a strong fascination for analysts in many parts of the world. The similarities of the economic structures of and problems faced by the two most populous nations on earth and the sharp contrast offered by their economic doctrines have been grist for numerous articles in many languages. Perhaps the best known and most influential in the English language has been that by Wilfred Malenbaum published in the June, 1959, *A.E.R.* In it, Malenbaum concluded that in the years immediately preceding and in the years of each of their First Five Year Plans (FFYP), 1950-56 for India and 1950-57 for China, China's performance dwarfed that of India's by a considerable margin. Many other economists, writing at the same time, came to similar conclusions. I could not, on the basis of the data available, agree with them then, nor, on the basis of somewhat more data and a little more hindsight, can I agree with them now. The functions of my paper today are to: briefly review the years 1950-57 so as to take into account recent scholarship by others; evaluate selected major developments since 1957; and offer some conclusions on the subject of Indian vis-à-vis Mainland Chinese economic progress since 1950.

II. *Developments 1950-57*

As all students of the subject agree, the statistical data issued by both governments have left and still leave much to be desired quantitatively and qualitatively. Particularly relevant to the question of their respective growth rates are the accuracy of the data in the base years and the relative strengths of the upward and downward biases in the data for subsequent years. In the case of China, studies completed since 1959 almost uniformly point to sharp upward biases in the official data and cast serious doubt on the accuracy of the large increases in gross national product, investment, and physical output reported for the years 1950-57. In one study of the statistical system, it was found that in predominantly agricultural China, up until 1958, regular statistical services to cover the agricultural sector had never

reached down to the county much less the town and village level; and in late 1957, the director of the State Statistical Bureau had been forced to admit that they were not even clear about such basic agricultural data as the size of the territorial area of and the amount of cultivated land in China. In the study, the conclusion was reached that for the first five years, i.e., from 1949 to 1954, the official statistics are poor estimates, with the exception of those for state and joint industrial enterprises, which had higher but widely varying degrees of reliability.

Another study which dealt exclusively with industrial enterprises found upward biases due to the concept of the gross value of output used, pricing practices, and changing coverage. While unable to determine the exact magnitude of the identified biases for any one year, the researcher was able to state that their impact on the official indexes operated throughout the whole period and, becoming more pronounced after 1958, exerted a greater upward influence on the official data thereafter. Still other studies of China's national income completed since 1959 have yielded estimates for one or more of the years between 1952 and 1959 which, to varying degrees, have been below those claimed in the official data.

On the Indian side, it has been observed by students of that nation's statistics that, similarly to China's, they have required and still require invocation of the doctrine of *caveat emptor*. It is apparent that neither the absolute data nor the index numbers issued by the Indian government warrant confidence with respect to their accuracy for a wide variety of reasons, including, in a number of cases, being strikingly mutually incompatible. In the late 1950's and in the early 1960's, in predominantly agricultural India, the Ministry of Food and Agriculture cautioned against overreliance on its absolute data, because variations in coverage and changes in the method of estimation had made many of the years noncomparable. Further, for lack of sufficient detailed information as to what specific techniques had been used to create a number of its index numbers, it has not been possible for outsiders to verify the calculations which lead to them. Other Indian ministries issued roughly similar caveats about their data and raised similar doubts in the minds of outsiders. The reticence of the Indian government to make technical information known which a priori one would expect to be innocuous, by the early 1960's led one close observer to the suspicion that a drastic downward revision in the government's assessment of India's progress over the past ten years was in process, and that it was being carried out under the guise of a correction for noncomparability. Still more recently, a study published in August, 1964, dealing with food grains production from 1949-50 to 1960-61, indicates that

actual production may have been only two-thirds to three-fourths that shown in the official data, even after adjustments for variations in coverage and statistical methods employed.

The most significant point which emerges from these studies of the statistics of India and Mainland China is that we have been hasty in rendering judgment on the issue of their relative rates of growth through 1957; and as we shall see below, should not duplicate this error by hastily rendering a strong official-data-oriented judgment on the issue of their relative rates of growth since then. Perhaps the most conclusive proof of the inadequacies of the official data for this purpose and the folly of accepting any more than their general tenor, if indeed even that, are the major developments which have occurred since 1957.

III. *Developments Since 1957*

According to the official data, between 1957 and 1962, national income and per capita income in India increased by 22.8 percent and 10.3 percent, respectively, or, on the average, by 3.8 percent and 1.7 percent per annum, respectively. While not unrespectable, these performance data seem to be dwarfed by estimates of China's performance based on official data over the same period. In the Middle Kingdom, between 1957 and 1959 national income and per capita income increased by 31.1 percent and 27.0 percent, respectively, or by 15.6 percent and 13.5 percent annually, respectively. The performance gap in the macroeconomic data seems to be reflected in and supported by the individual production data issued by both nations. However, the tenor of these data is not supported by developments in these two nations not subject to complete statistical control by their governments. This is particularly true of China. In at least three major categories, common problems have been encountered and deficiencies have been registered which indicate to this observer that the development of their economies has been and the structures of their economies are more nearly alike than has been generally believed; and that the sharp contrasts in performance alluded to by earlier writers overstates whatever differences in development and structure may actually exist. The common problems are:

1. *Major Failures in Their Industrialization Efforts*. The year 1959 is the last one for which the Chinese Communist Party (C.C.P.) has issued a relatively full set of economic statistics. From 1960 to the present, in accordance with its explicitly stated policy of issuing only data which reflect favorably on them, they have issued very few statistics of any sort. However, in spite of the data blackout, it is known that even in 1959, to say nothing of later years, grave inefficiencies and disloca-

tions due to lack of coordination among the various sectors of the economy and within the branches of industry occurred. In the years 1959 through 1962, machinery and equipment in a number of industries had to lie idle for long periods because the industries had so expanded that they were able to produce more output than the inputs required for them which were available. In various industries, widespread attempts to economize on the use of critically important raw materials (e.g., coke and silicon in the production of steel) resulted in large quantities of substandard output. Continued misuse of and failure to maintain adequately machinery and equipment in 1959 and subsequent years resulted in increasingly numerous reports of breakdowns of industrial equipment and transportation facilities; and their continued inoperability for long periods of time. The few data available indicate that in 1960, 1961, and 1962, industrial production declined by at least 10 percent in each year and more probably closer to 15 percent. In September, 1962, the C.C.P. was forced to recognize its inability to continue its industrialization drive and it directed that henceforth, for an unstated period of time, the limited resources at its disposal were to be invested in agriculture rather than heavy industry. For 1963 and 1964, the C.C.P. claimed substantial improvements in percentage or qualitative terms in the output of agriculture-related industries, such as chemical fertilizer and simple agricultural machinery, and only modest increases in others.

In statistically more "open" India, roughly comparable industrial failures took place. Between 1956 and 1961, serious production failures occurred in thirteen industrial lines, some of which warrant description as being of crucial importance to India's industrialization efforts. The outright failures in these lines and failure to maximize production in others were due to a wide variety of specific reasons similar to those which operated as development drags in China. Essentially, these drags were technical and administrative inexperience or incompetence in the construction and operation of the plants. The inexperience and incompetence resulted in not only lower production levels but considerably higher production costs than originally anticipated. Indicative of how poorly executed the industrial effort was is that in spite of a monetary investment approximately 30 percent above the Plan estimates, the physical targets attained in industry were only 85 percent to 90 percent of Plan.

2. *Major Failures in Agriculture.* In China, despite grain production having increased from 108 million metric tons to reportedly successive all-time highs of 250 and 270 million metric tons in 1958 and 1959, respectively, in the years 1959 through 1963 there were numerous widespread reports of severe food shortages. In 1960 and 1961, sharp

increases in the incidence of illness caused by malnutrition were reported. Still other evidence of food shortages in this period were occasional pillaging of rice stores, increased flaunting of authority in remote places, and the development of illegal markets. The food situation was reportedly so critical in China that between December, 1960, and the end of 1963, it was necessary to import 16 million metric tons of grain; and, as of mid-1964, an additional 5.45 million metric tons were scheduled for delivery prior to the end of this year. Thus, by December 31, 1964, China will have imported nearly 21½ million tons of grain over a forty-nine month period, or about 5.3 million metric tons per year on the average. While the agricultural crisis in China did not necessitate large-scale imports of industrial raw materials of agricultural origin, it did require sharp reductions in the exports of these goods.

In India, the agricultural situation was roughly the same despite the tenor of many of the official data. Despite increases in aggregate and per capita food production between 1957 and 1962, it was necessary for India to import 23.2 million metric tons of food grains or an average of 3.9 million metric tons per year. Assuming an average population of 432 million for India during the period and an average population for China of 714 million for the years 1961 through 1964, inclusive, then grain imports per capita on the average for each of the years in these different but partially overlapping periods were .00895 metric tons for India and .00736 metric tons for China. Although—or perhaps because—larger grain imports per capita occurred in India than in China, there were not the same reports of desperate acts or desperate situations within India that emanated from China. Also, although it was necessary for India to import more food per capita than China, unlike China it was both necessary and possible for India to import relatively larger quantities of capital goods and industrial raw materials as well. This will be discussed in greater detail below. Suffice to note now that as in China, the food imports required consumed some foreign exchange which otherwise would have been used to underwrite India's industrial efforts. Were it not for US PL 480 which permitted successively larger food purchases by India from 1956 on with Indian currency, the foreign exchange drain attributable to food and other agricultural products would have been considerably larger. Aid from the U.S. and other friendly nations permitted and sustained industrial growth in India which otherwise would have been severely inhibited, as in fact it was inhibited in China for lack of the same external assistance. This brings us to what may be the crux of the question of Indian vis-à-vis Chinese economic growth since 1950.

3. *Major Dependence on Foreign Aid.* Both before and after 1957,

India and China were extremely dependent on foreign aid in connection with their industrialization efforts; and without this foreign aid the efforts of both would have ended in failure. With respect to China, during the years 1950-57, inclusive, the total value of Russian exports was U.S. $4.9 billion, while the total value of the loans extended was U.S. $2.8 billion plus the costs incurred in sending up to 10,800 Russian technicians to work in China, 1949-57, and the cost of transporting and maintaining 13,600 Chinese students and workers in Russia, 1951-57. If one arbitrarily assumes an average cost of U.S. $1,200 per person per year, then the entire cost of the technical services and training involved was no more than U.S. $230 million.

The Russian exports consisted of equipment vital to China's industrialization program. In 1957, for example, 50 percent of the Soviet exports consisted of industrial machinery and equipment, with the remainder being fuels, lubricants, and related products, ferrous and nonferrous metals, and miscellaneous other items relatively scarce in China, such as chemical fertilizer and drugs. In return for such critically scarce capital goods and related items, China exported hair nets, knitted goods, and a whole host of other innocuous consumer-type goods plus industrial raw materials relatively more abundant in China. Especially significant is that the Russians shipped to China not only machinery and equipment per se but complete plants. In most of the years of the FFYP, the ratio of the value of complete plants to the total value of machinery and equipment imported from Russia was 60 percent or higher; in 1957 it was 77 percent. By the end of 1957, about 57 percent of China's steel production and 50 percent of her coal production were coming from Soviet-constructed enterprises—this despite the fact that the Russians had built only 3 of the 15 iron and steel plants planned for the period and only 27 of the 194 installations planned for the coal industry. The conclusion is inescapable that the Russian plants were the larger, more efficient production units as compared to the plants built by the Chinese and that without such significant external aid, as a minimum, the industrial aspects of the FFYP would have been a failure. This conclusion is reinforced by consideration of the role of the East European satellite nations. To the 156 "above-norm" (i.e., large-scale) industrial projects designed and constructed by the U.S.S.R., they contributed an additional 68 plants and mines plus the services of 1,500 technicians.

In 1958 and 1959, Soviet exports to China were sharply higher than in 1957, and as in 1957, consisted of complete industrial plants, individual pieces of machinery, transportation equipment, and varied other capital goods. The year 1959 represents the high point of Soviet aid to China's economy with total exports reaching U.S. $955 million, of

which machinery and equipment were valued at nearly $600 million, with two-thirds of this latter figure representing complete plants. Because of the Sino-Soviet rift which developed in mid-1960, 1960, 1961, 1962, and 1963, Soviet exports to China were 84.8, 38.1, 24.1, and 19.6 percent, respectively, of the 1959 level. In 1960, 1961, 1962, and 1963, Russia shipped U.S. $500, $100, $27, and $42 million worth of machinery, respectively, to China. It is not mere *post hoc propter hoc* reasoning to say that as Russian aid to China flowed and ebbed, so did the rate of industrialization in China. Indisputably in the FFYP and the first half of the SFYP, China received capital goods which she did not have and could not have made in so short a period of time. Without Russian assistance, the FFYP would have failed and there could not have been an SFYP. Without substantial assistance from Russia up to the break in mid-1960, the SFYP would have been more of a failure than it was and economic conditions would have been even more serious from 1960 on than they were.

In India the situation was roughly the same. For the relatively small-scale FFYP, external financial assistance accounted for only 10 percent of total outlay on the Plan; however, for the industrially more ambitious and financially far more costly SFYP, it was budgeted at 24 percent and in fact was actually a good deal higher. This was due to classification of large loans to the government by the State Bank involving US PL 480 deposits as internal rather than external financing. For the Third Plan, foreign financial assistance was budgeted at 29 percent of the total outlay on the Plan including such use of PL 480 funds. For the years 1950-57, the comparison with China seems close, for in this period the direct aid extended by the U.S.S.R. to China amounted to 12 percent of the amount reported invested in capital construction. For 1958 and 1959, the Russian aid apparently amounted to only 4 percent. However, this observer attributes the unusually low proportion to an upward bias in the capital construction data and would hazard the guess that the actual proportion was at least 6 percent.

In India as in China, these percentages based on the official data do not impart a sense of the full value of the foreign assistance extended to the industrialization program undertaken. Between 1950-51 and 1960-61 India's imports of capital goods increased in irregular fashion from U.S. $227 million in absolute terms and 20 percent of total imports to U.S. $686 million in absolute terms and 31 percent of total imports. Concurrently, in irregular fashion, imports of intermediate goods and consumer goods also rose sharply in absolute terms. However, while the proportion of iron and steel, nonferrous metals, fertilizers, and other intermediate goods tended to remain relatively stable at

about half of total imports, the proportion represented by consumer goods fell in irregular fashion from about 24 percent to 19 percent. The increase in the proportion accounted for by capital goods and the decrease in the proportion accounted for by consumer goods was made possible as noted previously by US PL 480 exports to India. For the years 1957 through 1961, such imports by India for local currency amounted to U.S. $1.1 billion and permitted the allocation of scarce foreign exchange to the capital goods imports cited above. Clearly, aid from the U.S. and other friendly nations was to India during the SFYP what aid from the U.S.S.R. and the Eastern Bloc of nations was to China during both the FFYP and the first half of the SFYP. For the even more financially expensive and industrially more ambitious Third Plan, the role of the friendly nations was expanded. In June, 1961, a Consortium of nations agreed to provide U.S. $3.2 billion to meet India's immediate balance-of-payments problem and import orders to be placed through 1962-63. Interestingly enough, the U.S.S.R., Czechoslovakia, and Poland—nations not members of the Consortium and former major contributors to the economic development of China—agreed to contribute credits of over U.S. $500 million in addition. Thus, insofar as the period of the Third Plan is concerned, the foreign aid dependence pattern is being continued.

IV. *Summary and Conclusions*

The performance of China's economy—and particularly her industrial growth rate vis-à-vis that of India since 1950—has been grossly exaggerated. This is as true of 1957 and earlier years as it is of "The Great Leap Forward" and the subsequent period of distress. The extent of the exaggeration over the last fourteen years and the extent of whatever differences in performance may actually exist are impossible to determine, at present, given the statistical impasse which prevails. What can be substantiated now, directly or indirectly, is that in both nations the industrialization plans formulated depended to a considerable extent on external financial and material assistance; and that with the passage of time and the accumulation of experience, this dependence increased rather than decreased.

In both cases, the statistical association between the amount and kind of foreign aid received on one hand and the industrial progress registered on the other was self-evidently very high. Moreover, this writer would suggest that we may have cases of concealed classification and downward biases in the correlation coefficients with respect to the statistical problems being considered. Actually, what was compared in the late 1950's by various writers was the progress of the FFYP for India as executed by India alone with the progress of

the FFYP for China as executed by China, the U.S.S.R., and the East European satellites jointly. What this paper on recent economic experience has compared to a large extent is the progress of the SFYP for India as executed by India and friendly nations jointly with the SFYP for China as executed by China, the U.S.S.R., and their East European allies jointly for the first half and executed by China alone for the second half. From an analytical rather than a chronological point of view, the comparisons should have been India's FFYP and the last half of China's SFYP; and China's FFYP and the first half of the SFYP on one hand and India's SFYP and the first half of the Third Plan on the other. Undoubtedly, such comparisons would add further support to the position that the development and structure of their economies have been and are still more nearly alike than has been generally believed. For obvious reasons such comparisons and thesis validation (or invalidation) must wait for another occasion. In the meanwhile, let us apply the caveat of that long-forgotten confidence man who said that the best way to get an education is to doubt. With respect to comparative growth rates in India and China, let us be more skeptical in the future than we have been in the past.

DISCUSSION

W. M. CORDEN: I shall comment only on Professor Smithies' paper. He has compared two countries which are often included in the same group—whether the group consists of the "region of recent settlement" or of Maizels' "semi-industrialized" countries where import replacement is an important feature of growth. As any map of climatic regions will show, these two countries have striking geographic similarities.

From his sketch of the two economic histories there emerge two periods when the contrasts were great. The first was the period 1900-14. The population of Argentina grew 46 percent and of Australia 17 percent. In Argentina the import of people was accompanied by the import of capital, and in Australia there was apparently capital export, though not necessarily of autonomous long-term capital. I suspect that the main reason for low immigration and low (or even negative) capital import into Australia was the shock which the great depression of the 1890's had administered. The reason for the exceptional inflow of people and capital into Argentina at this time is not clear from Professor Smithies' paper. What is particularly interesting is that output per head grew by about 25 percent in both countries. It appears from Professor Smithies' figures that the capital stock grew faster than the population in Argentina. Allowing in addition for the economies of scale which one would expect to result when factor supplies increase in economies of that size, it is surprising that Argentinian output per head did not grow faster than the Australian. But perhaps capital imports yield delayed returns. At any rate, it appears that during this period the faster rate of aggregate growth, while no doubt making Argentina a more exciting place, did not really give the Argentinians a faster increase in the standard of living.

The second period when the contrast between Argentina and Australia has been striking is that of 1943-64. As Professor Smithies convincingly tells the story, it is all a matter of the economic consequences of Mr. Peron. Between 1943 and 1949, incredibly, real wages doubled in Argentina and the difficulties of Argentina have resulted from the attempt to sustain the unsustainable. No doubt the comparison with Australia is instructive for students of Argentina: it shows "what might have been" if politics had been stable and economic policies reasonably sensible. It is not so instructive for students of Australia, since Argentine-style policies, or at least policies carried to that extreme, were never a serious alternative. Comparisons with, say, Canada or Sweden would be more interesting.

What then has been the Australian economic performance postwar? Professor Smithies is right in saying that "the mandate to maintain full employment has had a decisive influence on government policy." Full employment—which is understood to be less than 2 percent unemployment and has for long periods been less than 1 percent—has been the keystone of policy. Temporary departures from full employment in 1952 and 1961 were quickly reflected in loss of popular support by the government. The emphasis on full employment is the consequence of the traumatic experience of the 1930's. In pursuing this

policy, the government has not had an albatross of a balanced budget mystique around its neck. By comparison with the United States and Canada, Australia has been very fortunate that its puritans are more interested in the liquor laws than in public finance. The surprising departure from full employment in 1961 resulted from deliberate deflationary policies designed explicitly to deal with a balance-of-payments problem. Possibly these policies reflected impatience with the continued, if not remarkable, upward movement in wages and prices during the previous decade. And Australian official thinking was influenced by current conservative thinking in Britain and elsewhere. The government nearly lost the elections at the end of 1961. Yet the outcry was not as great as one might have expected in the light of Australia's previous commitment to full employment. Perhaps a new generation had forgotten the 1930's. Furthermore, a disproportionate burden of the unemployment was borne by immigrants, who were politically less vocal.

The other keystones of Australian policy have been immigration and capital inflow. Immigration has depended on a gap between living conditions in Australia and Europe—a gap which is rapidly narrowing. It has been absorbed smoothly, without great friction, mainly because of sustained full employment. Capital inflow has been impressive. It has been unrestricted, not discriminated against, and there has been no talk of nationalization. Much of it has gone into protected industries. Therefore it has been indirectly subsidized—a subsidy which must be set off against profits taxes collected on foreign capital. Insofar as capital inflow yields a net gain to the people of Australia (a question which is widely discussed now), it represents the payoff for political stability and conservative policies.

Most Australians regard population growth as an end in itself, or at least as justified by "noneconomic" ends. This is not unlike the Canadian attitude. They regard it as an achievement to have absorbed over one million immigrants without significant social frictions and without a fall in living standards. But if Australian performance is judged in terms of the growth in GNP per head, it is clearly not so impressive. Here a comparison with Sweden might be most instructive. Sweden's population has grown very little and output per head has grown more than Australia's. It would seem that, in spite of economies of scale, immigration has held back the increase in output per head in Australia. Possibly economies of scale are less important in Sweden than in Australia because Sweden is a more open economy. In other words, the optimum population may be greater with protection, so that there is an economic rationale for population growth in Australia which does not exist in Sweden. But in any case, the Swedish experience suggests that (contrary to the usual Australian view) immigration is not needed to provide savings and investment incentives. As for protection—in respect of which Australia and Sweden differ very much—it seems quite likely that the maintenance of a protective system in Australia beyond the infant economy stage has lowered not only absolute real income but also the rate of growth.

WILFRED MALENBAUM: Economists study the record of economic growth in nations pushing their expansion because economists desperately need new

insights into this elusive process. India and China are of special interest. India is the perfect case: statistics, models, and procedures are more or less public. There is no obvious bias in official presentations. China is a challenging case: official data are limited and what are available are held to be systematically biased. Models must be inferred; important procedures are not known. But China's economic philosophy has fascinating parallels in the Soviet Union, which has been extensively and effectively analyzed by foreigners without special access to official materials. Moreover, India and China offer such strong parallels and contrasts that there is a marked gain from joint study with focus on the marginal influence of specified differences. Needless to emphasize—as a further reason for our interest—is the importance of development in these lands to operating officials in third governments like ours in the U.S.

Given the limitations of accepted growth doctrine and the inadequacies of even good data in poor lands, analysis of India and China challenges the economist and social scientist. All his skills are tested, his perseverance tried. What is the correct record? How do you explain the pattern it reveals? What are the "true" technological and behavioral relationships for the short run? Klein's paper in no way reflects this challenge—nor this opportunity for discovery. He neither poses the economist's problem nor applies any of his tools. He offers one basic theme: the records available exaggerate the reality for the two countries and we therefore can make no valid inference about relative progress. Given the consistency with which estimates for China show a rate of growth well above that for India, such a theme requires propositions on the relative degree of overstatement. Klein's specific points in no way contribute to these propositions. It is difficult to find the logical basis for his position.

I shall expand on this matter, with respect to all his major arguments. First, he maintains that the period 1950-57 is one during which the more or less official records in China and the official records in India are exaggerated, and thus that "we have been hasty in rendering judgment on the issue of their relative rates of growth through 1957." No specific sources are mentioned; no argument is made as to the degree of overstatement; the only order of magnitude given would lower the rate of growth in India by a significant margin. Available estimates (see my *A.E.R.* article in June, 1959, for example) indicated an increase in GNP in China of 86 percent and in India of 26 percent over these years—a factor of 3.3 times in the differential rate of change. Certainly one needs some evidence as to the difference in the degree of exaggeration if one is to accept Klein's conclusion (even without the marked downward revision he suggests in the figures for India).

Next, Klein claims we have in "major developments which have occurred since 1957 . . . perhaps the most conclusive proof" of the inadequacies of the official data for making a judgment on relative growth since 1957. Here he gives the "inadequate" figures: a 9 percent increase for national income in India and a 31 percent expansion in China from 1957 through 1959. He also lists individual commodity series which are generally consistent with these increases in macro data. Klein challenges this picture by invoking, somewhat

mystically, "developments in these two nations not subject to complete statistical control by their governments." The developments are in not very mystical areas: industrialization, agriculture, and foreign aid. For industry in China we are told in a rather general manner of imbalances and inefficiencies. All "quantitative" (really order of magnitude) material for China refers to post-1959—although these mystical developments were to throw light on the 1957-59 data presented. For India his argument has nothing to do with the inadequacies of official output data, but with the gap between original plans for industry and the final results shown in the official figures. The reasons for this gap are important and fairly well documented both in official and other reports. Basic was a foreign exchange crisis of great economic interest (of which Klein seems completely unaware). But, again, the gap between plan and actual data has no bearing on the question of exaggeration in the official record for industry in India.

With respect to agriculture Klein says only what is familiar. Output was below expectations in both countries, with the likelihood of absolute declines from 1958 or 1959 in China. In both countries, food imports were high during most of the 1960's, well above what either country had planned. Nothing Klein presents here demonstrates or even suggests that official output figures for the 1960's (which are essentially unavailable for China, anyway) are exaggerated and differentially so. Nor does there appear to be interest on Klein's part as to why the inadequate levels of production occurred in the agriculture of these countries. Was this poor planning and/or poor implementation? Or was it simply acts of God? Only the "why" is what we now need to know; the facts and the shortfalls (absolute or relative to expectations) are familiar matters.

A point of analytic interest is raised in this agricultural account. Since India received food as aid while China had to buy it, China was in a relatively disadvantaged position with respect to other foreign purchases. Carefully stated, this makes an obvious point. As Klein presents it, the point has no meaning, since the question of China's purchases of nonagricultural products involves such matters as the scales of foreign earnings, of foreign exchange holdings, and of import content in consumption and investment. Not only are these not discussed, they are not mentioned. His agricultural section thus provides nothing of analytical value on the relative progress in the two lands.

The last mystical development concerns the role of foreign aid. Apart from the facts on India being incorrect, the logic of the treatment of the subject in both countries is limited. The consideration of aid and growth makes no attempt to look at what aid was actually used for and why, no attempt to relate the effects of aid to its relative importance in total expenditure, no attempt to relate industry to overall growth even at a primitive static level, to say naught of the appropriate dynamic level.

More can be said, but enough has been to make the point that Klein's paper throws no relevant light on the relative achievements of China's and India's development efforts. What he says bears neither on what was nor on what is likely to be with this comparison.

Again, this is a great pity, since the subject is of importance from both

an analytic and operational viewpoint, and since there is so much that the economist's tools permit him to say about it. May I therefore try to rectify this inadequacy by indicating in a few minutes what might now be said about these two countries? While these are my own statements, I use the abundant material now available (1963, 1964) from serious scholars on China.

China experienced major expansions in industrial and overall product in the 1950's until sometime in 1959; the peak agricultural output may have been in 1957 (1958?) but there was an adverse crop in 1959. The years 1959-62 were crisis years, including three very poor harvests. Industrial production may also have fallen absolutely after 1960, especially in the modern sector. Very rough estimates of national product in 1961 and 1962 place it below its 1957 level. By 1961 the rate of decline of modern industry had certainly slackened off; by 1962 an upward movement occurred in agriculture and national product. Expansion has continued to date, with some evidence of a marked growth during 1964. With agricultural output currently at the 1957 or 1958 level, it is probable that China in 1964 has at least attained its national product of the early-leap period, 1957-59.

If we now accept the official output data for India, there has been an expansion of about 40 percent in national product since 1952. One answer as to which nation has done better between 1952 and 1964 thus depends upon the quantitative increase between 1952 and 1957-59 in China, since 1964 output may not have exceeded the latter level. Even the most conservative estimates place this increase above 40 percent. Thus, progress as measured by total income is of the same order of magnitude in the two countries, if we take the official figures for India, if we prorate over the entire period the serious reversals in China over the 1959-62 years and if we discount heavily (by one-third or one-fourth) the official data on Chinese overall growth from 1952 through 1959.

In the context of present interest in these two countries, much more significant observations need to be made. For both the development of theory over the past five to ten years and careful study of the experience of labor-abundant countries have given us new insight for hypotheses on the growth process. There cannot be a self-reinforcing growth process in these economies without sustained increases in output per man in the populous labor intensive, more traditional parts of the economy. Progress in the modern industrial sector, whatever the levels of foreign assistance, cannot substitute for progress in the more traditional parts, at least over the early period (first decade or two) of a program for accelerating or initiating growth. This seems to be a basic characteristic of the dynamics of growth in countries like India and China. Whatever the inadequacies of their industrial effort, whatever the extent and duration of foreign assistance to them, their overall performance responds, with appropriate multipliers, to the trend in output per man in agriculture, say. Unless the action program is geared directly to generate this (not as a substitute for, but in addition to, the much easier program for modern industry), the nation will not move toward self-sustaining growth.

There are important differences between Indian and Chinese growth efforts in this regard. The course of economic policy in India has persistently, over

the past decade, disregarded this requirement of the more traditional sectors. With what must be termed ideological rigidity, the program for growth has not dealt with these sectors (except in words); the Indian ("Western") model has focused on modernization and industrialization, and these, in proper turn, were to spark change in the more backward sectors. India pays a high price for this rigidity: the rates of overall growth have been lower relative to plan in each successive plan. In a fundamental sense India is farther from being able to grow on its own momentum than India was a decade ago. Without policy changes (which are as yet in no way evident), the outlook for a successful growth effort in India is a bleak one indeed.

The situation in China is different. Economic growth policy has undergone basic shifts. During the period through 1957 (perhaps into 1958) policy was essentially consistent with what, according to the argument above, the Chinese (and Indian) situation needed. The industrial sector was favored, but a major effort was put upon agricultural and small industry development, with the conscious aim of expanding the use of manpower. Rural-urban migration was discouraged. The "great leap forward" reflected a shift to a more familiar pattern of investment and development priority. Push the modern sector and its effects will spread throughout the economy. The hard years, 1958-62, cannot be analyzed here. But it seems to be true that present policy (since late 1962) is essentially back to that of the early years. Perhaps, as this brief account suggests, "eleven years of planned development, including five years of economic trials, have driven home some valuable lessons" (to quote Professor Choh-Ming Li). The prospects for steady improvement may thus be favorable—although there is of course no guarantee that Chinese leaders, once the nation is again on the move economically, will not again seek what they originally wanted from the "great leap."

In the light of the "facts" and the research results currently available for India and China, one must conclude that the Chinese prospect is the more favorable. China is seeking an effective program for its Communist society. India's intellectual commitments to inappropriate models continue to defy the evidence of India's own experience and to thwart the real opportunities for India's economic expansion.

M. C. URQUHART: I have been asked to direct my remarks primarily to a comparison of Canada's growth with the growth of Australia and Argentina. Canada shares with them a location in the temperate latitudes, a large resource base relative to population, and a substantial degree of urbanization. And if, following Mr. Smithies, we take 1900 as a reference base, we note that the Canadian population at 5.3 million in 1900 was only slightly more than Argentina's 5 million or Australia's 4 millon. Canada differs in being in the Northern Hemisphere close to the industrial areas of the United States and even of Europe; and in that the ambivalent feelings of Canadians, at least at the present time, are directed more toward one another than toward older cultures.

Before turning to the comparisons, I should like to make two points that I think are relevant. First, the Canadian economy is closely related to that of

the United States and is, in some sense, part of a larger continental economy. The influence of the United States has made its impact in more than one way. The United States replaced the United Kingdom as the largest source of imported goods between 1870 and 1890 and by 1900 had become the predominant supplier of imports (of a wide diversity) which she remains to this day. Partly for this reason but also owing to other influences arising from geographical juxtaposition, the products and technology of Canada are very similar to those of the United States. And while it was only with World War II that the United States could be clearly labeled as the largest single market for Canadian products, she has nevertheless been a large and important market throughout the period since Confederation (1867) to which I largely confine my remarks. In part as a consequence of the foregoing, United States direct investment in manufacturing and mining in Canada has played an important and growing role since before the turn of the century.

The consequence of the foregoing is that, apart from the period 1900 to 1914, the Canadian economy has tended to move in ways that are strikingly similar to those of the United States— a fact demonstrated by the similarities in long swings in rates of change of critical economic variables in the two economies. At the same time, at least for the earlier parts of the period, Canada was in competition with the United States for immigrants from overseas.

The second point relates to the tradition in Canadian economic history of explaining many of the periods of high rates of growth in Canada by the rapid expansion of exports of some products (staples), the expansion resulting from changes in technology or world markets or both. This explanation does not rely on the development of an export surplus creating the stimulus, since such periods have tended to be characterized by expanded imports and large inflows of capital (and people). Rather, it must rely on the rates of domestic capital formation that are stimulated from the growth of the high-productivity export industries.

With this background I turn to a most cursory summary of the features of Canadian growth for the periods which Mr. Smithies has used: 1850 to 1900, 1900 to 1929, 1930 to 1945, and 1946 to 1963. For the period 1850-1900 it is convenient to begin with comparative population developments, both because they are interesting and informative in themselves and because they are the most accurate aggregate data, by far, with the possible exception of external trade data, in the pre-World War I period.

The population of Canada in 1900 was somewhat over double what it had been in 1850, compared with the three- to fourfold growth in Australia and Argentina. This growth was more than accounted for by natural increase. And whilst immigration statistics record arrival of considerable numbers of immigrants (some of whom were undoubtedly on their way to the United States) through much of the period, emigration, mainly to the United States, was sufficiently large that there was a net outmigration of people for each decade between 1860 and 1900.

The period from Confederation (1867 to 1900) has traditionally been regarded as one of unsatisfactory growth, though there has been much questioning of this view recently. It has been pointed out that the export trade

grew only very slightly from 1870 to the late 1890's, and much of the unsatisfactory performance has been attributed to the failure of an export staple to emerge to take the part that exports of wood and its products had played before they were partly displaced by iron and steel. In contrast, the recent work of Bertram and others has indicated that a substantial growth in manufacturing did take place during the period, the growth rate of real gross output of manufacturing being 4.6 percent per annum between 1870 and 1900. To what extent this growth of manufacturing was caused by the protectionism which was a part of the national policy introduced in the late 1870's is in dispute; that it played some part is probable. Be that as it may, there was growth, sustained in large part by investment in railways, in canals, in urban building, and the like, the investment being facilitated by an improvement in the terms of trade of between 25 and 30 percent and substantial inflows of capital to finance railway building and government spending, part of the latter being on canals. But the limited growth of population is evidence of a less satisfactory performance in Canada than in other countries and particularly than in the United States.

The period from 1900 to 1930 had many of the characteristics that Mr. Smithies has described for Argentina. Canadian population nearly doubled between 1900 and 1930 compared with increases of 160 percent for Argentina and a 60 percent for Australia. There was a very heavy immigration between 1900 and 1914, of the order of size Mr. Smithies notes for Argentina, composed largely of agricultural workers and unskilled labor; and a somewhat less but still substantial immigration, largely of agricultural workers, in the 1920's. In the period 1900 to 1914 there was substantial internal migration from Ontario and other eastern provinces to the prairies and British Columbia; in contrast, in the 1920's the prairies and maritime provinces lost population by internal migration to Ontario, Quebec, and British Columbia.

The growth in the period 1900 to 1914—and even in the 1920's—was undoubtedly caused largely by the rise in wheat prices, the fall in transportation costs, and by other developments arising from technological change that made the settlement of the prairies possible and made wheat a major export; it was sustained by growth in foreign sales of base metals and wood products, especially pulp and paper, throughout the period. Manufacturing became increasingly more broad based and diversified. Indeed, whilst the growth of exports of primary products undoubtedly accounted for the major part of the growth in the 1900-14 period, the growth in the 1920's of manufacturing, much of it due to indigenous forces, accounts for much of the general growth of that decade. From 1900 to 1914, the induced investment in railways, agriculture, manufacturing, and social capital was very large and capital inflows were of the orders of magnitude that Mr. Smithies gives for Argentina. Much smaller but still significant capital inflows also took place in the 1920's.

The period from 1930 to 1946 may be disposed of quickly. In contrast with the experience of Australia and Argentina, Canadian real gross national product declined by nearly a third from 1929 to 1933 and even in 1939 was only 6 or 7 percent above the 1929 level. Net immigration turned into net emigration and there was some capital export. Loss of export income was drastic,

and investment in 1933 fell to a quarter of what it had been in 1929. On the other hand, during the second World War there were large increases in production as many unemployed persons and substantial amounts of idle productive capacity were put in use and as manufacturing industry expanded substantially even though there were substantial limitations of investment in the war years. Canadian population increased by less than 20 percent in this period compared with about 25 percent for each of Argentina and Australia and real gross per capita product increased by over one-third from 1929 to 1946.

Canada began the post-World War II period with a diversified economy. Despite the view of her as a producer largely of primary products, the manufacturing sector accounted for over 25 percent of employment and a larger proportion of national product. At the same time the agricultural, forest products, and mining sectors were still of great importance and contributed substantially to the manufacturing sector insofar as the latter involved processing of primary products for export.

From 1946 to 1963 (eighteen years) Canadian growth has been characterized again by a large surge of resource development, supplemented by further growth in the manufacturing sector in part based on indigenous forces, by substantial immigration, and by large-scale capital inflows. Growth has been carried along by an upsurge in exports, particularly in forest products and mineral products, along with a substantial development of oil and gas production more for import replacement than for export. It has been characterized by high levels of investment in manufacturing, in public utilities, in resource exploitation, and in formation of social capital.

In contrast to Australia and Argentina, Canadian trade and exchange policy, after the immediate postwar years were over, has been liberal, though with one or two recent notable lapses. One feature of the period was the existence of a floating exchange rate (and no controls at all) from the very early 1950's until 1963—an arrangement that appeared to work well until official intervention and other policies in the latter part of the period caused an exchange crisis and the establishment of a fixed rate at a substantially lower rate than that of the immediately preceding years.

As a consequence of the foregoing, Canadian population increased by over 50 percent between 1946 and 1963, and real gross per capita product rose by about 25 percent. Productivity per man-hour rose by about 3 percent per annum in commercial nonagricultural production from 1947 to 1963 and by much higher rates in agriculture. A consequence of the latter development is that the farm labor force fell by nearly a half during the period and now constitutes less than 10 percent of the total labor force.

What does this all add up to in an appraisal of Canadian economic growth? I think one can conclude that Canadian development has been more influenced by the emergence and development of new export products, based upon resources, than Mr. Smithies finds the case for Australia and Argentina. The importance of the development of resource industries for export may have in part been accidental. But it might also in part have been expected. The existence of an abundant resource endowment relative to population means that comparative advantage lies in the resource industries. And given

ie limited scale of the domestic market, resources could be developed on a rge scale only if exports were substantial. The growth in exports contributed an environment favoring immigration, stimulated large domestic investent, and was accompanied by capital inflows.

Having said the foregoing, I should add that the circumstances of the anadian economy have changed greatly during the period and that the part growth dependent on domestic markets has increased. The expansion of population from 5.3 million in 1900 to over 19 million in 1964 and the rise in er capita income have created a large domestic market. And as I noted earer, a considerable amount of the growth in the 1920's and again in the 950's has been generated by an expansion of manufacturing for the domestic arket. And the percentage of exports to gross national product which increased substantially from the last decades of the nineteenth century through ie 1920's has more recently been at more modest, though still important, vels.

COMPARATIVE ECONOMIC SYSTEMS: NATIONALIZED INDUSTRY

BRITISH NATIONALIZATION AND AMERICAN PRIVATE ENTERPRISE: SOME PARALLELS AND CONTRASTS

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This paper attempts some free-ranging, loosely linked observations about aspects of nationalized industry in Great Britain and draws some parallels and contrasts with American industrial corporations. It pretends nothing in the way of a systematic survey, and, although taking off in Britain, it spends as much time here as there.

In the Queen's speech, Britain's new Labour Government formally declared its intention to nationalize steel. The Loyal Opposition forced a division, and forthwith, by a precarious margin, Labour's decision was bound and sealed. The government is now committed to nationalize steel in some manner and degree. Graphically, Labour, holding a bear by the tail, has painted itself into a corner. It can wait for the paint to dry, but the bear will still be there. In the United States, in the meantime, the Administration and steel are eyeing each other warily.

The British iron and steel industry, aided and abetted (indeed, almost forced) by the government, managed during the 1930's to tie itself hand and foot in a network of restrictive price, output, and quota arrangements under a revitalized British Iron and Steel Federation. Mild supervision was provided by the government's Import Duties Advisory Committee. The Federation flourished during the war, working closely with the Ministry of Supply—and, of course, continues to this day. After the war, government supervision was vested first in an Iron and Steel Board and later in the Ministry of Supply. The steel firms which the Labour Government finally nationalized at midnight on February 14, 1951, after a bitter legislative and electoral battle, were scarcely prototypes of free enterprise; the industry and government hands which controlled their policies and practices were clearly visible—and heavy.

During its brief life, government-owned steel was virtually indistinguishable from privately-owned steel. The government's Iron and Steel Corporation owned the capital shares of most of the firms, but the

firms retained their individual identities and, indeed, their former boards. The industry, under the Federation, was powerful; the Corporation's brief hold on life was always tenuous. Nationalization produced new letterheads and some reshuffling of files; it cannot be said that the industry was swept by a strong, fresh breeze.

The Conservatives' Iron and Steel Act, 1953, dissolved the Corporation, and most of the industry shares have found their way again into private hands. A new Iron and Steel Board was established by the government to consult with and to supervise the industry in matters of planning for plant investment, maximum prices (including individual items as well as the level of prices), imports, and the distribution of materials and products. The Board speaks with authority on prices, and it has a veto power on major construction projects. It is concerned to see that the industry is not overbuilt, but it cannot order the replacement of old by new, more efficient plant. Its very recent adverse comments on the state of efficiency in the industry may add strength both to Labour's purpose and to Labour's case for renationalization. The British Iron and Steel Federation is, of course, omnipresent. This is the industry, privately owned, heavily cartelized, government supervised, nationalized in 1951 and denationalized in 1953, which in 1964 faces the prospect of renationalization. The fact that now, as in 1951, steel, unlike most candidates for nationalization, is neither "sick" nor "disturbed" cuts two ways: it makes it less apparent that renationalization of steel is an urgent response to a crying need; it also gives nationalization something like an even chance to achieve a commercial success.

In the United States, private ownership of steel has never been displaced, and no displacement is even in long-run prospect. The industry has had its skirmishes with antitrust; it has been singled out sporadically for hearings and lectures by the Senate Subcommittee on Antitrust and Monopoly and was the victim of a special going-over by the President on the occasion of its abortive price increase in the spring of 1962; but it has never been subjected to formal supervision in peacetime by any government regulatory body. The term "free enterprise" alone, however, is inadequate to characterize steel in our economy. For many years now, our political statesmen have been admonishing industrial and labor leaders to don the mantle and grasp the torch of economic statesmanship, and, in those markets where "private parties may exercise considerable discretion over the terms of wage bargains and price decisions," to act with responsibility to society. The admonitions have derived from high-level concern that "administered" and "negotiated" wage and price increases in power-concentrated basic industries might generate and sustain inflation. It is generally under-

stood and accepted that steel is a (the) prime target of these admonitions.

Such admonitions can conceivably have an economizing role to perform, of course, only if in the industries in question competition as an effective controller of prices and wages is absent. The growing flow of admonitions suggests a dawning realization that competition in these industries is, in fact, a weak economizing reed. Naked and unsupported, however, admonitions to be "responsible" can scarcely be taken seriously either by those to whom they are directed or by the public in whose name they are delivered. The shape of "responsibility" in an economizing sense is not self-evident, nor is an injunction to be responsible self-enforcing. Without a standard, "responsible" affords no direction, and without a sanction it carries no force in a rough, tough world.

Both a standard and a sanction have recently been supplied. In January, 1962, the Council of Economic Advisers laid down guidelines for noninflationary wage and price decisions, and it also announced a sanction—atmospheric pressure. As the Council saw it, "an informed public, aware of the significance of major wage bargains and price decisions, and equipped to judge for itself their compatibility with the national interest, can help to create an atmosphere in which the parties to such decisions will exercise their powers responsibly." Admonitions have burgeoned since 1962. At the direct instance of President Johnson, the Council restated and elaborated its guideposts in its *Annual Report, 1964;* and the President has served notice that he will "keep close watch on price and wage developments with the aid of an early warning system which is being set up in the appropriate agencies," and he will "not hesitate to draw public attention to major actions by either business or labor that flout the public interest in noninflationary wage and price standards."

Official Washington denies that the government is engaged in price or profit control. The guideposts, it is insisted, "contain no element of control—or of incipient control." So be it. We are entitled, nonetheless, to believe that our government is acting deliberately, not frivolously—that it is not intentionally making idle pronouncements and setting vain processes and forces in motion. We are entitled to believe that the government expects the price record in the years ahead to bear the impress of its efforts. Guided and pressurized admonitions and early warning systems may not add up in the government's semantic calculus to pure and perfect control, but they certainly rate a recognized position well along the control spectrum. And if they do not achieve a recognizable measure of success, the government is going to look a lot sillier than responsible and politically sensitive governments can afford to look.

As an aside, we should note that the classical concern of economics with ineffective competition has grown out of an interest in allocation of resources and distribution of income. In the area of public policy it has manifested itself almost exclusively in antitrust—the maintenance of competition as an economizing force. We have not been too happy with antitrust, but except in the case of public utilities, we have not hitherto resorted in peacetime to direct controls. Guided and pressurized admonitions have come in response to our concern over the possible inflationary effects of noncompetitive pricing. This is a macroeconomic concern, as distinct from allocation and distribution which reside in the house of microeconomics. But, if admonitions have any effect at all, they will have micro as well as macro consequences, and both will demand attention. Macro infractions will certainly elicit micro explanations. It is amusing to contemplate the spectacle of macro-inspired controls entering the economy and having their way with micro matters hitherto insulated from controls. Could it be that we are on the brink of a breakthrough—a Grand Neoclassical-Neofrontier Synthesis? The possibility inspires me to verse:

Oh Mac and Mīc, they looked alike
to Jack and Ike;
To L.B.J. they're both okay
and here to stay!

Whether the renationalization of the steel industry, presently ridden by industry and state controls, will mark Britain as more "socialist" than at present is of the same order of unimportance as whether our own attempt to dampen down the pricing proclivities of steel and other industries by admonitions is to be called "price control." I suggest that what both of us are doing is of a piece with what all politically democratic societies are constantly doing and will continue to do as long as they remain politically free. It represents the restless movement of a politically free people, concerned to make headway now against some part of the total economic problem with which they are everlastingly contending. Sometimes, as in the case of nationalization, we move directly, openly, confidently. Sometimes we feel our way, confused, self-deluded, hopeful.

Nationalized industries are in the thick of society's great and never ending economizing adventure. As an economizing device, nationalization is uniquely endowed and situated to illuminate the nature of economizing and to contribute to its understanding. The ways of nationalization are direct and open, and they compel public concern and involvement. They also conduce irresistibly to introspection and agonizing self-appraisal.

Nationalization lays its performance on the line. Since the first of

Labour's nationalization acts (Bank of England) came into force on March 1, 1946, an overflowing river of official reports has rolled from the presses and keeps on rolling along. The daily lives of coal, gas, electric power, transport, and the others, as they are lived in Britain, with all of their achievements, shortcomings, hopes, doubts, and frustrations, are worked over, detailed, analyzed and quantified, and bared to the public. The industries' problems are the public's problems. Nationalization has no secret life, and if confession does something for the soul, Britain's nationalized industries can look to the hereafter without apprehension.

That part of the British economizing market occupied by its nationalized industries is not characterized by invisibility, automaticity, or anonymity. Of necessity, nationalized industry approaches its tasks directly and explicitly. Its economizing machinery is uncomplicated and exposed; the kitchen is always open for inspection. Problems are posed and resolved, programs are promulgated, and actions are taken explicitly by identified persons, and reasons or rationalizations are spelled out painfully and in octuplicate. No one can hide behind "flows" and "forces"; these are man made, and their making and their makers are matters of record.

Of course, widespread public scrutiny of, and involvement in, the lives of nationalized industries and constant self-examination by the industries themselves are not to be had without cost. The party can become too crowded, and self-questioning can become an obsession. In fact, the industries are in trouble with someone—frequently with each other—much of the time. And each is always in trouble with itself, notably in the matter of relationships and responsibilities within its own four walls, and as between itself and its Ministers, Parliament, the Treasury, and the Cabinet. British nationalized industries are prolific producers and insatiable consumers of organization charts.

But the openness, directness, and public involvement which characterize nationalization are capable of making a unique and significant contribution to public understanding. Nationalization sets up and displays with stark clarity to people who are concerned the real nature of economizing which the indirection and more convoluted processes of our private enterprise economies tend to obscure. The basic economizing problems present in the private sector of the economy do not differ from those with which nationalized industries are required to deal, and the nature of their consequences for the public is the same; but only rarely do they rise or are they drawn to the surface, and even more rarely do they make their way as issues into the public domain. The mysterious, impersonal processes of private enterprise—and of its most celebrated institution, the corporation—can be understood and

need to be widely understood, if, as I believe, economizing is the public's business. But private enterprise and its processes are not themselves vehicles of understanding. Nationalization is, by nature, such a vehicle, and where it exists it should be exploited fully in this capacity.

Nationalized industries are forever ensnarled and entangled in investment and pricing problems and decisions which involve them, their supporters, and their adversaries in public debate. Regulated private industries (steel in Britain, public utilities in the United States) are similarly, although less frequently and less deeply, engaged. "Free" private industries are largely exempt from systematic public debate, even though basically the same investment and pricing issues are present, hidden beneath the surface of the market.

Ensnarled and entangled are appropriate words to characterize the involvement of nationalized industries with investment and pricing. It is difficult to deal with any phase of either investment or pricing without becoming engaged with all phases of both. Coal, transport, gas, and electric power in Britain have continuously to ask, when is expansion justified, how much expansion is "enough," where is the necessary capital to come from, and how much should the industry be required (permitted) to earn—and a complementary array of questions on the level and pattern of prices.

When, indeed, is further investment justified and in what amounts? A nationalized industry operating under conditions of monopoly or semimonopoly and openly determining its own prices is denied the luxury of automatic market answers to these questions. It cannot look to "profit" as a guide or measure; its profit is in substantial degree a function of the prices it sets with its "needs" for expansion, along with other considerations, in mind. An uninspired government pronouncement in 1961 laid down that, in general, British nationalized undertakings shall balance their revenue deficits with revenue surpluses over five-year periods, and that charges against revenue shall include interest, depreciation on a replacement cost basis, and "adequate allocations to general reserves which will be available *inter alia* as a contribution towards their capital development and as a safeguard against premature obsolescence and similar contingencies." Undertakings that have large and expanding capital needs are expected to contribute from their reserves, built up for the purpose, toward their own financing. Such additional capital as may be needed is to be borrowed from the Exchequer rather than the capital market, and the state expects "capital employed in this kind of business to earn a higher rate of return than the cost of the money to the Exchequer." (*The Financial and Economic Obligations of the Nationalised Industries*, Cmnd.

1337, April, 1961, pp. 7-8.) Development plans for the ensuing five years are to be discussed by the industry annually with the government, and the government will fix the upper limit on investment for the following two years and will approve borrowing on an annual estimate. The government requires to be especially informed of proposals to invest in low-return projects.

Investment decisions in these industries are necessarily more responsive to demand as expressed directly for physical output than demand as reflected in prices and profits. This is bound to be the case, since prices and profits themselves represent policy decisions similar to (in fact, practically a part of) the investment decision. The "profit" return of nationalized industries is not a residual representing the favor or disfavor in which the product of the industry is held by the market; it is to all intents and purposes an explicit cost calculated to provide the industry with as much investment capital as the government believes should be forthcoming from this source. It is estimated as other costs are estimated; it differs from other costs only in that it does not represent a contractual obligation. Its justification, as a cost and in amount, is found in the use to which it is to be put—a fact which stamps it as something quite different from the "profit" of market theory.

In market theory, profit is a reward for assuming the risk of ownership—the ownership investor, at the time of investment, is moved by the prospect of profit to accept the risk of losing his capital. In a market society in which risk is inherent, profit, in an overall sense, can be said to be a cost, in that ownership risk will not be accepted and borne unless the possibility of profit is present. In the case of individual firms, however, profit (when positive profit actually appears) is a residual. It is not bargained for. Its appearance and dimensions are a function of market forces—it is a reflection of those forces, and a guide to those who, as producers or consumers, would respond to them. Neither its appearance nor its amount is affected in the slightest degree by the use to which it is put, either actually or prospectively. The market does not award profit to those who propose to save and invest it, any more than to those who propose to employ it in the most frivolous consumption. It is not peculiarly and by nature a source of investment capital.

In our own regulated public utilities we encounter profit problems similar to those in nationalized industries. Profit—a "fair return" or a "return equal to that being earned by comparable industries"—is explicitly named and provided for as a cost to be recovered in rates established by the regulatory authority. If the named profit does not appear, rates will be adjusted upward; if excess profit appears, rates will

be reduced. The market machinery by which profit serves as a guide to investment and disinvestment is bypassed; existing investment, whatever it may be, is explicitly validated. If a regulated utility decides to expand and the regulatory authority is acquiescent, it is expected that the authority will make any rate adjustments necessary to provide the appropriate profit, and the federal Constitution stands ready to sustain the expectation. It is notable, but not surprising that the British Iron and Steel Board finds itself in a similar bind: it determines that investment for expansion and modernization are necessary; it is then prepared to approve prices designed to produce an explicit "profit" high enough "to warrant investment in this as against other industries." (Iron and Steel Board, *Annual Report, 1961,* pp. 18 *et seq.*) In specific actions, when demand falls off and, because of lower plant utilization, unit cost rises, a rise in price is indicated!

And now, the cream of the jest: our own steel industry, seeking to build a public opinion favorable to an increase in steel prices, presents its case for a larger profit, not on the classical market theory which its activities presumably exemplify, but on good solid nationalization theory—the industry needs more profit as a source of capital funds for needed expansion. I won't labor the point, but if the American steel industry should one day find itself pictured in the public mind as a promising candidate for regulation or nationalization, it can take credit for having, itself, built the image.

We do not know a great deal about investment decision making in private industry, but it seems probable that the processes and operative criteria are not as dissimilar to those in nationalized industry as a preoccupation with classical market theory might suggest. The process by which corporate managements decide to retain and invest earnings has some bearing on this matter. These are captive decisions. The corporation may or may not in particular instances be closely hemmed in by the market in its pricing decisions, and managements may or may not in particular instances consciously construct their reinvestment decisions according to market profit-maximization specifications. But, in all instances, management decisions to reinvest earnings bypass a significant set of economizing screens native to the classical market. The decision to invest in the firm is made almost universally in the absence of the test offered by the investment possibilities held out by other firms in the same or other industries. Further, of course, it escapes completely from the primary test of saving versus consumption (only the stockholder is in a position to administer this test).

But what this really tells us is that there is a lot of collectivism in all of us—even in private enterprise. Classical investment criteria are partially avoided in the case of the corporation, not because managements

rather than stockholders are in the drivers' seats. This determines only the identity of the avoiders. The classical market criteria for investment are bypassed because of the fact of collective ownership; individual stockholders would necessarily bow to the will of other (majority) stockholders even if corporation managements were not allowed to manage. Essentially the same phenomenon is explicitly present in nationalized industries, as it is throughout the entire "public goods" sector of the economy. Not infrequently this is represented as a shortcoming of nationalization and of public provision of goods and services generally. We should not lose sight of the fact, however, that collectivism, under which the economic will of individuals is subordinated to the economic will of other individuals, is widespread in private industry; and that this has important implications for the economizing processes of the "free" market.

A great debate has raged over whether or not British nationalized industries should be required to pay their way. Much of the debate is sterile, but it has helped to focus issues which are central to the public task of economizing. I have already nodded to the debate, necessarily, in talking about investment criteria. Officially, Britain has decided that, in general, nationalized industries should price their output so that over five-year periods revenues will cover charges against revenue.

The financial record indicates that the rule has in fact been departed from, but the precise measure of the deviation in all instances has yet to be calculated from the record; and there is reason to believe that responsibility for any deviation lies more with the government (concerned with elections and with inflation) than with the undertakings. The major consequence of failure to follow the commercial "market" rule is said to be misallocation of resources: prices too low to cover costs, or costs made artificially low by reason of loans at artificially low rates of interest are alleged to lead to overinvestment in the industry at the expense of underinvestment elsewhere in the economy. The claim that prices have been too low and loans too cheap and that significant misallocation by market standards has in fact occurred has been bravely challenged by one of the discussants at this session (William G. Shepherd, "Public Corporations and Public Action," *Political Quarterly,* Jan.-Mar., 1964, p. 58, for the dwindling few who prefer their economics watered down in English; "Cross-Subsidizing and Allocation in Public Firms," *Oxford Economic Papers,* Mar., 1964, p. 132, and "British Nationalized Industry: Performance and Policy," *Yale Economic Essays,* Spring, 1964, p. 183, for those who take it straight), and I hope that the discussion earlier in the present paper has already cast some doubt on the logic of the claim. The benchmarks are price and cost, and in the nationalized industries both

of these are so greatly within the arbitrary determination of the very undertakings they are supposed to guide as to lose much of their value as guides. Prices, costs, investments, and outputs which are "made" are determined, not determinants, and a balance of price and cost can be had at many different levels of investment and activity. The "revenue equal to charges" rule established by the British government is not the equivalent of the classical market guide to and test of economic allocation.

Price equal to cost as the guide to economic allocation and test of efficiency also falters in the costing and pricing of individual products produced by multiproduct undertakings (and, of course, it is individual products rather than total outputs that are regularly bought and sold). I have no stomach for taking on the accounting profession at this or any other session, but I still labor under the long-standing impression that the distribution of common costs among the several products of a multiproduct undertaking is an art and not the administration of a natural law; and, hence, that the price-cost calculus of the free enterprise market bears, at best, a slight tinge of the arbitrary. This has relevance for the issue of cross-subsidization—the carrying of unprofitable branches of a nationalized undertaking (e.g., coal, transport) by its profitable branches—which has engendered a spate of controversy in Britain. It is argued that cross-subsidization should be superseded by a strict "commercial" policy of withdrawing from unprofitable branches—high cost mines, underused stations, etc.

Professor Shepherd has demonstrated that cross-subsidizing has in fact been less widespread than is sometimes supposed, and his sympathetic treatment of such cross-subsidization as has occurred, in the setting in which it has been practiced, is both convincing and encouraging. I can add little but happy conviction to the debate. I should like, however, to mention the universality of cross-subsidization. It is to be found both in the regulated and "free" private industries as well as in nationalized undertakings. It is as familiar to regulated transportation in this country as to government-owned transport in Britain; and the ubiquitous American drugstore may be offered as one popular candidate to represent cross-subsidization in free private industry. We worry about its presence in the case of railroads; when it appears in the drugstore we pay the druggist to worry.

I am quite prepared to accept that, as an intellectual exercise in the classroom or computer laboratory, a pattern of investment, output, cost, and price might be worked out which would bring an allocation of overall resources of nationalized undertakings into line with a free market pattern of resource allocation—also worked out on a blackboard or computer. But no such idealized allocation worked out for

flesh and blood nationalized undertakings would mesh "naturally" with the allocation of resources produced by the forces actually at work in the free enterprise markets of the real world. This latter does not make me unhappy, because I am quite prepared to believe also that the investment, output, cost, and price decisions which can be made by, or for, nationalized industries by more direct, blunt calculations, can perform the resource allocation task which society wants performed quite as satisfactorily as it is in fact performed by any free enterprise market which we have known or are likely to know. Further, the processes available to nationalized industries provide a mechanism for taking into account—openly and responsibly—considerations which society wants taken into account outside the strict rule of price and cost, and which private industries operating in the market either cannot accommodate or are not suited to accommodate in their economizing decisions. Indeed, to many this is the moving argument in favor of nationalization. Its realization calls for a little more imagination than the British government has displayed to date, but the potentiality is wide open.

I have great admiration, even fondness, for the logic and the calculus of the classical market—as a benchmark. But it is neither an infallible nor a universal guide for the economizing conduct of great societies of men living together. Men can, and do, express their wants for themselves and for each other honorably and efficiently in ways other than individual market demands backed by purchasing power—notably by public expenditures for public goods such as defence, health, education, and the like. I can think of no reason why they should not similarly express their economizing desires through the medium of nationalized industries whose investment, output, and price decisions depart deliberately and openly from the standards of the classical market. The only restriction I should impose is that they should be aware of the reasons for the departure and, within the limits of easy calculation, the extent of the departure.

Who should make the decisions for the nationalized industries on investment, overall and individual service pricing and disinvestment, since the decisions are not to be guided surely and without deviation by precisely identifiable market criteria? Who, if not "the market," is to speak for society? In the case of British nationalization, two voices have been heard and not always in complete accord. Investment and price decisions have been formulated in the first instance by the undertakings, but the government, represented by the Treasury and the Minister to whom the undertaking is officially attached, have had the decisive, and sometimes a different, word. The undertakings have been known to complain that their industrial-economic expertise was being

subordinated to the political-economic expertise of the government, even before 1951, and particularly since they have been operating from the enemy's camp.

The government must have the last word. We live in political economies, and only the government elected by the people can speak for the people where the market has been silenced. Further, economizing involves overall coordination, and only the government—certainly not each individual undertaking—is in a position to coordinate and to accept, for society, the responsibility of coordination. If we are to forego the soothing, anesthetizing processes of the market and face the problems of economizing squarely and head on, the government must take charge as the overall directing, coordinating, responsible agent.

I stand on this conclusion despite my unhappiness with the pricing decisions which the British government has made. But I hasten to offer two caveats. First, a sensible government will not forget that the function of management is to manage; it will delegate large authority to the management of the industry and will spell out and publicize the limits of that authority. It will give great weight to managerial judgment and will encourage imaginative experimentation as a value particularly to be sought in a nationalized industry—even, say, a spot of marginal cost pricing, when no one is looking.

Second, and on the matter of coordination: a nationalized industry is part of a total economy, and its policies and program must dovetail with the overall policies and programs—macro, micro, and social—established by the government. This relates to situations in which the industry is peculiarly involved, where, for instance, the industry is tempted to cross-subsidize to assist a derelict area, or where the industry is caught up in a price dispute with another nationalized industry; and also to economy-wide matters as broad as inflation or aggregate unemployment. Industries, acting alone, can deal only partially and fleetingly, and in many cases only harmfully, with these matters, but directed and coordinated they can be employed to achieve a national purpose. The other, and equally important, side of this coin is that the government itself must have an economy-wide view and a total set of positive policies and programs into which the affairs of the industry can be purposefully drawn and firmly fitted. Much of the complaint that British nationalized undertakings have leveled against overriding governments might have been precluded if it could have been made to appear that government decisions were called for by basic policies and overall plans and were not the reflection merely of arbitrary and piecemeal judgments.

The argument has been made that managements of British nationalized industries should be free to run their undertakings and to indulge

their inclinations in the general area of welfare independently of the government, on the ground that managements of private industry, notably in the United States, enjoy such freedom. This permits us another look at the American scene. The look is conclusive, but the conclusion is not that which the analogy was intended to establish.

The idealized version of the management of the giant American corporation places it amidst and slightly above the other claimants to corporate bounty—consumers, labor, creditors, suppliers, the public and, oh yes, the owners—dispensing economic justice among them, inspired, restrained, and guided by its social conscience and its deep sense of social responsibility. This version is overdone, but it is still quite true that American corporate management does occupy a unique position. Its four freedoms—from government, from competition, from stockholders, and from want—are not absolute, but they are substantial and significant. Of these, the one that most concerns us here is freedom from competition. To the extent that the corporate undertaking is free from competition as an economizing force, it is, in the logic of American capitalism, irresponsible. In that logic, market competition both defines and enforces economic responsibility; there are no free agents. There is nothing startling in this—the very purpose of any economy is to bend the will of individuals to the will of society in economic matters, whether the determination and enforcement of society's will are functions of the market or of society organized in government. If competition is ineffective and government is remote, there is a void that must be filled either by the restoration of competition or the introduction of government. It cannot be filled by an infusion into enterprise managements of a sense of social responsibility. No matter how sincere they are or may become and no matter how ardently as individuals they may seek the good life for all of society, there is still lacking what is lacking in Britain if individual nationalized basic industries are left as free agents: direction and coordination. To the extent that giant corporations in the United States are in fact free to dispense economic justice, they are a problem for us; they certainly are no model for British nationalization.

Let me turn finally to a type of problem which I suggest is spawned in the semidarkness of private industry, but from which nationalized industry, in its simple innocence and because of the light of publicity which everlastingly shines upon it, is relatively free. Top-management compensation presents little difficulty for Britain's nationalized industries, but if I read the signs correctly, it threatens a real blowup one of these days in American private industry. Good business executives must be paid enough to induce them to practice their arts effectively in the places where society wants their arts to be practiced. Top execu-

tives of nationalized industries are hired and paid under conditions and on terms which are completely open, straightforward, and understandable. No mystery surrounds the transaction. The conditions and terms which characterize the engagement and payment of top executives of American corporations, on the other hand, are complex, mysterious and, one has reason to believe, highly personal to the participants. Through the medium of the corporation, corporate executives are the employees of the owners of the corporation—the stockholders. It is a rare stockholder, however, who can work his way through the melange of straight salary, bonuses, options, pensions, expense allowances, payments in kind, vacations, and privileges that make up the executives' compensation package, to a confident conclusion on how much his executives are costing him. He is handed a self-serving rationalization—"we must meet the market if we are to attract and hold able executives"—but he can scarcely avoid the conviction that the make-up and dimensions of the package can be said to be market determined only if by the term "market" something resembling a private preserve is meant.

The market for corporation top executives is indeed a peculiar affair. Through its control of the proxy machinery, top management determines the selection of the board which selects top management; top management sits across from itself at the bargaining table, hiring itself and setting the terms of its own employment. Top management is a vitally important factor of production, but operating in the context of the corporation its precise functional character has never been thoroughly examined or buttoned down. It can be hired on a fixed-dollar contract, and in this sense is a labor service; it can "invest" itself for a possible return in profit (just as ownership capital can be invested), and in this sense its provision partakes of entrepreneurship. In the latter case, the risk component may or may not be present. Irrespective of their designation and whether their remuneration be termed "wages" or "profit" or both in uncertain proportions, the services rendered by top management to a private corporation are unstandardized, and the measure of their productivity is, at best, imprecise. Small wonder that sitting in economic judgment on itself in its own specially contrived "market," top management comes up with a compensation for itself which in form, content, and dimension is wondrous to behold. And when one contemplates competing top managements pursuing competing top managements and luring them away from other competing top managements in a kind of incestuous, fenced-in, closed circle, using a teeming witches brew of wages, bonuses, options, and the rest as bait, the totality presents a bizarre scene in contrast with which the hiring and paying of top executives by nationalized industries is almost pastoral.

I have no reason to suspect anything untoward in the determination of top-management compensation in private corporations, or that the arrangements are consciously devious. But the top-management compensation situation that has developed in private corporations is murky and highly charged. The private corporation as an institution, as well as the economy, would be well served if the situation could be cleared up—and made clear—before it explodes. I would add that nationalized industry is fortunate not to be burdened with this one.

THE NATIONALIZED FIRM IN YUGOSLAVIA

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The neoclassical theory of the firm assumed an entity operating in a market environment in competition with other private, profit-seeking organizations. Within the firm, specialization and the division of labor in production were considered to be well developed. But this factor in itself could not explain the existence of firms, since the same phenomenon occurred among firms as well as within them. Rather the existence of these relatively large organized units was to be explained by the existence of externalities—gains obtained and losses avoided as the result of collaborative action. An important aspect of this was thought to be the economies resulting from centralized direction of activities.

The rise of management science, without changing this picture fundamentally, has provided major new insights into the complexity of enterprise organization—a complexity which persists even when quite modest numbers of people are involved. The growing specialization and division of labor within management itself has provided much evidence of this complexity. Closely related is the role of information, communication procedures, and uncertainty in influencing enterprise decisions. But the picture of the firm which emerges from this line of study is still restricted by the limited range of organizational forms available for study in Western countries, and particularly in the United States, where the bulk of the work still seems to be done. In a complex organization burdened by substantial externalities, there may be a great deal to be learned by varying the form of organization. For the effect may well be to generate much new information and consequently new decision patterns, some of which may prove to be more effective than those evolving from contexts presently under study.

Yugoslav nationalized firms offer a highly original combination of features with whose operation there is now a twelve-year experience. In this paper a few general and somewhat speculative comments will be offered as to the functioning of this type of organization and its significance for the understanding of problems of enterprise decision making in general.

I

At first glance the fact that Yugoslav firms operate in a market would seem to be the most important fact determining their behavior.

However, this need not be the case; in fact I think it is easy to exaggerate the necessary causal impact of the market environment on firms in general. The point is that there are a wide variety of circumstances in which market pressures are scarcely felt. This is not just a matter of monopoly position or the absence of close substitutes. It is also—and perhaps even more importantly—the nature of the goals which govern decision making. A nationalized firm need not seek profits or strive to satisfy consumers in order to survive. A government need not and indeed may not be capable of generating appropriate goals in the enterprises it owns or controls.

The market does put the making of deals at the center of enterprise operation and as a consequence develops more or less naturally money measures of performance which are likely to be adopted simply because of their convenience and regardless of goals. This in itself may tend to create pressures toward more conventional orientation both by government and by firm managements; but the point is that it need not, for the pressures of the market are in keen competition with political pressures in influencing nationalized firm decisions.

Naturally the state in Yugoslavia plays a considerable role in defining the framework in which nationalized firms operate and in controlling their decision processes. Firms are subject to very heavy taxation, their contributions covering about half of governmental budget expenditures in recent years, with excise taxes covering an additional sixth [5, pp. 46-49] [9, p. 124]. In principle, the policy has been to establish regular and anonymous tax forms rather than making arbitrary deductions from the incomes of individual enterprises. In practice, however, the tax system has been sharply overhauled about every other year, and a variety of discriminatory penalties and favors have proliferated. Control of depreciation allowances and also of expenditures for capital maintenance has been maintained from the beginning, though recently there has been some discussion about ending the latter. Over the last ten years there has been a rather steady growth in the scope of price controls until by 1962 about two-thirds of the value of industrial goods was subject to this control [3, Dec. 21, 1963]. On the other hand, governmental participation in the setting of wage differentials—a feature of the early operation of the system—no longer has any legal basis.

As might be expected, the Yugoslav firm is subject to political pressure from several sources. Because it is often exercised informally, this kind of pressure is very difficult to evaluate, but it is clearly substantial. The manager of the enterprise is the legal representative of the state within the firm and is expected to see that regulations are obeyed. The local government plays some role in his appointment and general

regulations set minimum qualifications. In addition to the director, both the League of Communists and the trade-unions are expected to exhort participants to appropriate levels of performance and to serve as watchdogs of that performance. For example, a union is said recently to have stepped in to prevent salaries being set for managers at a level deemed too high in relation to other salaries within the firm.[1] Local governments have been known to put pressure on firms for additional income for local purposes and have attempted to influence price policies as well. However, legislation has now somewhat restricted the power of local governments to act in this way. Despite such restrictions, it is obvious that the power of government over nationalized firms is very great, so that such informal influences are likely to remain an important feature of the system.

Though the forms and perhaps the extent are novel, the fact of involvement in both market and political pressures is a common feature of operation of nationalized firms in the West. Novel also is the extent of nationalization, which in Yugoslavia includes all industrial firms of significant size. As a consequence, a rather large fraction of the dealings of these firms is with other nationalized firms. Both the market and the state in its interventions serve to define the nature of these interactions. Competition is one manifestation of this interaction, and observers are agreed that quality improvement has been one of the products. The administrative decentralization, which has provided some autonomy to both republican and local governments, has also been a factor promoting competition as territorial units vie, among other things, to expand their tax bases. That this interfirm competition can be quite keen is testified by a recent anecdote concerning the competitive granting of price rebates by chocolate firms; a firm which was apparently losing out in the struggle complained of its intensity and denounced this price shading as "unfair competition" [1, Feb. 19, 1964].

However, a very serious problem has arisen in the form of collaboration among firms in nationalized industry. The extreme version of this—monopoly—is of frequent occurrence in the sense that in a small and developing economy like that of Yugoslavia there are very many products having only a single producer. Since many internal markets are still well isolated from world market trends, this offers opportunities to firms which have often not been resisted and which account in part for the growth of price regulation. Instances of price fixing and market sharing agreements are reported rather frequently in the press. Though there are antitrust regulations against this, the state nevertheless has often provided a good deal of indirect assistance to attempts at

[1] This story was told to the author by a Yugoslav management expert.

collaboration. Until the foreign trade reform of 1961, the allocation of foreign exchange among firms by economic associations was a major instrument of informal agreement. Since that time the movement toward economic integration of firms has no doubt provided a convenient device.

Nationalized firms almost by definition are easier for the state to reorganize than private firms; so that it should be relatively easy to break up nationalized firms in order to stimulate competition. However, in a small economy domestic competition is inevitably relatively less viable. In Yugoslavia perhaps the major gain from this sort of control has come from regulation of the size of urban wholesale trading establishments. A series of reorganizations in the early days of the new economic system has led to the development of a comparatively efficient and reasonably competitive distribution system—a sector which in developing economies is generally notorious for its inefficiency.

Variations in state policy toward firm size and cooperation among firms suggest the difficulties that must be faced. A set of syndicalist industrial associations was a part of the original worker management legislation of 1950, but these were abolished in 1952 when it became apparent that they were being used to circumvent market competition. Then in the mid-fifties a new set of associations was established to serve research and informational functions and which in fact were a basis for more cooperation. More recently, concern over export markets and substantial excess capacities in some industries has led to new stimulation for association. Shipbuilding represents one instance in which common export effort seems to provide a major stimulus, whereas in steel and textiles economies of integrated operation seem to be the dominant consideration [3, Mar. 30, 1963] [1, Feb. 13, 1964]. The dangers in this movement toward cooperation and merger can be illustrated by the newly integrated firm Makedonka which will process over 80 percent of its region's textiles, or from the expansion of Zenica, already the nation's largest steel producer, by integration with a mine and two existing mills [1, Feb. 17, 1964]. A variety of forms of integration have been concocted but none as yet that will in themselves prevent the firm from profiting in socially harmful ways from market control. For example, the chocolate firm mentioned above decided to try to get an agreement in its industrial association to prevent further price competition.

II

Aside from the number of firms involved and the consequently more pressing need for impersonal control and performance-evaluation schemes, the problems described above are not fundamentally different

from those faced by governments operating nationalized firms in other market economies. The really distinctive feature of the Yugoslav case is the use of workers' management as an important element in the internal decision process of nationalized firms. One might anticipate that this would lead to strikingly different enterprise decision patterns and would also strongly influence the kinds of problems already mentioned.

The introduction of workers' management in industrial factories was a revolution from above and from the beginning was rather carefully controlled by the instruments for exerting political influence. Elsewhere I have argued that in the early years the manager generally ran the firm, though the workers' councils often did manage to make their voices heard on questions of wages and hours [10]. In searching the Yugoslav press, it is not difficult to find evidence that director dominance is still a factor in enterprise decision making. For instance, a recent spate of articles in the League of Communists' newspaper show, disapprovingly, managers ignoring the rights of workers and their councils [1, Jan. 6, 9, 28 and Feb. 7, 1964]. Three of these dealt with the unjustified firing of workers and technicians by managers; the fourth with a manager who reportedly never consulted his workers' council, merely seeking their approval of his decisions after the fact. The cases appear to have come to light mainly because illegal acts were also involved; in each case it seems clear, however, that the workers' councils were not acting effectively to control the managers' actions.

Even instances which are cited to illustrate the power of the councils are not always convincing. The story was recently told of a Serbian textile factory in which the management entirely ignored the workers' council. When finally a meeting was called to go through the formality of approving the annual plan, the entire council was silent, refusing to vote on the ground that they were not prepared. A League official was present, supported the workers and brought the issue to the attention of "union, party and government circles" [8]. If it takes action by an official of the League of Communists to bring council members to the exercise of their formal rights, the council's power base must be rather insecure.

Despite the continued appearance in the press of anecdotal material of this kind, there is strong evidence that the workers' councils generally are functioning more effectively today than a decade ago both as representatives of worker interests and as an effective influence on enterprise decision making. Perhaps the most convincing evidence is the attitude of the state: legislation since about 1956 has clearly trended toward increasing the influence of workers' councils vis-à-vis both manager and management board. Most important, as two recent stud-

ies agree, is the transfer of full authority over hiring and firing decisions to the workers' council [4, Chap. 9] [7, p. 83]. Though there are rules governing qualifications, especially for technical personnel, both the number of people to be hired or laid off and their names are matters for the workers' council to decide. This is even true of the manager, though the local government is represented on the appointing committee, and is concerned in dismissal procedures.

A second area in which workers' council authority has been greatly strengthened is in fixing wages. Both local governments and unions were previously involved with the workers' council in this decision, but now competence is entirely in the council's hands. Of course the watchdog agencies are concerned about inequities and may intervene, as the above-cited story about a manager's salary testifies; but this is now exceptional rather than regular practice. And it may well be true that where a strong workers' council exists it is in a position to resist such pressures. An implication of this increased authority has been that control by external agencies of the allocation of funds within the firm has been greatly weakened. This is partly the result of a deliberate policy of relaxing control of internal enterprise decisions and partly a consequence of the difficulty in determining the size of the "surplus" when national wage standards are no longer available for estimating labor costs. There is very wide variation in the ratio of funds at the free disposal of firms to personal incomes, which reflects the state of the market, the quality and size of the capital stock, and the wage rates fixed by the workers' councils—all in unknown proportions. In recent times, successful firms (in 1962, 355 of 2,684 industrial enterprises operated at a loss) have been passing out 75-80 percent of these funds as additional wages [5, pp. 76-79]. But when the figure rises above this level or falls below it, the union, for example, finds it difficult to decide whether there is a question of social responsibility or whether the deviation is a result of arbitrary setting of wage levels by the workers' councils.

In fifteen years of experience with the institutions of workers' management quite a substantial fraction of the workers (in most places ranging from a quarter to a third of the current staff) have participated as council or board members. Both the formal legislation and the trend of changes in that legislation are evidence of governmental and League interest in strengthening the institutions. Increasing levels of education and increasing experience with the problems of market operation and with the techniques of becoming informed about these problems must have played a role in stimulating worker interest in enterprise decisions. A sample survey of workers in the small industrial town of Varaždin indicated that perhaps as many as a third could be

rated as reasonably well informed about the operation of their respective enterprises (interestingly enough, fewer than half of the members of the League of Communists who were interviewed were so rated) [6, pp. 47-48]. The study certainly indicates that the informational basis essential for the exercise of power has been established; it might be noted that a priori the town of Varaždin, with its small size (under 30,000) and large immigration from the countryside, is not an especially favorable site for this kind of test.

There is no real basis for confident generalization as to the internal power structure of the Yugoslav nationalized firm, except that there is a great deal of diversity. One might guess that external political factors are likely to be greater in the smaller towns and more backward areas, where political interest tends to be concentrated on the performance of a few enterprises and where the techniques of politicians are likely to be cruder. One might also guess that firms with a large fraction of unskilled employees or with a large fraction of recent immigrants from the villages would also have less effective workers' councils, in one or the other of the two senses mentioned at the beginning of this section. The anecdotes which deal with malfunctioning would support these guesses and some Yugoslavs with broad experience with management problems would agree. There is no doubt that in a large fraction of firms the workers' council and even the entire work force constitute a group whose primary interests managers ignore at their peril.

III

The notion of consumer sovereignty should relate to the production as well as the consumption activities of individuals. The provision of labor services possessing negative marginal utility contributes to individual satisfaction in the same way as does the consumption of valuable services. Though recognized in principle, this symmetry is not always apparent either in economic theory or in the more philosophical discussions of the question. In the latter, the influencing of tastes tends to dominate discussions of consumption and alienation tends to dominate discussions of production; in the former it seems to be widely assumed that mobility, free choice of occupation, and reasonably free access to training are sufficient guarantees of sovereignty. This I think is a mistake. Not only does alienation relate to consumption but—and this is the relevant point for the present paper—sovereignty is closely tied both to the extent to which attitudes are manipulated within the enterprise and to the range of choice of human activities within the enterprise.

Workplace interactions are of a peculiar intensity which determine

many aspects of behavior during half or more of the waking hours of participants. And since the decline of Taylorism, there is a common view that a great many enterprise decisions, including the technical ones, have no one-best-answer regardless of the goals of participants; that is, the complexity of the enterprise makes it a much more open environment with respect to the range of feasible choices by participants. Also, as noted above, enterprise labor is a public good in the sense that one man's behavior and satisfactions are stongly influenced by the behavior of other participants in the firm's activities.

Most of the difficulties with the concept of individual sovereignty stem from public good aspects of human interaction. What is clear is that nonmarket processes become more interesting where externalities are important. Where individuals and their interests differ, the solutions adopted and even the range of alternatives considered come to depend strongly on the distribution of power and influence within the decision unit. This is what makes the Yugoslav experiment with workers' management especially interesting: the possibility that a new power arrangement within the firm will reveal, through the process just outlined, new solutions to old problems of enterprise decision making—solutions whose very consideration is blocked by the limited range of enterprise power structures with which we have any experience.

It is clear that the organization of the Yugoslav firm does not eliminate all causes of conflict among participants. In fact, relatively stable interest conflicts may be generated and to some extent are fostered by the organizational form. Both the manager and the management board have some independent authority which may at times be opposed to the interests of the workers' council—an authority which is based partly on the manager's role as representative of the state and partly on the institutionalization of several internal organs with decision-making powers [7, Chaps. 3-4]. Also, the manager must often stand as a defender of the interests of the technical and managerial staff before a council which by law contains a majority of production workers.

Nor are the workers without some problems of interest conflict among themselves. Recently a referendum was held among all workers in Interplet, a textile enterprise, to decide whether to close down a mill which had been making heavy losses since it went into production three years ago. The decision was overwhelmingly for shutting down though, not unexpectedly, some three-fourths of the workers in the affected mill voted to continue operation [1, Nov. 18, 1964]. Another recent description shows the local government and workers' council of a chemical firm unable to reach agreement on the appointing of a new manager, the difficult choice lying between the president of the

local communal government and the secretary of the factory committee of the League of Communists. The decision was finally achieved by the workers' council after the withdrawal of the former official from the competition without giving cause [2, Nov. 21, 1964]. It is indeed striking to find decisions of these kinds being made by workers' representatives, but interest conflicts are not thereby avoided.

In any social unit the most divisive issue is likely to be that of fair shares. In the Yugoslav enterprise this conflict is probably somewhat muted by the profit sharing scheme which provides some generalized incentive toward the making of profitable choices by the workers—a point which has recently been illustrated in a model of group decision making [11, Chap. 9]. A practical example was the already mentioned case of a workers' council wanting to pay an exceptionally high salary to the enterprise manager in order to attract an outstanding person. The other side of the coin is suggested by one of the questions in the Varaždin study. Workers were asked whether they thought profits should be shared equally among all workers or in proportion to regular earnings; they divided almost equally in their answers [6, p. 89].

There is another area in which the Yugoslav power structure seems to differ substantially from that of the typical nationalized (or large-scale private) firm in the West: the absence of a strong trade-union in the traditional sense of the term. A hierarchy containing many officers full time on the union payroll and devoted jointly to defense of the workers' interests and the preservation of the union, can often serve to sharpen worker-management conflicts and at the least inserts a new interest group into the picture. To mention only one place in which this difference is evident, information regarding commercial operation of the firm is often withheld from Western union officials, but Yugoslav management is required by law to provide all information desired by the workers' councils.

A list of differences stemming from the unique internal organization of the Yugoslav nationalized firm could be extended almost at will. An analysis of the implications of these differences can hardly be undertaken here; indeed, lack of detailed information on actual operation of these firms would force one into the realm of speculation at a very early stage. But I would like to suggest that it is possible to exaggerate the impact of these differences. This is not intended as a criticism of Yugoslav practice but rather as an appreciation of the extent to which the work force and its representatives have become involved in enterprise decision making in the West. One might question whether the referendum at Interplet or the final council meeting of the chemical firm were the actual scenes of effective decision making, though it does seem likely that workers' representatives were involved in the decision pro-

cess in a significant way. But such decisions are not generally taken in Western nationalized firms without giving serious thought to worker and union reaction. Wages and working conditions are not formally the bailiwick of workers in the West, but their influence on these decisions is often substantial, while in Yugoslavia the authority of the director and the influence of the various watchdog agencies is far from nil. Even in the area of commercial secrets motives may be mixed. It has been claimed that workers' councils had no knowledge of several price fixing agreements in brick making and other industries [8]. There are gains to being uninformed on certain matters.

But it would be inexpedient to close on this note; for what is left out of account may well be the most important aspect of the problem: the effect on worker attitudes and behavior of changes in information flows and influence at the lowest level of interpersonal contact. A distinguishing feature of group decision processes is their tendency to be very sensitive with respect to apparently minor changes in data. A large number of Yugoslav firms have not yet achieved or only recently achieved effective workers' council influence on many internal decision processes. Whether in these firms new and more effective forms of workplace interactions are also developing remains an open question.

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DISCUSSION

George Macesich: Professors Lewis and Ward have examined in an interesting manner nationalized industries in Great Britain and Yugoslavia. Both countries are well known for their ability to generate and apply new ideas. And both consequently have considerable worldwide influence. Their experiences are indeed well worth examining.

I have several comments on the two papers. Let me first turn to Professor Lewis' paper. Most economists will agree that all complications and qualifications aside, optimum output is attained by marginal-cost pricing. Simply put, the socially best amount to produce is the amount whose price per unit is equal to marginal cost. It is after all little more than a standard proposition in welfare economics. And if I understand his paper, he does not dispute this proposition, nor does it appear that the application of this proposition is the paper's principal message.

More important, Professor Lewis' paper does serve a very useful purpose in drawing one or two interesting and important implications from British experience for some American industries. For example, the American steel industry would appear to be well on its way towards creating a climate favorable to the type of regulation current in public utilities. I agree with Professor Lewis that if this does in fact occur, the steel industry can lay principal blame on its own managers. I would add that the economic illiterates employed on the industry's public relation staffs have also made a contribution.

A case in point is the recent commotion over "administered price" inflation and the steel industry's supposed leading role in such inflation. The spectacle of otherwise sophisticated members of congressional committees listening and even eliciting economic nonsense from various political advocates is surely a wonder to behold. Indeed, one committee produced a staff study in currently fashionable Harvardese complete with appendices reporting *ex cathedra* the dominance of "market power" in the steel industry and its considerable role in the inflation of the 1950's. A closer examination of this particular staff study reveals that its predictive power is zero owing to implicit requirements that the United States must be on the steel monetary standard and in a world of rigidly complementary goods. Needless to say, both of these conditions remain to be fulfilled in the world as we know it.

The steel industry's inept defense turned on attempting to blame unions for the alleged price rises and its own helplessness before such "all powerful" unions. I suspect that this is but another example of industry attempting to place the blame for inflation on unions in the hope of advancing industry's public image and downgrading the union's image. It is also an illustration of the misuse of the term inflation which regards every rise in an important individual commodity price as inflation and disregards quality changes and relative price declines in many of the less obvious sectors. In their zeal to discredit unionization, the steel industry has set itself up for nationalization or, more imminently, for treatment as a quasi-public utility.

A reading of British postwar experience with nationalization suggests to me that it is inconclusive so far as strict economic criteria are concerned. To some it is an article of faith. To others it is simply a matter of expediency. Professor Lewis argues that it may also be educational to the general public by introducing people to "society's great and never ending economizing adventure." If so, the Yugoslav experiment with which Professor Ward deals in his paper promises to be even more educational.

Professor Ward's paper considers Yugoslavia's unique system of workers' management. I would certainly agree with him that the system has come a long way since its introduction in 1950. The disagreement and confusion among supporters and critics of Yugoslavia's system seems to me to be traceable to the implicit use of different criteria in judging the viability of the country's system. Critics appear to use as a criterion the inability of the system to give scope to "talent," such as directors and technicians. Supporters, on the other hand, use as their criterion the ability of the system to give scope to the "untalented," such as production line workers. The untalented cannot translate their grievances into creative effort and so are more likely to be more troublesome than the talented. The problem then is how to give scope to the untalented while at the same time insuring the system against them.

If judged by the latter criterion, the Yugoslav system appears well engineered. How well the system performs on this score is suggested by the success of so-called "blue-collar" workers (not all of whom, of course, are untalented) in moving into managerial position. Studies reviewing the operation of the system from 1950 to 1960 report 77 percent of the blue-collar workers and 24 percent of the white-collar workers have participated in some capacity in managing the country's enterprises. Over 70 percent of these workers had less than an American high school education. It would seem that the Yugoslavs are full force introducing their people to "society's great and never ending economizing adventure."

My own observations of worker management in practice in such firms as Iskra in Kranj, Slovenia, which is in the more developed region of the country and in several firms located in less developed regions such as Macedonia suggest to me the importance of the quality of the group participating in management. It is really a unique system for on the job training of management, and many workers do in fact rise to very responsible positions. Some evidence of the degree to which "professionalization" of the managerial functions in a Yugoslav firm has occurred is available. Mirjana Poček in a study of 143 of the 193 enterprises located in the Zagreb area during the period 1953-58 concludes that professionalization does not seem to be a serious problem. Her study is based on worker mobility into an out of offices in the principal organizations (president of the workers' council, president of the managing board, president of the trade-union, and secretary of the League of Communists within an enterprise). Unfortunately, we do not have a comparable study for the mobility of the director. Scattered evidence suggests, however, that his position is not as secure as the critics of the system would have one believe. On the other hand, it would be interesting to see how much the mobility of the key personnel depends on their political affiliations.

The really critical problem in the Yugoslav socialist firm is the propriety of the allocation of additional capital among competing uses. The worker-managers have a vested interest in the actual or potential profitability of their own firm. In a private enterprise economy the same may be true, but the price system exerts a continual check on their ambitions. The socialist firm in Yugoslavia does have a bias toward the use of excessively capital intensive techniques. This derives in part from a desire to hold down the number of workers who share in the *pita*, which is a Serbian pie.

I have one suggestion for improving the system and it is to broaden the profit sharing base. For example, instead of sharing in the profits of their firm, the profits of the various firms could be pooled by the commune and then shared in a periodic dividend by all worker-managers within the commune's territory. Such a solution might have an undesirable side effect in that it might lead to a misallocation of resources among the country's eight hundred or so communes. This could be corrected, however, by appropriate taxes administered on the federal or national level. Capital could be given more rational geographic distribution. And also Professor Lewis' brand of economic education would be broadened.

HERBERT G. GRUBEL: In my comments I shall concentrate attention on Professor Ward's perceptive observations on the problems of decision making in the Yugoslav firm.

In the short time allotted to me I shall argue that the changing and confusing picture of the decision-making process in the Yugoslav firm in recent years is due to the presence of a basic dilemma. The dilemma is that the institution of workers' councils and the public ownership of capital is incompatible with the objectives of efficient allocation of resources and economic growth.

Let me begin the defense of this proposition by recalling the following well-known fact about the classical model of the capitalist firm. When it is assumed that the firm operates in competitive markets and under certainty, then the given and known production function and price parameters yield only one unique optimizing solution. In principle, it therefore makes no difference who directs the operation of the firm, since the constraints and alternative uses are known. In a dynamic world of uncertain prices and production functions, however, someone has to take a position on the outcome of the future events. This role is assumed by the owner of the firm's equity and by virtue of assuming the risk he acquires the right to make the decisions. But it should be noted that while conditions in the future are uncertain, the entrepreneur nevertheless bases his decisions on price parameters given to him exogenously, including the price of capital. And, important for my analysis, it is pure self-interest, the effort to maximize his personal utility function which motivates the entrepreneur to actions which are ultimately in the social interest.

In order to derive the benefits which decentralized decision making and scarcity prices have on the efficiency of resource allocation and the maximization of consumer satisfaction, the Yugoslav firm was organized so that it would operate just like a competitive firm. The firm was given the liberty to make input and output decisions purely on the basis of market determined

prices. The important difference in comparison with the Western firm, however, is the public ownership of the means of production.

The power to make input and output decisions must ultimately be tied up with the ability and willingness to accept the consequences of a wrong decision in a meaningful way. If we assume that prices of factors and products in Yugoslavia are competitively determined, then it follows that the capital invested in the enterprise carries this burden, just as in the capitalist firm Therefore, the firm's decision-making authority should be vested in the hands of a government representative, whose pay (and social status, etc.) should be based on his ability to make the proper allocative decisions and maximize the value of the enterprise for the government. It is quite clear that such an institutional arrangements leads to statically efficient resource allocation. Economic growth is determined by the government's decision to channel savings into enterprises so that marginal rates of return are equalized in all uses or so that other social objectives can be achieved. Theoretically, therefore, the static allocation of resources is not necessarily affected by the public ownership of capital.

For ideological reasons, however, Yugoslav firms are not supposed to be run by state-appointed, state-supervised managers. Instead, workers' councils are empowered to make basic input-output decisions. The heart of my argument is that if in fact they have these powers these councils are faced with the insoluble conflict between looking after their own interests and looking after those of the state. These interests do not coincide; therefore efficient operation of the firms requires a social conscience on the part of the workers as decision-makers which, the record shows, they do not possess. Let me sketch for you—and it can be no more than a sketch—how some recent regulations of Yugoslav firms can be explained as having arisen as a result of the operation of this dilemma.

First, depreciation allowances and expenditure for capital maintenance had to be regulated by the state. Otherwise workers would decide on economically unjustifiable depreciation rates, thus inflating profits available for distribution to themselves.

Second, legislation regulating the share of profits distributable as wages had to be introduced. Workers were willing to appropriate the positive returns from risk-taking whenever they accrued.

Third, firms turned out to be very ingenious in avoiding the consequences of losses made. Liquid positions were improved and true losses hidden for a long time by delay of payments, etc. Thus laws prescribing the maximum length of time over which debts could remain unsettled were passed.

Fourth, bankruptcy laws had to be tightened. The problem of making the consequences of bankruptcy a really effective stick, as it is in the capitalist society, is one of the most serious obstacles and probably an insoluble one to the proper functioning of the workers' council decision-making process. Workers tend to have no real assets the loss of which can be threatened. It is socially and politically unthinkable that their wages would be reduced below the current norm for any length of time. Workers will always know this. Consequently the state's capital is assuming ultimately the risk of decisions made

by a group of individuals not primarily interested in its preservation. The allocation of the state's capital resources out of current savings into genuine growth, difficult enough as it is in the real world where prices of final products are not competitively determined, tends to become even more muddled and obscured by the need to rescue firms in trouble because of mismanagement.

Let me close by arguing that the institution of workers' councils leads theoretically to a misallocation of resources because the true price of labor is distorted—a point made by B. Ward in his 1958 paper in the *American Economic Review*. When the council decides on hiring an additional worker, this decision is made on the basis not of the going wage rate but the wage rate plus a share of the bonus to be paid out of profits. Even if the true wage is considered to be a base rate plus some average bonus, the fact remains that from the council's point of view the cost of the added man may be higher or lower than that, depending on expected profits. Thus one of the basic requirements for efficiency—a fixed, market-determined wage rate—is not met, implying all the well-known additional distortions in the general equilibrium setting.

In my discussion so far I have abstracted from other difficulties besetting the Yugoslav economy, such as inflation, collusion among firms, balance-of-payments problems, and others. Given these difficulties and the basic inconsistency of workers' councils decision making with public ownership of capital, the need arises to explain what many observers regard as a reasonably satisfactory performance of the Yugoslav economy in the recent past. The explanation has to be sought in the fact that in reality workers' councils have not had the decision-making power theoretically belonging to them. Professor Ward refers to the reported incidents of managers' domineering over their councils. Furthermore, the policies just outlined and the extremely flexible taxation laws of the government mentioned by Professor Ward have succeeded in remedying many of the gross manifestations of the basic dilemma. At the same time, however, these laws tend to reduce the basic freedom of choice for the firm and thus threaten to defeat ultimately the objective of the experiment.

William G. Shepherd: Pricing and investment policy are Professor Lewis' main topics, and they will be mine too in this comment. The "overflowing river" of reports and debates in Britain has steadily pushed the government and the industries toward the application of "commercial" criteria in guiding and rating industry performance. This has boiled down in practice to two things: company-wide profits targets which are "comparable" to those in private industry, and the elimination of cross-subsidizing, which would permit unprofitable operations to continue.

There are merits to these and related guidelines. They lead to improved costing, more careful screening of investment, and an awareness that nationalized industries belong in a general allocational framework. But their limitations have frequently been overlooked. Though I have discussed this elsewhere, a few points may bear repeating here. Many all-too-familiar public

utility rate structure problems remain entirely unsolved by commercial criteria alone. For all the major British nationalized industries (electricity, railways, coal, telephones, gas), commercial criteria are therefore incomplete by themselves. Also, overall profit rates may do little to determine output in the thousands of separate markets in which these firms sell. Repercussions external to the public firm (as from mine and rail closures and reorganization) would be excluded from commercial calculations by the industries, and yet often these have not been adequately dealt with by direct government programs. Therefore, in many cases, commercial guidelines would cause a departure from patterns based on real costs and benefits. This seems to me a fair conclusion, despite the current controversies over external effects. Also, the proper "comparable" profit rates are difficult, perhaps impossible, to discover.

Less obvious, the commercial guidelines imitate certain market patterns (profits, closures) but close off other commercial activities which are important to any large corporation, public or private. Chief among these is diversification across industry lines; this is now well known to be necessary and integral with growth and, if you will, with dynamic efficiency of the firm. Yet this gap in the "new" approach toward public firms has been given virtually no attention by professional economists.

So much for analytical points. A statistical look at actual behavior of public firms in the fuel sector (especially the National Coal Board) indicated to me that wholesale misallocation had not occurred for want of commercial guidance. More recently a study of the British telephone system has suggested that the commercial approach may in part lead directly away from efficient patterns. These case studies cast empirical doubt (in addition to the theoretical objections) on the necessity for relying heavily on the commercial criteria which have been discussed.

I take it that Professor Lewis and I are in broad agreement on these points. Lewis also comes down hard on the peculiar meaning of "profit" for nationalized industries such as these, and again I mainly agree. But if points like these are as clear and compelling as they seem, how could the main drift of policy have gone so seriously wrong, as we both think it has? Is public ownership really a vehicle of public understanding? Perhaps the openness, explicit directness, and public involvement have, after all, not guaranteed a proper understanding of the policy problems. To draw another parallel, public regulation has trained the floodlights on many (not all) American public utilities, with a brightness not far less than for British public firms. Yet Professor Lewis is among those ranking experts who would credit American utility regulation with something less than Divine Light.

The British-American parallels I would draw from all this bear mainly on American regulatory policy toward public utilities. Commissions in this country focus on overall rates of return; so does the British commercial approach. In fact, both approaches have been arriving at pretty much the same percentage rates of return. There are sticky closure problems (in railroads) where external effects need to be allowed for but usually are not in any explicit way. There is (at least formally) a division of responsibility, with the utility com-

panies leaving the determination of the social interest to the commissions who, on their part, make very few cost-benefit calculations. Similarly, the British commercial policy would endeavor to draw a line between commercial and social considerations and leave the latter entirely to the ministries. There is also, in American utilities, a good deal of cross-subsidizing (such as in the telephone system). Some of it is only occasionally challenged, and in some cases it may nicely (though not explicitly) approximate a socially efficient pattern.

Therefore (except for the cross-subsidizing), the British commercial approach hankers after something remarkably similar from an analytic viewpoint to the main outlines of American utility regulation. And yet American economists have recently begun to insist that much of this regulation is economically empty. Indeed, who has stressed this more forcefully than Professor Ben Lewis, unless—and here is a remarkable and persuasive coincidence—it be Professor George Stigler? Overall rates of return leave most rate structure problems unsolved; moreover, one side effect may be an excessive use of capital, as Averch and Johnson and Welliscz have pointed out. In fact, the guidelines most heavily stressed in the British commercial approach are just the ones which—as applied in utility regulation—American economists are hard at work undermining.

For future British policy, there are two directions in which economic research and advice are mainly needed. First (and inevitably), improvements are needed in methods of calculating the appropriate costs and benefits for price and investment decisions. This embraces both marginal-cost pricing in the usual sense and external effects, too. Second, the integration of government and industry policies requires much more than the simple attempt to draw the line between social and commercial elements. In many important cases, such as mine and rail closures, no such line can be drawn.

On any practical view, British public firms will continue to cope with commercial-social problems within a mixed and changing framework of government and industry responsibility. There is room for much study and experimenting in this, though one should not expect public discussion to be wise or neutral.

At this point, Britain appears to have no great leeway, in resources or public attention, for experimenting with nationalized industries—new or old—on a large scale. Moreover, there are deep doctrinal obstacles to some of the experiments that might be tried, such as diversification and dropping of branches by public firms. Even so, much can be learned from British experience; it would be too hopeful, I fear, to say that the British had as much to learn from parallel American experience.

I too am inspired to poesy, to a brief

Economist's Ode to Public Ownership

O curious social strategem,
I cannot here avoid

Acknowledging my debt to you
For keeping me employed,

With your quasi-competition
And cross-subsidization,
Joint costs and target profit rates
And government dictation,

With thy thrilling externalities
And the marginal-cost price game—
O, the more thy problems change about,
The more they stay the same.

H. EDWARD ENGLISH: I am very much in sympathy with the main theme of Professor Lewis' paper. He is in effect arguing that in comparative economic systems we are all involved in a search for the second best and had better admit it. I would also agree with the point, which is more implicit than explicit in the paper, that under any system of organization, it is to the factors which determine the environment for managerial decisions that one should turn to promote improvement in performance.

But this leads me at once to my principal criticism of Professor Lewis' views. He rightly parallels the collectivism of nationalized industry with that of the private corporation, but he attributes to the fact of collective ownership the reason why both depart from the criteria of classical economic theory. Left to themselves, proprietors should surely have a common interest in the maximization of profit (whether they benefit through social or private dividends or through the capitalization of returns in rising share values). The literature on nationalized industry, much of it written by students of public administration, indicates that the factors determining managerial behavior play a more important role in modern industry, however owned, and are likely to be a more important source of explanations for departures from theoretical criteria based on the profit maximization assumption. Furthermore, if we are interested in identifying the differences between public and private enterprise, it is surely to the set of factors governing managerial decisions that we should turn our attention. Collectivity of ownership is only one of these.

Professor Lewis deals with pricing and investment policies under nationalized and private industry and finds the differing versions of the profit motive a principal distinguishing feature. Although he later admits that for the real world of oligopolistic private enterprise, as for the nationalized industry, profit is in part a function of administered prices, I think he places too much emphasis on allegedly distinctive implications of the rule employed in the United Kingdom that publicly-owned enterprises shall "balance their revenue deficits with revenue surpluses over five-year periods."

There is very little in the paper on the pricing implications of this rule. Had Professor Lewis explored these further, I believe that some of his conclusions might have been modified. The important issue is whether under private or nationalized industry we are likely to come closer to the allocation norm,

properly qualified to account for social as well as private welfare criteria. In the theory of imperfectly competitive enterprises, there is no general reason why it will be the pursuit of maximum profit or of zero profit which will come closer to the allocation criterion, nor for that matter is there necessarily much difference. It depends, of course, on the particular demand-cost relationships in each industry. One possibility which might be tested empirically is that involving a demand curve which intersects the average cost near its minimum point but which has a low enough elasticity to ensure substantial profits at lower levels of output. Under such circumstances the $P = AC$ standard will approximate the $P = MC$ standard more closely than will $MC = MR$. Since any tendency to overload capital, as reflected in a move to the right, up the average cost curve, is likely to promote the building of new capacity, the situation cited could be typical. Ironically, however, in the British situation the nationalized industries have often appeared to be under greater investment restraint than has private industry.

Turning to investment, Professor Lewis notes that profit is not a residual but "an explicitly contrived cost calculated to provide the industry with as much investment capital as the government believes should be forthcoming from the source." This distinction is in my opinion not very helpful. The theoretical definition of pure (i.e., residual) profits is as foreign to the familiar profit concept of private industry as it is to that of Britain's nationalized industries—a point which is later acknowledged by the author.

More important is his view that investment decisions are not very sensitive to profit expectations and that the rate of return itself represents "a policy decision." There might well be problems arising out of the administrative discretion involved in a completely socialized economy, but surely not in a mixed economy like the British. If in a mixed economy real productivity cannot be clothed in meaningful values for the purpose of testing the net benefits to consumers or to private industries using public goods and services, it must surely be because of the shortcomings or handicaps of management in the public enterprise, or because of the imperfections of private markets which make difficult a relevant comparison of returns.

Considerations which affect any assessment of the economic appropriateness of rates of return in the real world include monopoly rents and such considerations as whether capital is valued on replacement, original cost, or some other basis. (In British public enterprise revenue is supposed to include "interest, depreciation on a replacement cost basis, and 'adequate allocations to general reserves which will be available *inter alia* as a contribution towards their capital development and as a safeguard against premature obsolescence and similar contingencies.' ") Frequently in public utility regulation the resistance to replacement cost valuation has been accompanied by tolerance of rates of return which have been higher than required to assure access to the capital market if replacement values had been used. Implicitly the ability of any kind of enterprise to rely on internal finance would seem to introduce a probability of departure from theoretical allocation criteria, but isolation from the capital market is rarely complete. For example, as long as industry

development plans in Britain are discussed among firms as well as with the government, relative rates of return on different uses of the investment dollar are bound to receive some consideration.

As I indicated at the beginning of my comments, if one were to select and classify the factors affecting the management decision, one might evolve a frame of reference within which particular comparisons between private and public enterprises could best be made. Let us assume that the typical manager is governed by the profit motive modified by the various personal motives usually attributed to him—prestige and salary—which explain why he may opt for growth rather than short-run (and sometimes even long-run) profit maximization. His scope for discretion (or to put it another way, for departure from the $P = MC$ norm), is likely to be limited by several factors of which four appear paramount: (*a*) extent of surveillance and control by the owners; (*b*) the information available on returns to alternative uses of resources and especially capital; (*c*) the extent of competition in relevant product and factor markets; and (*d*) public policy.

Only the first directly concerns ownership. Professor Lewis has in my opinion rightly contrasted the blaze of publicity surrounding public enterprise with the relatively undisturbed life of private management. If the latter enjoys too much privacy, it is clear that at times public managers have had too little. It took many years for the British Parliament to learn how to distinguish the day-to-day operations of public enterprises from those more fundamental policy decisions which are the business of the people's representatives. Furthermore, the amount of attention given to nationalized firms has been extremely uneven, having often been governed by such improbable variables as the degree of public subsidy involved and the weakness of the private interests affected.

As for the other three factors cited, there appear to be no very general reasons why the nature of ownership need affect the information available, the extent of competition, or the impact of public policy. Particular observations may, however, be made. On the one hand, there will probably be more public consideration of alternative uses of resources assigned to public enterprise. On the other, a private enterprise is usually freer to expand into other industries which offer better profit opportunities. The extent of competition may often be greater in a private enterprise because public enterprise is more often adopted where monopoly is likely. It is my impression that governments desiring to intervene in private markets should more often have contemplated the "yardstick competition" approach. Swedish experience generally suggests that it probably deserves consideration elsewhere. The current British steel nationalization move is not a complete takeover, but the fact that all the larger firms are involved reflects a greater concern for a curious sort of political equity than for economic necessity.

Finally, one of the doubtful features of the relationship between state economic policy and the operations of public enterprises is that the latter are, of necessity, more likely than private firms to be sensitive to policy changes. The lag in modernization of Britain's railways has been explained in part by

the most direct effects of the notorious "stop-go" policies of the postwar years.

My argument is then that the performance of public or private enterprise should be judged in relation to the sort of factors cited and their impact on managerial decisions, and that the specific role of ownership, while sometimes consequential, can at least be put in better perspective by assessing all the forces affecting management.

This can be illustrated from some of the particular topics covered in Professor Lewis' paper. The effectiveness of incomes policy is surely determined primarily by the degree of competition and by policy to promote competition, and perhaps by the kind of information which promotes labor-management cooperation.

The most uneconomic cases of cross-subsidization are probably primarily the consequence of noncompetitive industry behavior, the absence of information about relative costs and net benefits, and the neglect of policies for covering the transitional losses from rationalization. Even the curious market for private managerial services, though it may be greatly influenced by the interdependency of demand and supply, is also affected by public policy; namely, the income tax.

In sum, then, I would call for an examination of the forces governing managerial decisions in private as well as public firms, with a view to better understanding the sources of departures from acceptable allocation criteria and placing in perspective the limited direct role of ownership per se.

ECONOMIC HISTORY: ITS CONTRIBUTION TO ECONOMIC EDUCATION, RESEARCH, AND POLICY

THE STATE OF ECONOMIC HISTORY

By DOUGLASS C. NORTH
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I wish to make two points in this paper: (1) that the quality of research in economic history is generally very poor and that the economics profession must take a large share of the blame and (2) that the new economic history falls short of the mark in remedying this problem.

Despite the fact that a good deal of economic history in the United States is taught in economics departments, there appears to be some schizophrenia on the part of economists between the way they look at the quality of research in economic history and the way in which they regard the research of colleagues engaged in other fields of economics. If economists were to apply the same critical standards to economic history that they apply to the rest of the field of economics, very little of today's economic history would be recognized as high-quality research. There appears to be an implicit notion that the criteria by which we judge economic history should differ from those used in judging economics.[1] If so, then we should turn the field back to the historians, who at least write with charm and style.

A moment's reflection on the part of any economist should convince him that to the extent that economic history moves beyond the simple cataloguing of facts, it must meet of necessity the same set of standards that we attempt to impose by the use of scientific methods in economics. It should not be necessary to elaborate this point, since the excellent article by Conrad and Meyer, "Economic Theory, Statistical Inference and Economic History,"[2] at the 1957 annual meetings, as well as the more recent statement by Bob Fogel in his book on railroads,[3] both make the point effectively. I am well aware that we frequently do not have either adequate theory or the statistical data to

[1] Indeed, three well-known economists at the 1957 annual meeting of the Economic History Association implied as much, when they were reported by Simon Kuznets to believe that economic theory had limited relevance for economic history and that the state of the field was, in fact, rather good. Perhaps these three economists had not bothered to read much economic history or simply wished to be polite amongst economic historians.

[2] *J. of Econ. Hist.*, Dec., 1957.

[3] *Railroads and American Economic Growth* (Johns Hopkins Press, 1964).

develop and test hypotheses in any definitive fashion. My point is that economic historians do not make use of the theory we do have. While it is true that we have no overall theory of economic growth worth the name and that therefore the grand theme of the economic rise and fall of nations cannot be treated in a formal fashion, we still know a good deal about productivity change and its sources; but little of the literature in economic history reflects any awareness of this fact. And for the rest of economic history, much of it deals with problems in which various fields of economic theory are directly relevant.

A summary statement of deficiencies of economic history is as follows: (1) Vast areas of economic history have not been treated at all; that is, treated in the sense that economic theory and statistics have been used to examine the past.[4] (2) Many writings in economic history are loaded with statements which have economic implications and imply causal relationships which are not only not supported in the research but which run counter to basic economic propositions. In fact, in most such cases, the author appears to be completely unaware of these implications. (3) Even more conspicuous is the character of the evidence advanced to support propositions. In good part it consists of a mishmash of quotations and oddly assorted statistics which do not provide any support or test for the propositions developed. (4) A good deal of economic history draws broad welfare conclusions which are by no stretch of the imagination warranted from the evidence cited. In fact, a general characteristic of economic history is that the treatment of propositions with broad welfare implications is typically undertaken without even a token acquaintance with welfare economics. Let me illustrate my point with respect to five broad areas of economic history.

First, the industrial revolution is still looked upon as the great threshold of economic history, and in turn technological change is regarded as the *deus ex machina* of this threshold. Quite aside from the fact that this does not seem to have inspired economic historians to do much analytical work on a theory of technological change (nor indeed even to have encouraged them to have any precise definition of technological change) no such simple view of the acceleration of growth of the Western world is consistent even with our limited knowledge of sources of productivity change. I would hazard the speculation that if we ever did the research necessary to get some crude idea of the magnitudes involved, we would discover that improved economic organiza-

[4] In a few other cases, there has been a great deal of research done on areas far beyond the extent to which they were important in economic history. Labor history is a case in point, where the tendency to identify the history of labor with the history of unionism is all too common despite the fact that trade-unions did not exceed 5 percent of the labor force before 1900. In a widely used current text, one-fifth of the book is devoted to trade-union history.

tion was as important as technological change in the development of the Western world between 1500 and 1830. I mean by this, improvements in the factor and product markets, reduction in impediments to efficient resource allocation, and economies of scale. Moreover, the complementarity between physical and human capital in the development, application, and spread of technological change requires equal analytical attention before we can begin to make sense on this subject. Clearly, we need to overhaul our view of the whole process by which the Western world developed in the last five or six centuries.

Second, the colonial period of American history, lasting for almost two hundred years, is nearly a void as far as any economic analysis is concerned. This period has been the exclusive province of the historian, and therefore it is not surprising that the treatment of economic issues leaves much to be desired. There have been no studies of the performance of the colonial economy, particularly in the crucial years 1763-75, although historians have drawn broad inferences from scraps of evidence. The relationship between the money supply, price levels, specie flows, and the balance of payments has not been adequately treated. There are no analytical studies of the major industries. What has been written with respect to the implications of the effects of British imperial policy on colonial welfare is completely devoid of any use of incidence theory. Even one of the best articles on the subject—Harper's piece on the effects of the Navigation Acts on the thirteen American colonies[5]—measures the burden upon the colonies of British imperial policy without any reference to the elasticity of demand for the commodities involved. (He implicitly assumes a perfectly inelastic demand.)

Third, the ten years following the end of the Revolutionary War are one of the most interesting periods in our economic history. This was not only a time of important economic readjustment but one in which a new society had to make a set of basic political decisions which would set the fundamental ground rules for the operation of its economy. The problems of tax incidence, government participation in the economy, federal-state economic relationships, and monetary policy were all influenced by the performance of the economy, by immediate economic issues, and by the underlying philosophical bent of the participants during this critical decade. Yet the argument over the economy's performance has usually been settled by a few contemporary quotations and odd statistics.[6] The immediate economic issues

[5] Lawrence A. Harper, "The Effects of the Navigation Acts on the Thirteen Colonies," in R. B. Morris, ed., *The Era of the American Revolution* (Columbia University Press, 1939).

[6] A beginning on a systematic examination of the economy's performance has been made in the recent article by Gordon Bjork, "The Weaning of the American Economy," *J. of Econ. Hist.*, Dec., 1964.

have received scant analytical attention, and the economic interpretation of the era has gone by default to the naïve views of some historians.

Fourth, it is an axiom of a good deal of our economic history that government investment played an important role in accelerating the growth of the economy in the nineteenth century. Yet absolutely no evidence exists that warrants such a conclusion. While the work of Carter Goodrich and his students has made an important contribution in showing that the government did, in fact, intervene in canal investments, railroad investments, etc., neither Goodrich nor his students, nor anyone else, has shown that this actually accelerated the rate of growth of the economy. To make the case, it would be necessary to show that there were significant differences between the private and the social rate of return on investment such that private investors were underinvesting in these activities; that government investments did in fact yield a high social rate of return; that alternatively these same funds invested through private channels (under the conditions of full employment that typically prevailed) would have yielded significantly lower returns; and that the magnitude of this differential was sufficient to have altered significantly the rate of growth of the economy.[7]

Fifth, perhaps nowhere are the deficiencies in American economic history more glaring than in the voluminous writings about the disposition of public land in nineteenth-century America. Did public land policy adversely affect the rate of growth of the economy? Or slow down the pace of the westward movement? Or cause a more unequal distribution of income? While the extensive literature on the subject seldom put the issues so clearly, these are obviously the meanings implicit in a great many of the assertions made in leading articles on the subject.[8]

If it is true that the form of distribution of public lands through the various land acts or the purchases of speculators or the railroad land grants produced the results cited above, none of the evidence advanced on the subject proves the point at all. It may very well be that factors cited above did have some of the results which have been suggested, but if so, the whole question has not been examined in any analytical sense yet. And it will require such research before any such statement can be made.

The illustrations cited above are only a very small ripple of an endlessly dreary stream that would flow from cataloguing the poor quality of research in economic history. However, it is not necessary for me to continue. Anybody can play the game of testing the quality of the literature in the field for himself. Let me suggest to you a simple way of

[7] For a further discussion of the issues involved, see my comment on Stuart Bruchey's article in *Explorations in Entrepreneurial History*, 2d Ser., I, No. 2 (1964), pp. 160-62.

[8] A convenient source is *The Public Lands*, Vernon Carstenson, ed. (Univ. of Wisconsin Press, 1963).

playing this game which I use in my graduate seminar in American economic history. I ask my students to take leading interpretive articles in American economic history and to make explicit models of the articles.[9] Even by plugging into each model the most favorable possible implicit assumptions, most of the resultant models turn out either to be internally inconsistent or to run counter to the most fundamental propositions in economics.

Let me turn now to the new economic history. There has been some valuable work—particularly in the gathering of statistics—to provide us with some solid footing for analysis, and there have been a few first-rate articles and books, yet the results have been generally disappointing. Too much of it has been dull and unimaginative, and there seems to be a widespread conviction that econometric techniques, the computer, and running a few regressions can substitute for theory and imagination. Some of the new economic history written by economists is of distressingly poor quality. Some of it is so imprecise and fuzzy as to make it difficult, if not impossible, to make any model at all. A good deal of it includes partial-equilibrium analysis of problems with broad general-equilibrium or disequilibrium implications. Too much of it shows that the writer clearly had no fundamental understanding of the way by which an economy operates. In particular, a lot of it shows that the role of prices in resource allocations and the implications of price behavior have completely eluded the writer.

The inadequacy of the new economic history is nowhere more evident than in its failure to nail down and to refute the shoddy arguments and propositions that riddle the literature. Let me illustrate my point by discussing at some length an article in the new economic history. In fact, it is one of the classics. I refer to the article by Conrad and Meyer on "The Economics of Slavery in the Ante-Bellum South."[10] The Conrad and Meyer article is frequently taken as the epitome of the way in which the new economic history should be written, indicating the sophisticated use of economic theory and statistics to nail down a long-debated issue of American economic history. It does deserve a special place in the literature as a pioneering piece of work. The article pinpointed and clarified a number of issues which have been muddied in the history of the controversy. In particular, it demonstrated that resources in the South were allocated efficiently and that the market for slaves operated in a fashion that was compatible with a profit maximizing economy. However, the article did not accom-

[9] For an illustration, see my comment on H. J. Habakkuk's essay, "Population Problems and European Economic Development in the Late Eighteenth and Nineteenth Centuries," *A.E.R.*, May, 1963, pp. 639-42.
[10] *J.P.E.*, Apr., 1958, p. 95.

plish its objective, and it has permitted the endless discussion of an issue which long since should have been buried. Conrad and Meyer set out to measure the viability of slavery by testing its profitability. As a result, given the limited and imperfect nature of available data, they have perpetuated an endless controversy around the issue of whether their data (or anyone else's) do indicate that slavery was profitable and therefore viable.[11] In fact, there is no possibility that slavery was economically not viable. As long as there existed both a rent on land and a rent on slaves—that is, the price of slaves was above the real reproduction costs of slaves—any short-run unprofitability of slaves as a result of their price being bid up for noneconomic reasons would simply result in a readjustment, either in land rents or ultimately in slave prices, so that the equilibrium rate would again prevail. Only if the wages of free labor fell to the subsistence level, so that in fact the prices of slaves fell to below their reproduction cost, would the institution become nonviable.[12] And since no such consideration was involved, there never was any real issue about the viability of slavery. While one might well make the argument that slave prices might be maintained at a higher level than would be justified by the rate of return in cotton production because of their use for conspicuous consumption or noneconomic reasons, then the answer simply would be that in that case land rents would fall so as to adjust to an equilibrium solution. In short, the Conrad and Meyer article has perpetuated an issue which is really no issue at all.

In summary, it is my convinction that we need to sweep out the door a good deal of the old economic history, to improve the quality of the new economic history, and it is incumbent upon economists to cast a skeptical eye upon the research produced by their economic history colleagues to see that it lives up to standards which they would expect in other areas of economics.

This criticism of the state of economic history is not based on some utopian notion that we can achieve a scientific consensus on interpreting the past, but rather on the wide gap that exists between contemporary practice and the potential which could vastly enrich our knowledge of the past and reduce the range of uncertainty and disagreement over our economic heritage.

[11] For the two most recent illustrations, see Edward Saraydar, "A Note on the Profitability of Ante-Bellum Slavery," *S. Econ. J.*, Apr., 1964, and Richard Sutch, "The Profitability of Ante-Bellum Slavery Revisited," *S. Econ. J.*, Apr., 1965. It should be noted that Sutch's paper, presented in preliminary form in my graduate seminar in American economic history, makes clear that the issue discussed is short-run profitability but not viability.

[12] See the article by Yasukichi Yasuba, "The Profitability and Viability of Plantation Slavery in the United States," *Econ. Studies Quar.*, Vol. XII, No. 2.

THE REUNIFICATION OF ECONOMIC HISTORY WITH ECONOMIC THEORY

By Robert William Fogel
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In the brief time allotted to me I want to outline two provisional propositions concerning the new economic history.

The first proposition is that one can observe in the work of the new generation of economic historians a departure from the past sufficient to justify a title like new economic history.

The departure is not to be found in the realm of subject matter. The new generation has not turned away from the traditional theme of its discipline. The central interest of the new economic historians is still the description and explanation of economic growth. This continuity with the past holds in the small as well as in the large. The specific research projects of the new generation focus on such familiar issues as the developmental impact of railroads and canals [14] [17] [22] [36], the effect of changes in supply and demand on the growth of the iron industry [40] [12], the profitability of slavery [6] [13] [39] [44], the factors affecting the growth of productivity in agriculture [35], the effect of federal land policy on the distribution of income [3] [16], the influence of foreign trade on the creation of a market economy [33], the sources of the capital required for industrialization [9] [43], the explanation of urbanization [8] [38], and the causes of economic fluctuations [43].

The novel element in the work of the new economic historians is their approach to measurement and theory. Economic history has always had a quantitative orientation. But much of the past work on economic data was limited to the location and the simple classification of the numerical information contained in government and business records. While continuing this pursuit, the new economic history places its primary emphasis on reconstructing measurements which might have existed in the past but are no longer extant, on the recombination of primary data in a manner which enables them to obtain measurements that were never before made, and on finding methods of measuring economic phenomena that cannot be measured directly.

In performing these tasks, the new economic historians draw on virtually the whole gamut of the theoretical and statistical models of economics. William Whitney uses input-output analysis in his attempt to measure the effect of tariffs on the rise of manufacturing during the

post-Civil War period [42]. Eugene Smolensky and D. Ratajczak employ location theory to explain the growth of San Diego [38]. Paul David relies on a constant elasticity of substitution function to infer the growth of the capital stock in Chicago during the nineteenth century [7]. James K. Kindahl applied an extension of the hypergeometric distribution to estimate the number of nonnational banks in existence after the Civil War [29]. The theory of rent has proved to be relevant to the analysis of problems as diverse as the economic viability of slavery [44] and the estimation of the social saving of canals [36]. Even so rarefied a construct as the Von Neuman-Morgenstern utility index proved to be of practical value in quantifying the effect of risk on the financial enervation of the Union Pacific Railroad [19].[1]

The measurements obtained through the application of economic theory and statistics have yielded considerably more precise information than has hitherto been available. For example, Paul MacAvoy, combining regression analysis with a theory of cartel stability, has been able to date the onset and duration of the rate wars among the trunk-line railroads, to identify the initiators of these wars, to measure the intensity of the conflict and to estimate certain of the gains and losses of the participants [32]. Stanley Engerman, applying regression analysis to a body of cross-sectional data on the iron industry that has lain fallow for more than a century, has been able to produce remarkably detailed time series on the growth of the capacity of blast furnaces by state and type for the years from 1800 through 1856 [12].

Such improved information has frequently resulted in dramatic reevaluations of the economic impact of past events and institutions. Thus capital theory has been used by Alfred Conrad and John Meyer, Yasukichi Yasuba, Robert Evans, Jr., and Richard Sutch to show that slavery was a profitable system [6] [44] [13] [39]. Richard Easterlin's construction of estimates of the regional distribution of income combined with Robert Gallman's new series on gross national product imply that between 1840 and 1860 per capita income in the slave South grew at approximately the same rate as the long-term average for the United States as a whole [11] [20].[2] The last finding is in startling contradiction to the traditional view that the antebellum South was economically stagnant. Other traditional views that have been upset or at least seriously challenged by the new economic history

[1] For other statements on the redirection in measurement characteristic of the new economic history and the role which theory plays in this redirection, see [1, Part I] [15] [34].

[2] If slaves are included in southern society, the average annual rate of growth of southern per capita income is 1.3 percent per annum. If slaves are treated as inputs in the production of final products rather than as consumers of final products, the rate of growth of per capita income rises to a little over 1.4 percent per annum (cf. [11, pp. 530, 546]). The average rate of growth of per capita income for the U.S. as a whole between 1834-43 and 1944-53 was 1.5 percent per annum [20].

include the hypothesis that railroads were built ahead of demand [14], the view that the lag of wages behind prices during the Civil War led to unusually great prosperity for northern manufacturing interests [5] [26], and the proposition that the development of the Bessemer process was of transcending significance for the rapid emergence of a modern iron and steel industry in the United States [18] [40].[3]

My second proposition is that the new economic history represents a reunification of economic history with economic theory and thus brings to an end the century-old split between these two branches of economics.

Economic history emerged as a distinct discipline during the course of the mid- and late-nineteenth-century revolt against the deductive theories of classical economics. Led by Roscher, Knies, Hildebrand, and Schmoller in Germany and by Leslie, Ingram, and Ashley in England, the original aim of the historical school (or schools) was to replace what they believed to be the unrealistic theories of deductive economics by theories developed inductively through the study of history. Yet despite a half century of progammatic proclamations and despite the many fine historical monographs produced by the school(s), no alternative theory emerged.

In a 1901 review of the accomplishments of the historical school(s), Thorstein Veblen called their effort to supplant classical theory a "failure." "There seems," he continued, "no reason to regard this failure as less than definitive" [41, p. 71]. Veblen's judgment was reaffirmed three decades later by J. H. Clapham who wrote, "Most scholars are now agreed that such an attempt failed even in the hands of Schmoller" [4, p. 329].

Recognition of this failure led neither to the disappearance of economic history nor to its reunification with economics proper. When Ashley ascended to the first chair of economic history at Harvard shortly before the turn of the century, he called for a truce between the warring factions. He disclaimed any desire to compete with deductive economics in the formulation of rival theories of value and distribution, asking only that economic history "be let alone."[4] Ashley believed that conflict was avoidable because in his view economics proper and economic history focused on different problems: the former on the static properties of modern economies; the latter on the evolution of economic societies or—as we now call it—economic growth [2]. To

[3] Because of the limitation of time and because the topic assigned to me specified the field of American economic history, this brief survey omits important contributions by scholars in the United States and abroad whose field of research is the economic history of other nations.

[4] Cf. with [21, p. 412].

J. N. Keynes's contention that "familiarity with economic theory is essential to the interpretation of industrial phenomena such as it falls within the province of the historian to give" [27, p. 271], Ashley replied: economic theory revealed little about the connections of economic phenomena that could not be understood through the application of "plain common sense" [2, p. 127].

The truce for which Ashley called lasted for more than a half a century. During this time intellectual enmity abated. Within economic history, scholars such as Callender, Heckscher, Cole, Hamilton, and Rostow effectively applied economic theory and statistics to the study of history. Within economics proper, empirically- and historically-oriented analysis developed far more extensively than Ashley foresaw. One of the centers of such work, although not the only one, was the National Bureau of Economic Research where Wesley Mitchell, Simon Kuznets, and others applied theory and statistics in massive empirical studies of the development of the American economy.

Still, as late as 1941, the relationship between economic history and economics proper was essentially one of truce. In that year Edwin F. Gay, on the occasion of his election as the first president of the Economic History Association, both gave recognition to the breach that remained and called for its elimination. "Full cooperation," he said, "is not yet easy or intimate and one of the first tasks of the economic historian today is to open the way to a more complete connection of the two disciplines" [21, p. 412].

In the years following World War II, the movement toward the reunification of economic history with economic theory accelerated. Among the factors that led to the quickened pace, two may be singled out. One was the substantial increase in the range and subtlety of the models encompassed by economic theory. The other was the widespread experimentation, in many fields of economics, with the adaptation of general models to specific (historical) situations—an experimentation stimulated by the upsurge in econometrics and other forms of applied mathematics.[5] With such developments Heckscher's isolated plea of the 1920's for a greater use of theory in the study of economic history [24] became a relative commonplace in the 1950's. Post-World War II texts announced their reliance on the "framework of economic analysis to elucidate the historical narrative" and treated their "emphasis on economic principles" as a mark of distinction [37, p. xi].[6]

However, the reunification of economic history with economic theo-

[5] An experimentation, it might be pointed out, that has increased awareness, not only of the usefulness, but also of the limitations of existing models in the explanation of economic growth.

[6] See also the prefaces to [10] [25] [28].

ry could not have been brought about merely by the interjections of theory in textbooks. Reunification required the utilization of economic theory as an integral tool in the basic research on which the discipline of economic history rests. This condition has been met, as I tried to illustrate in the first part of my comments, by an outpouring of studies published or initiated during the past half-dozen or so years.

The effort to improve the precision of measurement in economic history has been a powerful catalyst in transforming desire into reality. For as Simon Kuznets (whose work, perhaps more than that of any other scholar, inspired the new economic history) has frequently pointed out, there is an intimate connection between economic measurement and economic theory [30] [31]. Hence the emphasis placed on theory in the work of the new economic historians is neither an irrelevant, popular affectation nor a stilted superimposition. Rather it is the logical consequence of their desire to quantify the contribution of various changes in economic institutions, in factor supplies, and in technology to the rate and direction of economic growth.[7]

It is probably too soon to attempt a generalized evaluation of the quality of the output of the new economic history and of its contribution to our knowledge of the economic past. Much of this output is still in the prepublication stage or has only recently been published. And many of the debates it has touched off are in full flush.

Yet I cannot resist making one observation. N. S. B. Gras concluded his 1930 survey of the status of economic history in the United States on a gloomy note. "The universities," he wrote, "have generally neglected the study of economic history, apparently regarding it as a very special subject. There has been a lack of controversy, even of intellectual resilience, in the field" [23, p. 327].

The situation today is quite different. Controversy abounds; and the level of the debate is, in my opinion, quite high. Imaginative applications of theory and statistics have brought to the fore evidence which until recently was considered unobtainable. Moreover, the hiatus in recruitment into the field appears to have come to an end. Meetings of the Economic History Association which just a few years ago were peopled almost exclusively by scholars who received their training before or during World War II are now marked by the attendance of a large corps of young people who entered economic history during the last half-dozen years or so. And the rate of entry seems to be rising. At the same time several leading departments of economics have for the

[7] In this connection mention should be made of the Purdue Seminar on Quantitative Methods in Economic History. Convened for the first time in December, 1960, the Seminar has met annually since that date. It brings together twenty to thirty scholars for three days of intensive discussion on problems encountered in the adaptation of theory and statistics to the requirements of historical analysis.

first time appointed teachers in economic history, while other departments have expanded or are in the process of expanding their appointments in this area. Vibrant is the word that best describes the present atmosphere in economic history.

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HAS THE EARLY HISTORY OF DEVELOPED COUNTRIES ANY CURRENT RELEVANCE?

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To discuss the possible contemporary utility of historical studies is, at best, a hazardous venture. And, at most, it can easily become a tedious tramp through a jungle of stale arguments. However, since an occasional draught from the bottomless cup of methodological platitude is an unavoidable occupational hazard, we ought perhaps to welcome this particular opportunity. And I, at least, am comforted by the fact that an audience of economists is normally less hostile to the topic (although, no doubt, more intolerant of its clichés) than an audience of historians. This difference itself seems to me to reflect a fact directly relevant to the discussion; namely, that economists are accustomed to proceed by generalization, conceptualization, and the explicit construction of theoretical systems embracing and relating a wide variety of experiences; whereas most historians (although there are outstanding exceptions) are traditionally averse to model building, receptive to the idea of the "uniqueness" of events, and hypersensitive to the apparent complexity of particular historical situations.

In these necessarily abbreviated remarks, however, I want to beg some of the more obvious questions—and in particular the problem (or, perhaps, the pseudoproblem) of whether or not history exemplifies significant patterns or illustrates usable "laws."

That history is relevant seems to me to be beyond doubt. As one example, take the most depressing (and improbable) conclusion imaginable: that the economic and social problems of the past bear absolutely no relation to those of the present. Now, even this sobering hypothesis could only be arrived at or tested by a systematic process of conceptualization, research, and comparison; and would therefore involve a positive and quite important step towards understanding the problems of modern preindustrial societies. To say that A is in no significant respect similar to B implies the acquisition of knowledge about B as well as A. Indeed, to contrast opposites may sometimes be more illuminating than to compare similarities.

On the other hand, quite apart from the precise nature of the possible relevance of past to present, any problem in the social sciences can only be approached by the use of conceptual devices and the deployment of models of greater or lesser sophistication. And there is surely

no a priori reason to expect that historical study and observation will be substantially less fruitful than any other ritual for the generation of concepts and hypotheses potentially useful in our approach to current economic problems.

Having, then, like a good historian, spent some time dealing with a question I said I would ignore, what about those questions we have left?

In general, my answer to the main question is that history is, or can be, very relevant to the contemporary economic scene. And I would argue (along with other and better men before me) that this relevance lies less in the provision of timeless examples of socioeconomic processes than in the use of economic history as a source of expectation about such processes. From time to time, no doubt, the direct parallels between past and present will be both clear and significant. Far more often, however, the nature of change will preclude history repeating itself in any useful way. And, as a result, the most we can expect is that our systematic study of the past will yield an approach to rather than a set of conclusions about our own world. That the expectations and analytical framework which we derive from, say, the study of preindustrial Europe could be of considerable use as shaping and guiding influences for research into current economic problems seems to me to be a significant fact. And this remains true (indeed, its significance is, if anything, enhanced) even if these expectations are frustrated or have to be modified in the course of their application. I hope it is not necessary for me to emphasize that the past is not a unique avenue for understanding the present; and certainly history itself is not a substitute for research or ratiocination. But I fail to see why a historical perspective could not be a powerful weapon in the armory of economic research—at least as long as we remember its limitations as well as its potentialities. To decline upon a well-worn and comfortable platitude: the questions may be more significant than the answers. And, as so often happens in the social sciences, the proof of the pudding may be in the beating, not in the eating.

Nevertheless, it may justly be said, all this does not really get us very far. In methodological discussion, as in economic theory, a valid argument is not always the same as a useful one. How far, then, is the sort of history which has been or is being written actually relevant to current problems? To tackle this question, in turn, involves some consideration of the attitudes and activities of historians and economists. For the use of history as a social science demands considerable care in its writing and circumspection in its application.

In these respects we are all, from time to time, miserable sinners. On their part, for example, economists tend to expect too much from his-

tory, and to extend their own necessary emphasis of simplicity and generality into an area which cannot sustain it in precisely the same form. More particularly, they tend to criticize historians for not achieving something that most historians very rarely set out to do. And they are naturally disappointed when, looking for universal hypotheses, they find that economic history does not yield what it cannot produce.

But, since this is ostensibly a discussion of economic history and I am a historian, my task ought to be to confess rather than to accuse. And in this respect it is obvious that economic historians frequently fail to aim for that degree of analytical sophistication which would make for better history—let alone a more fruitful relevance to current problems. As Professor North suggests, one of the most serious weaknesses of much (although by no means all) economic history has been the tendency to use implicit or rank bad theorizing, the implications of which are ignored. That is, historians too often make use of explanatory frameworks in which the analytical relationships are confused or unexplored; which lack explicit conceptual definition; and which oscillate violently between, on the one hand, desperate and unstructured hypotheses to the effect that "life is complicated and everything affects everything else," and, on the other hand, simplistic, unicausal hypotheses which tell us little if anything about dynamic processes. And, insofar as this is true, the output of economic history is simply not of a type or presented in a way which would usefully accommodate it to the study of economic or social problems in the modern world.

But all this, it seems to me, is quite enough breast-beating for one day. I should like now to balance its pessimistic exaggerations by using the power of more positive thinking.

First, I should emphasize an important methodological implication of what I have been saying. If the worth of economic history is severely limited insofar as it eschews explicit "theorizing," and if the relevance of history to current problems in part depends upon the nature of its application, then in fact the distinction between economics and economic history is far less precise than many traditionalists would have us believe. History (that is, worthwhile history) becomes a dialogue between certain types of facts and certain types of conceptual approaches; and this is a broad definition which applies equally to economics, with the proviso that the facts may be different and that the operational usefulness of the concepts may vary. In this sense, economic history as a continuing discourse between the past stream of events and theory becomes merely a variation of economics as a discourse between the present stream of facts and theory.

Second, however, one is bound to ask whether the concrete results of attempts to understand such processes in the past are sufficiently illu-

minating to warrant optimism. And in this respect, in spite of some of my earlier and more gloomy remarks, I think the answer is yes.

Thus, to take a very general example, the more historians examine the experience of the English economy in the eighteenth century, the more they tend to revise traditional beliefs about the origins, preconditions, and nature of the industrial revolution. And this, in turn, has led to a fruitful rephrasing of the approach to such topics as the extent of the discontinuity in economic trends, the role of increased investment, the importance of working as against fixed capital, the significance of apparent "leading sectors," and the interplay between economic progress and demographic change. Obviously it would be ludicrous to argue that the results of this work provide a precise guide to the process of industrialization in the mid-twentieth century. But as long as they are appraised with a reasonable degree of intellectual awareness, there is every reason to expect that they may provide very useful first steps towards an understanding of modern situations and potentialities. And if this is true for a case study of a single country, the attainment of that higher degree of generality implied by comparative studies must surely be even more useful.

That this sort of study is concerned with the growth of presently industrialized societies (which, *ipso facto,* are different from presently backward economies) does not, to my mind, reduce the usefulness of historical work. And this is so for two reasons. First, as an Englishman knows only too well, it is not only preindustrial societies which have economic problems in the 1960's. And the economic history of "advanced" lands is surely not irrelevant to their economic present. Second, even were we to decide, in Professor Gerschenkron's terminology, that discontinuities in the historical scale of economic backwardness mean that we cannot assimilate modern Eastern problems to eighteenth- and nineteenth-century Western problems, it is still likely that our approach to the former can benefit—and benefit considerably—from our conclusions about the latter.

Moreover, it is not only the economic history of industrializing countries which is involved in this sort of argument. The preindustrial history of Western Europe, for example, yields analyses of the role of institutions, markets, population-land ratios, social psychology, and entrepreneurial processes which are surely neither completely nor even largely irrelevant to modern analyses of economic underdevelopment. It may well be that, to obtain a positive and direct relevance to the present-day situation in (say) Africa, we have to go back in time to the European middle ages and the sixteenth century. And, further, we must be aware of the fundamental changes wrought by the availability

of twentieth-century technology and culture. But essentially we are dealing with comparable problems and processes.

In the last resort, however, I would not rest my argument merely on the presumption of historical continuity. Rather, it seems to me that we know too little about economic processes (whether in a backward or a developing context) to ignore any possible source of enlightenment or technique of study. In the last resort and in principle I do not see why the answer to the question posed by this session's title should not be of the same type as the answer to some such question as, "Has modern growth theory any current relevance?" In each case the appropriate reply is: "Of course—maybe!"

IS THERE NEED FOR HISTORICAL RESEARCH ON UNDERDEVELOPMENT?*

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The need for scientific knowledge to guide the formulation of growth policy in underdeveloped nations is obvious. With reference to this aim, the question, has the early history of developed countries any relevance? is but one of a family: Have untested a priori growth models any relevance? Are econometric models estimated from cross-section data of value? What about intensive case studies of individual underdeveloped areas?

If one takes the pages of the journals as indicating professional opinion on these questions, the answer to all of them must be yes—and this paper is superfluous. Unfortunately, to an economist such perusal necessarily raises further questions. Is the allocation of research effort among these alternative lines such as to equalize marginal returns? Granted that any one line of research may yield results of positive value, at what cost is it purchased in terms of opportunities foregone?

Even these questions might be escaped if one assumes that directions of research are in any event determined by hard and fast habits of mind, and therefore that individuals will go on doing what they have always done, despite good arguments to the contrary. There is doubtless much to this, and in the short run, when not only the aggregate size but individual makeup of those comprising the economics profession is essentially fixed, little can be done to alter the nature of its research biases. But replacement via demographic and other processes is an ever present, if somewhat unpleasant, fact, and, in the long run, changes can and do occur. Hence, one may usefully discuss these questions in the hope (perhaps vain) of exercising some slight influence on the interests and training of today's uncommitted graduate students, as well as offering a gleam of hope to those who, on the verge of choosing a field of graduate study, while attracted by the subjects of economic research are repelled by its methods. It is this younger generation who will comprise the core of the research effort two decades hence.

Because of space limitations and, it must be admitted, lack of both competence and inclination, this paper will not try to muster all the pros and cons. Instead it will try to suggest some of the reasons why

* I am grateful to Robert Summers for his characteristically pertinent comments.

there is need for more historical research. These are of two types: limitations of other approaches and positive arguments for historical study—the "push" and "pull" factors, so to speak, operating to shift research toward history.

Push Factors

To dispose first of purely deductive models, such as those of the Joan Robinson or Kaldor type, certainly no one denies the importance and necessity of theory. But the potential number of models is infinite. What is needed as a basis for decisions in underdeveloped countries are models validated by tests against actual experience. To be sure, appeals to experience have always been employed to justify such models, but fortunately both the supply of data and professional standards have advanced to the point where casual empiricism no longer finds wide acceptance.

The question thus becomes one of the range of experience relevant to the formulation and testing of growth models. Current generalizations are chiefly based on two types of evidence.

One is single-point-of-time observations for a number of countries or areas at different income levels, exemplified by work such as that of Chenery and Houthakker. These studies are of considerable value in identifying broad similarities and differences currently existing among countries at different levels of development. That I do not mean this as an empty gesture will be shown at a subsequent point when I cite a representative study for support. However, such studies have inherent shortcomings, of which the authors themselves are doubtless aware. One, attributable no doubt to my particular bias, is that they cannot be used to test hypotheses regarding longer term fluctuations in the growth process such as Kuznets cycles. A second, and fundamental one with regard to relevance to development policy, is that the customary approach involves generalizations from observations ranging from the poorest nations, with measured incomes on the order of $50 per capita in current U.S. prices, to the richest, the U.S., with an income level thirty to forty times as great. (It is well known, of course, that reported incomes exaggerate the "welfare" difference, but it is these reported incomes that are used in cross-section regressions.) Aside from the suspicion that the U.S. as an extreme observation often exercises undue weight in the result, one may ask what span of experience, in years, is implicitly being used to obtain generalizations presumed applicable to development policy, the time horizon of which is typically one, or at most, two decades? This question calls for fuller discussion, but it takes little reflection to conclude that the implicit span of experience is very long. The current level of real per capita income in the United

States, which comprises the extreme upper observation in most of the cross-section studies, was reached after a century or more of growth from an initial level which appears to have been well above that in many underdeveloped countries today. Thus the range of income observations in the typical cross-section study implies a time span greater than this, unless one is willing to assume potential growth rates much higher than those heretofore experienced. In other words, generalizations presumed applicable to development planning over one or two decades are being inferred from data which average together the experience of countries separated by a century or more in time. To circumvent this shortcoming, one might attempt to confine the cross-section study to a range of incomes likely to be more relevant to the experience of a given underdeveloped nation over, let us say, the next two to three decades. However, if this were done, I believe one would typically find that the fit of the regression line based on the much longer span is poor, and that the number of observations available in the shorter span for fitting a new line is very limited.

One naturally turns therefore to time series. Here, one finds a second body of evidence commonly used in the testing of growth models; namely, the experience of individual underdeveloped countries. This is obviously relevant, and such studies are desirable. There is sometimes a tendency to treat a given case as unique, but I am sure most scholars would strongly advocate attempts at comparative study. The only question is: Should one confine oneself to presently underdeveloped countries? What of the early experience of Japan? Of Argentina and Chile? Of Southern and Eastern European nations such as Spain, Italy, Bulgaria, Poland, and Finland, not to mention the experience at still earlier dates of nations in Northern and Western Europe?

In this way we return to the question with which we started: Has the early history of developed nations any relevance to currently underdeveloped areas? When one approaches it in this manner, however, the burden of proof would seem to be on he who would prejudge the question in the negative; on the person who would arbitrarily draw a line and say the experience of all these earlier countries is *sui generis*. At the very least, one can say: it seems premature to exclude from study any potentially relevant experience; there is precious little to go by as it is.

Pull Factors

But are there positive reasons for thinking that earlier experience is relevant? Several come to mind. First, important similarities have been noted, especially by Supple,[1] between characteristics of today's under-

[1] Barry E. Supple, "Economic History and Economic Underdevelopment," *Canadian J. of Econ. and Polit. Science*, Nov., 1961, pp. 460-78.

developed nations and those of currently developed countries in their preindustrial stage—archaic taxes, laws, and land tenure conditions; unstable financial policies and currency management; political and military instability; preference of workers for leisure; of entrepreneurs for investment in inventories rather than fixed capital; unprecedented population growth. How did the now-developed countries overcome these obstacles, so much like those confronting today's underdeveloped nations? One would suppose the answer to this question would be of some interest.

Second, as has been shown particularly by Kuznets' studies,[2] in every nation that has developed, a number of similar trends has appeared: decline of agriculture, rise of white-collar employment, shift to urban areas, growth of larger units of economic and social organization, increased participation by the individual in nonfamily centered activities, and so on. These similarities have occurred despite substantial differences among nations in geographic situations, institutional conditions, and the international environment in their early stages of growth.

Why should this be so? Obviously, much basic research is needed, but at the risk of oversimplification one may suggest that the principal cause which has produced essentially the same effect in country after country has been the adoption of a common new technology as the basis for production. Although this technology has continued to change since the days of Watt's steam engine and although there are differences among developed countries, nevertheless there can be little dispute that the developed countries share in common essentially the same production methods, and that this technology contrasts sharply with that current in underdeveloped areas. The point could be demonstrated by appeal to various technological indicators: use of inanimate power, of chemical fertilizer, iron and steel, and so on. Its plausibility is evident if one considers the relative ease with which workers in Russia or Japan might be transferred to jobs in the United States or Great Britain. (The point refers, of course, only to the adaptation necessary with regard to methods of production, not to those regarding broader social, economic, and political conditions.)

A second factor, less fundamental but nevertheless important, is the apparent existence of essentially similar income elasticities of demand for broad categories of output among populations in different countries. Evidence for this has been furnished by wide-ranging studies in time and space by Houthakker, Kravis, and Kuznets.[3] Because of this

[2] See the series of papers, "Quantitative Aspects of Economic Growth," appearing intermittently in *Economic Development and Cultural Change*, Oct., 1956—Oct., 1964.

[3] H. S. Houthakker, "An International Comparison of Household Expenditure Patterns, Commemorating the Centenary of Engel's Law," *Econometrica*, Oct., 1957, pp. 532-51,

basic similarity in preference structures, rising incomes associated with adoption of the more productive technology have led to pressures for rather similar types of resource reallocation.

Is the technological base, on which today's underdeveloped countries will build their higher levels of living, likely to be so different from that which has evolved in now-developed nations? Are the populations of these countries likely to exhibit strikingly different consumption characteristics as income grows? If the answer to each of these questions is no, as I suspect it is, and if the doubts voiced earlier about the more popular research alternatives are valid, then it would seem desirable to allocate relatively more research effort to the study of historical experience on underdevelopment.

To summarize in more general terms: Over the past two centuries we have been witnessing the gradual spread of a technological revolution among the nations of the world within the framework of a broadly stable structure of human wants. While there are many important differences that deserve explicit recognition (and which, incidentally, historical research may help explain[4]), the impact of this revolution has produced many similar results in country after country, and there is no obvious reason why it should not continue to do so in the future. Although alternative lines of research are certainly of value, there seems reason to believe that relatively more study of the earlier history of now-developed nations may contribute in distinctive fashion to understanding the current experience of less developed countries. From this viewpoint, to the question, "Is the history of developed nations relevant to the current experience of underdeveloped nations?" one may respond, "Is not the current experience of underdeveloped nations a part of history?"

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and "The Influence of Prices and Income on Household Expenditure in Various Countries, *Bulletin de l'Institut International de Statistique,* Tome XXXVII, 2[d] Livraison, pp. 1-16; Irving B. Kravis, "Comparisons of the Structure of Consumption," in Lincoln H. Clark (ed.), *Consumer Behavior* (Harper, 1958), pp. 308-54; Simon Kuznets, "Quantitative Aspects of the Economic Growth of Nations: VII. The Share and Structure of Consumption," *Economic Development and Cultural Change,* Jan., 1962, pp. 1-92.

[4] Alexander Gerschenkron's work shows the potential of historical study, not only for identifying repetitive features of industrialization, but also for systematic explanation of some of the differences. Cf. *Economic Backwardness in Historical Perspective* (Cambridge, Mass.: Belknap Press, 1962).

THE ROLE OF ECONOMIC HISTORY IN THE EDUCATION OF THE ECONOMIST

By ROBERT E. GALLMAN
University of North Carolina

No self-respecting economic historian could answer "no" to the question put to Professor Cameron and me. Therefore, I take it we are intended to give our ideas as to what role economic history should play in the education of the economist—presumably the formal graduate education of the professional economist. I am going to assume that we are commissioned to talk both about the features of economic history which are of value to the economist and the ways in which these features can be presented in a formal course offering. Hopefully, I will avoid straying off into the provinces of the other panel members.

Economic history takes the long view. It is chiefly concerned with long-term economic growth, stagnation, and decay. To avoid awkward phraseology, I will speak of economic growth or development, but I do not intend thereby to limit my conception of the field to cases of growth alone. Into my subsequent remarks should be read appropriate parenthetical qualifications which expand them to cover stagnation and decay.

The graduate student should come to economic history with the expectation of learning something about development and there should be in the curriculum a course in economic history built around this theme. The student specializing in economic history may begin his work with bibliography and historiography and then take work in the economic histories of various nations over specified stretches of time, assembling in this way a cumulative record and analysis of development, as well as some knowledge of the historian's craft, the state of old controversies, and fairly detailed knowledge of various histories. Dipping into such a curriculum will, no doubt, be rewarding for the nonspecialist. But it seems to me that his first, and perhaps only, graduate brush with economic history should be in a course of work directed toward the process of growth, in which as wide a historical record as possible is put to use. (Indeed, I think this is the appropriate beginning place for the specialist, too.) Bibliography, interpretive controversy, aspects of national histories should be selected and organized around this theme.

This proposal seems, at first, dictated by fashion—a course proposal that uses the right words in order to slip by the watchdog faculty com-

mittee charged with the duty of keeping the curriculum lean. Economic development has caught our fancy and, therefore, the term is a safe one. But this, in fact, is not the case. Economic development has never gone out of fashion with economic historians and the essence of the proposal is by no means novel. I am suggesting only that the first graduate course be addressed explicity to growth and that materials for it be sought in a wide range of national histories.

The first task of such a course should be to describe the economic aspects of the process of growth, so far as possible in quantitative terms: levels and long-term rates of change of output, inputs, and productivity; the changing regional, industrial, and product composition of the same aggregates; the distribution of income among size classes, inputs, and final uses; output and input flows among units of economic organization, firms, industries, regions, nations; changes in the size and character of units of economic organization, including markets; movements in the level and structure of prices, etc. The history of growth of any one national unit should be understood as a variant of an experience held in common with all growing nations and some grouping of variants should be attempted.

This catalogue lists some items that are not in stock, at least in the form of measurements generally accepted by the profession, within reasonable margins of error, and others which are not in stock for all variants. It is an ideal catalogue, intended to suggest the kinds and scope of the materials from which this aspect of the course should be constructed. The responsibility for a significant fraction of those available measurements lies with scholars who might not class themselves as economic historians. According to the last *Handbook,* Simon Kuznets does not claim membership in the guild. Economic history is more than what economic historians do.

Economic history is a consumer of theory and one of the determiners of the tasks of theory. *The identification of the features of economic* growth to be described is a theoretical matter; so, in large measure, are the classification, aggregation, and analysis of the data. The economic theory consumed comes principally from outside the field. But the needs for theory are not always met. The recognition of the need for theory sets the tasks of theory. No one can study the growth of the nations of the West for the last hundred years or so without appreciating the importance of the various relations between population change and growth, or the factors determining the volume and pattern of technical change, or the significance for the diffusion of technical change of the levels and, more particularly, the kinds of technical competences of the relevant labor force and capital stock—all matters of abiding interest to economic history but often slighted by economic theory.

The graduate student studying economic history ought, then, to have the opportunity to test and exercise his analytical skills. The course ought to give him some feeling for the empirical content of the concepts, magnitudes, and relationships treated by growth theory, as well as the limits of current growth theory and the tasks lying before theory. Every applied field has similar characteristics. Like courses in other applied fields, the course in economic history affords an opportunity to encourage student research. It may have special advantages for this purpose. No doubt this is simply the view of an insider, but the field seems filled with important questions which can be dealt with in limited compass; for example, small questions of how much, which have significance, and which can be answered by some digging and analytical ingenuity. Furthermore, the field is wide enough to cover a variety of interests. And, of course, everyone who takes a course in economic history expects to have to write a paper—a not inconsiderable advantage in itself.

I have been talking about phenomena which can be easily labeled economic. A final task of the course in economic history is to describe the relations between economic growth and institutional change. The course should see growth in the context of social and political change, broadly conceived. The object should be to distinguish, so far as possible, experiences common, in essence, to the histories of the developing nations, as well as the aspects of these relationships which appear to have had bearing on the speed and character of change.

In summary, economic history has a role to play in the education of the economist. It will play this role best if it speaks explicitly of economic development. To change, and mix well, the metaphor, it provides the student with a field for the exercise of his analytic skills, the empirical content for his models of growth, insight into the tasks of theory, and some conception of the relations between economic and social change.

HAS ECONOMIC HISTORY A ROLE IN AN ECONOMIST'S EDUCATION?

By RONDO CAMERON
University of Wisconsin

The question posed as the topic for these comments is obviously rhetorical. Currently the vast majority of professional economists are trained in graduate schools that require their students to take course work or examinations in economic history. (My own institution, one of the last holdouts, only this year adopted such a requirement.) The answer, therefore, can only be a categorical "yes." A more meaningful formulation of the question would be, what is the role of economic history in an economist's education? Or even, what should be the role of economic history?[1] The answers in these cases are not only more controversial, but, especially the latter, doomed to perpetual discussion, since the question implies a normative judgment.

In analogous discussions concerning the role of theory in historical research the argument is frequently made (perhaps because it is valid) that the historian will inevitably be guided by some a priori ideas. It is desirable, therefore, that these ideas be made explicit and systematized if possible. The choice, in other words, is not between theory and no theory, but explicit, consciously formulated theory and implicit, unconscious theorizing. Much the same can be said for the use of history by theorists. Even the most scornful ahistorical economist makes some use of history: his own experience, the experience of his generation, or the loose historical generalizations which abound in the folklore of even highly sophisticated societies. Historians are not the only users, perhaps not even the most frequent users, of some such phrase as "history proves. . . ."[2] Whether or not history proves anything in any meaningful sense is another question; but there is no doubt that history is used and, when used properly, useful. The role of economic history in the economics curriculum is thus to try to assure that it is used properly.

[1] My colleague J. G. Williamson suggests that another meaningful question is when in a student's educational process formal exposure to economic history would be most fruitful. My colleagues R. H. Day and Jan Kmenta also made helpful comments on an earlier draft of this paper.

[2] An interesting example occurs in an essay by Oscar Morgenstern on the use of mathematics by economists. Morgenstern stresses the point that new mathematical tools will have to be developed to deal with economic phenomena. "If this be the case—*as I believe history will show*—then nothing at all can be said until these developments have occurred. . . ." "Limits to the Uses of Mathematics in Economics," in J. C. Charlesworth, ed., *Mathematics and the Social Sciences* (Philadelphia, 1963), p. 14. Emphasis added.

What constitutes the proper use of economic history? One graduate department of economics, which is well known for both its mathematical and quantitative orientation, not only requires economic history of its students, but is unusually explicit in stating the reasons for the requirement:

First, since we view economic history as the empirical side of economics, the courses provide the student with a basic knowledge of economic institutions and their evolution. Moreover it emphasizes the impact of these institutions on economic processes. Second, since all empirical work is by its very definition economic history, the sequence [of courses] introduces the student to the techniques of empirical testing of economic hypotheses. In particular it introduces the student to the sources of economic data and, in connection with the course in research methodology, the formulation of hypotheses in forms that are subject to test.[3]

The quotation above emphasizes the importance of ascertaining the nature and reliability of empirical data. An econometrician, R. L. Basmann, goes even further in this direction:

. . . in view of the fact that a proclaimed falsification can be discredited, not only on the ground that the methods of statistical inference employed in making the test . . . were inappropriate or misinterpreted, but also on the ground that the supporting historical investigation of initial conditions was inadequate in respect of method and resoluteness, the judgment is warranted. Its corollary is, of course, that the econometrician—if he wishes to test theories—has to cope with the economic historian as peer in respect of criticism and technical assistance.[4]

In my view an even more important function of economic history in the education of economists is that it introduces the student to the full complexity of economic processes. Most graduate students in their advanced research and subsequently in their careers specialize in particular sectors or aspects of the economy; typically, also, they employ predominantly the method of static or comparative-statics partial equilibrium analysis in dealing with short-run problems. The study of economic history forces them to look at whole economies in their geographic, political, social, and cultural setting; to explore the interactions between different sectors of the economy and between the economy and its environment; to observe the variety of responses to economic problems in human history; and most important, it forces them to contemplate the problems and processes of economic change through time. In short, the study of economic history broadens their horizons and stimulates their imaginations.

Such broadening and stimulation can save young economic theorists and policy-makers of all ages from many kinds of error. A common

[3] *The Graduate Program of the Department of Economics, Purdue University*, pp. 5-6. I am indebted to J. R. T. Hughes, not only for bringing this and the following reference to my attention, but for many stimulating discussions of the role and methodology of economic history.

[4] R. L. Basmann, "On Predictive Testing of a Simultaneous Equations Model: The Retail Market for Food in the U.S.," Institute for Quantitative Research in Economics and Management (Krannert Graduate School of Industrial Administration, Purdue Univ.), Institute Paper No. 78, pp. 19-20.

error is to mistake short-run for long-run problems, or vice versa, and to apply inappropriate solutions. The controversy concerning the alleged "dollar shortage" after World War II was of this nature, as is much of the debate over the effects of technological change on employment. Another source of error is to confuse target variables with decision variables, in the terminology of decision theory, and to try to operate directly on the target variables. Experiments with price control and exchange control are familiar examples of this kind of error. Historical experience, by demonstrating the long-run effects of such errors, can powerfully supplement the didactic warning of the theorist.

However relevant and important historical experience may be for problems of short-run policy, by far the most interesting cases intellectually are those which arise from long-run problems, defined as problems of structural change. It is significant that the last twenty years, during which economists have once more become increasingly concerned with problems of economic growth and development, have also witnessed a marked revival of economists' interest in economic history. Since the essence of economic development is fundamental structural change, one might say that the business of policy-makers in underdeveloped countries is to promote, encourage, or otherwise bring about favorable structural change. The characteristic business of the economic historian, on the other hand, is to describe, analyze, and explain structural change. Moreover, any theory of structural change must, in order to command respect, be tested against historical or long-term data. The symbiosis of history, theory, and policy in application to problems of economic development is therefore a natural consequence.

In dealing with short-term problems the economist is entitled—indeed, in most cases he is obliged—to regard the fundamental structure of the economy as unchanging, to lock it up in the box of *ceteris paribus*. To take a familiar textbook example, an aggregate production function of the Cobb-Douglas type views output as a function of the inputs of capital and labor. The more fundamental determinants of output—population, resources, technology, and institutions—can be ignored on the assumption that they will not change significantly during the period under consideration. Obviously, for problems of economic development such an assumption is not only unrealistic but self-defeating, since development consists precisely in changes in the fundamental determinants. Unfortunately, in the present state of our knowledge we are unable to state the form which such a function would take, or even to quantify its principal components. It is nevertheless important for young economists, whether or not they are primarily concerned with problems of economic development, to be aware of the existence of such fundamental determinants and of the impor-

tance of structural features and structural change in the short run as well as the long. It is also an extremely useful pedagogical device to ask students to estimate the shape of the function for specific historical cases or to quantify some of its components.

In stressing the importance of the study of economic history for problems of long-run structural change, I do not wish to give the impression that I believe that economic historians know all the answers. To put it more pointedly, I am arguing the importance of economic history, not praising the accomplishments of economic historians. Economic history should have a role in the education of economists, not only (to paraphrase the founders of Harvard College) to preserve us from the curse of an illiterate profession, but also to insure that economic historians will be better economists.

DISCUSSION

Evsey D. Domar: When invited to participate in this program, I accepted the offer eagerly in order to quarrel with economic historians about their predilection for descriptions and tautologies and their neglect of analytical models. But after Professor North's paper, which will be known to posterity as "The Confessions of Douglass North," I am unable to say anything nasty. I am even ready to volunteer for the defense of economic historians from self-mutilation. And though I am still unhappy about the meaning of such expressions as the leading sector, absolute scarcity of capital, transformation of government bonds into capital, and many others, I will say no more about them during this session.

To come to the question which we are supposed to discuss: "Has the Early History of Developed Countries Any Current Relevance?"—presumably for economic development. Now, would anyone have the nerve to reply, no? With nerves of only average strength, I first said yes, and then spent the better part of the Christmas vacation trying to figure out why. For assistance I turned to Barry Supple's old paper on "Economic History and Economic Underdevelopment" (*Canadian J. of Econ. and Polit. Sci.*, 1961, pp. 460-78) —and since I am supposed to discuss Supple's paper at this session I trust you will not care which one I take—and did find quite a few suggestions. As one reads his list of the plagues that handicapped English economic development in the seventeenth and eighteenth centuries which Professor Easterlin has just cited to you in his paper, and then recalls that England did develop after all, one gains a better understanding of the state of affairs in the underdeveloped countries of our times and a greater hope for their future. I was reminded of the feeling of personal relief which I derived some years ago from reading Karen Horney's *The Neurotic Personality of Our Time* after I discovered what the rest of humanity was like.

But if there are similarities between England of the seventeenth and eighteenth centuries and the underdeveloped countries of our day, there are also some striking differences. In those days, and indeed throughout most of English history, both the technology and the institutions of England were of a piece. Now the underdeveloped countries have the canoe and the airplane, the tribe and the corporation, the witch-doctor and penicillin. But what is perhaps more important for our discussion is the change in the social climate. In the seventeenth and eighteenth (and even in the nineteenth) centuries, there was a greater tolerance of social and economic evils some of which, such as high profits and low wages, concentration of income and wealth, certain monopolies, regressive taxes and the like, were nevertheless conducive to economic development (though many others, like corruption and waste in government, were not). In those days people were much more patient with the slow, almost imperceptible, rate of economic improvement. There is much less tolerance of these evils today, though they still exist, and there is certainly much less patience, if not among the masses, certainly among the intellectuals and the leaders. But with increasing educational levels, this impatience will

spread. The knowledge that better life exists on earth, that great possibilities for improvement are inherent in the technological gap, and the glitter of the Russian advance—all reinforce the urge for speed. No wonder that so many of these countries turn to socialism, in one form or another, and sometimes only in name, with its twin promise of rapid economic development and of social justice. Surely few underdeveloped countries would now assign their governments the relatively minor roles they played in England, and even in France and in Germany at similar stages of economic development.

If then the history of some developed countries has lessons to offer, it is not so much the history of Western Europe which grew in a rather leisurely manner that is relevant here, but the history of the accelerated economic development of Russia (both during 1890-1913 and since 1928) and of Japan. True enough, some of the lessons may be negative. But the knowledge of what not to do is most valuable, not less so than the knowledge of what is to be done. It is indeed hard to acquire the latter without first learning the former.

R. A. Gordon: Like Professors Cameron and Gallman, I am prepared to assert strongly that economic history has a role—and an important role—to play in an economist's education. You may recall Schumpeter's dictum that, of the economist's tools of history, statistics, and theory, economic history "is by far the most important." Almost certainly, most economists would not be prepared to accept this ordering of the standard tool fields of our profession, and the statement is more strongly worded than I should prefer. Nonetheless, this tribute to the importance of economic history is worth remembering.

We all agree that economic history does have a role in the training of economists, and Professors Cameron and Gallman offer some views as to what this role should be. At this point, naturally, differences of opinion arise.

Let me begin by disagreeing with Professor Gallman. It seems to me that he identifies economic history too much with, in his words, "economic growth, stagnation, and decay." He goes further and proposes that the first graduate course in economic history, presumably required of all students (or at least those who are candidates for the Ph.D.), be a course in the quantitative aspects of economic growth. It seems to me that this is too narrow a conception of the role that economic history should have in the training of our graduate students.

Should we not ask of economic history a good deal more than this? The quantitative and comparative study of the economic growth (and decline) of nations should certainly be included. But the new "quantitative economic history" goes much further than this. Economic history is, or should be, concerned with studying, at various times in the past, all the standard economic relationships with which our students will be concerned in their work in economic theory, money and credit, international trade, and the other applied fields. How have these relationships changed over time, and in response to what institutional, demographic, and other forces? I view economic history as the application of the time dimension to all of applied economics. This calls for all the help that we can get from economic theory and from quantitative methods. It also requires the help of other branches of history.

This is not yet the end of what economic history has to contribute to the

education of the economist. The latter needs to know something about th pattern of institutional change over the decades and centuries, the reasons fo such change, and the ways in which such change has affected the economi relationships in which we are most interested.

As you can see, I am asking for the moon. The educational assignment tha I would hand to the economic historians is an impossibly broad one, and n graduate program can afford to give to economic history as large a fraction o the student's time as would be necessary to do even a significant part of wha (in my opinion) is needed.

But let me offer a few partial suggestons. Is it unreasonable to require full year's graduate course of all Ph.D. candidates? Such a course would attempt to do something with each of the different approaches that I have suggested. In addition, a significant amount of historical material should be introduced into the standard courses in the applied fields, if it is not there already. Such historical material can be as analytically oriented as the othe parts of such courses. I should urge also closer collaboration between economic historians and their departmental colleagues in both economic theory an the applied fields.

Reflecting my own interests, also, might I urge on my friends in economi history that they do more than they are now doing to help the rest of u maintain the ever moving bridge between the past and the present. Economi history does not end in 1914, 1929, or even 1959. If I may be trite, today i part of tomorrow's history. It is inevitable that much of recent specialize economic history must be treated in the courses and seminars in the variou applied fields. But we in the applied fields need more help than we are currently getting from the economic historians in evaluating and putting int perspective many developments since World War II—such as the spectacula acceleration in European growth rates, the forces making for greater cyclica stability, the factors underlying the recent and current "creeping inflation" ir prices, the implications of the continued shift in demand toward the servic industries, the changing character and tempo of technological change, and so on and on.

I should like to close with a few words on the relation between economi theory and economic history, which was the subject of Professor Fogel's paper. The feature of the "new economic history" is its use of modern theoretical and statistical tools. But so far as economic theory is concerned, we are still on a one-way street. The movement is from theory to history; there is little if any movement the other way. Perhaps it is too early to expect any reverse movement yet. But I hope that eventually there will be. Perhaps I am merely exhibiting my own prejudices when I express concern about the extent to which various branches of economic theory continue to operate in an institutional vacuum and build on assumptions which, if useful at all in contributing to our understanding of the world around us, frequently have an applicability which is strongly bounded by time and place. It is my hope that the new economic history will not only continue to borrow from economic theory but will also increasingly contribute to the development of economic theory And I hope, too, that our students will increasingly have the opportunity, in their graduate work, to participate in and benefit from such interchange.

THE CONTRIBUTION OF THE HISTORY OF ECONOMIC THOUGHT TO THE UNDERSTANDING OF ECONOMIC THEORY, ECONOMIC HISTORY, AND THE HISTORY OF ECONOMIC POLICY

THE ROLE OF THE HISTORY OF ECONOMIC THOUGHT IN THE UNDERSTANDING OF MODERN ECONOMIC THEORY

By DONALD F. GORDON
University of Washington

My assignment is a discussion of the role of the history of economic thought in the understanding of modern economic theory. It is difficult to get beyond personal impressions and subjective prejudices in such a discussion and mine for the most part will also fail in this respect. However, I think that this audience might be at least as interested in what the profession thinks of the role of the history of thought as in my private predilections. As a consequence I have taken an informal poll of those departments which produce the mass of doctoral degrees in our field. The results, which I will summarize briefly, indicate a decline in the interest in this area. In the rest of my discussion I will attempt a fairly obvious explanation of this decline and will conclude with a brief plea that historians of economic thought reorient their approach and broaden their horizons.

In a report upon graduate education in economics in the United States published in 1953, Howard Bowen related that "from conversations at many institutions I have gained the impression that it has declined in popularity."[1] He went on to suggest that his impression was partly confirmed by the fact that "the number of professors who believe it should be in the core [courses required of all graduate students] is considerably less than the percentage of institutions at which it is required or usually elected."[2] While 76 percent of reporting institutions listed some work in the field as required or usually elected in the Ph.D. program, only 37 percent of responding professors recommended its inclusion in the common core.

This past summer I distributed a questionnaire on this topic to the chairmen of the forty departments of economics in the United States and Canada which produce the largest number of doctoral degrees, ac-

[1] H. Bowen, "*Graduate Education in Economics,*" *A.E.R.*, Sept., 1953, Part 2, p. 115.
[2] *Ibid.*

cording to the latest available figures from the United States Office of Education. The response was greater than 90 percent, for which I thank any of you who may have been partly responsible. The results are broadly similar to Bowen's answers and his inferences therefrom. The history of economic thought appears to be persisting quite tenaciously in the face of declining popularity.

Eighty-six percent of reporting institutions still offer graduate work in the history of economic thought, and in 70 percent of reporting institutions it is required (fifty-three) or elected (seventeen) by more than half the doctoral students. Moreover 83 percent of the institutions that do not require a course nevertheless include some material from the field on the general examinations for the Ph.D.

A different picture emerges in response to questions designed to elicit some indications of trends in opinion and action. About 40 percent indicated a decrease in offerings or requirements over the past five, ten, or fifteen years; about the same number indicated no change; and only 20 percent indicated an increase. I am inclined to think that in some of the latter cases the increases were a consequence of increases in the size of the graduate programs rather than a change of heart concerning their composition. (Presumably no graduate programs have decreased in size over the past few years, so that the reverse would not apply to the first group.)

Even more significant, perhaps, are the indications that there is a broad difference of opinion between younger and older economists. Respondents to the questionnaire were asked to guess the attitudes of the younger, as compared to the older, economists toward replacing persons on present faculties who teach or do research in the history of economic thought. Fully 63 percent of the replies (the respondents presumably being mostly chairmen) indicated that they believe the younger economists would be less inclined than older ones to replace faculty in this area. Only one institution reported the opposite.

Somewhat contrary evidence appears from published work in the field. The following table shows, for the five volumes of the *Index of Economic Journals* sponsored by the Association, the proportion of pages listing papers in this field to pages required to list all papers. It seems fairly clear that there is no trend during the past thirty-five years in these figures. It may be that figures for the past five years would show a decline; yet while Bowen's data were collected during 1951-52, the last half of the decade of the 1950's shows virtual constancy.

While the above evidence may not be conclusive, I think there is a reasonable presumption that the profession, particularly its younger members, has a declining interest in the history of economic thought. I

TABLE 1

Volume	Period	Percent of Pages on the History of Economic Thought
I	1886–1924	5.1
II	1925–39	3.9
III	1940–49	4.4
IV	1950–54	3.9
V	1955–59	3.8

now turn to some speculation on why this is the case, and what, if anything, we should do about it.

Let us first make the distinction between the history of economic analysis or theory (concerned with the record of the formal logic of economic models together with what empirical evidence has been adduced to support them) and the history of economic doctrines (or the history of views of economic policies, together with their necessarily attendant political, social, and philosophic connections). This appears to be an increasingly common distinction, and without denying or affirming any causal connections running either way, I think it logically valid and useful.

Is a knowledge of the history of economic analysis a necessary condition for understanding current conventional economic theory? I think the answer is negative. In the ordinary meanings of words, to understand an economic model and to know its historical origin are logically two different things and empirically are observed apart. Leaving aside the great economic theorists, surely we can observe, on the more mundane level of our colleagues, economists who understand and are perfectly competent in modern economic theory but who are quite innocent of any knowledge of its sources.

We might further ask: Is the history of economic analysis a useful way to learn contemporary economic theory, as Bowen suggested in 1953? Again I suspect, although with somewhat less confidence, the answer is again negative, unless we believe (as I certainly do) that there is some value in history for its own sake. It is true, of course, that most of the great economic theorists have had a lively interest in and knowledge of the analytical work of their predecessors, and some have done scholarly work in the area. Perhaps they have been greater economic theorists for those interests. On the other hand, one might more plausibly argue that while fine analysis and lively intellectual curiosity are frequently correlated, this does not show that the satisfaction of the latter contributed to the former. Suppose we imagine two graduate students of perfectly equal analytic ability who differ only in that Mr. A has had x number of courses in economic theory plus one

in the history of analysis, while Mr. B has had only the x courses in theory. We might agree that Mr. A will have a better understanding of economic theory; but the relevant question is whether this would be the result if Mr. B, in lieu of the course in thought, had had an additional course in economic theory. Both common sense and a decent humility suggest the opposite answer.

Some perspective may be gained on the place of the history of analysis in training professional economists by asking under what conditions, in time or cross-sectionally, is the history of a discipline considered an essential part of professional training in that field. Impressionistic answers are easier to gain cross-sectionally, and it may be useful to rephrase the question: In what contemporary fields will the study of "original sources" be deemed important? In literature and the creative arts, the professional gains his education by exposure to the works of other writers or artists, principally from earlier periods. Textbooks other than collections or reader guides are deemphasized. In many of the social sciences—and perhaps in philosophy—textbooks play a larger role. But in many of these areas parallel readings from original sources and from the "classics" of the field are an essential part of learning the discipline. So far as I can gather, the training of natural scientists relies almost exclusively on textbooks until, in his third or fourth year of graduate work, the student begins his own research.

Economics lies somewhere between the extremes but surely closer to the natural sciences than to the humanities, and perhaps further in that direction than any other social science. Consider the subjects, physical theory, economic theory, and political theory. The first two are largely learned from textbooks; the latter is virtually defined as the history of the subject.

We do not have to be so arrogant as to insist that we, like the natural scientists, have seen the basic truth and that we do not need to know from whence it came. It is true, however, that to an extent that I do not think economists fully comprehend, we have in economic theory a degree of consensus around a basic model, which makes us closer to the natural sciences than to some other fields. This can be put in perspective by a comparison of the history of economics with the history of the natural sciences.

According to a recent interpretation of the history of science by T. S. Kuhn,[8] a "normal science" emerges historically when a "universally recognized scientific achievement" provides "model problems and solutions to a community of practitioners" (p. x). These achievements or paradigms, as Kuhn calls them, attract "an enduring group of adherents" away from random fact gathering and competing models.

[8] T. S. Kuhn, *The Structure of Scientific Revolutions* (Univ. of Chicago Press, 1962).

At the beginning a paradigm is "largely a promise of success discoverable in selected and still incomplete examples" (pp. 23-24) and an "object for further articulation and specification under new and more stringent conditions" (p. 23). Thus from basic models "spring particular coherent traditions of scientific research" (p. 10) which develop the promise inherent in the basic model "by extending the knowledge of these facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself" (p. 24).

Research, which consists in the further development of such basic models, and which defines scientific communities, has both strengths and weaknesses. It has a criterion for choosing problems "that . . . can be assumed to have solutions" (p. 37) and for gathering data which are relevant; for these reasons, normal research can be highly productive, but it is essentially "mopping up operations" (p. 24). On the other hand, normal research does not "test" the basic model, the soundness of which is taken for granted. Normal research "seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies" (p. 24). Phenomena "that will not fit are often not seen at all" (*ibid.*) or at least are put aside for future problem solving. "Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others" (*ibid.*).

Occasionally, revolutions occur. In the further development of a basic model, empirical anomalies appear, the resolution of which can normally be postponed. But if enough such anomalies occur, or if they are particularly critical, a crisis emerges which may shatter the tradition:

> Confronted with anomaly or with crisis, scientists take a different attitude toward existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research (p. 90).
>
> Scientific revolutions are inaugurated by a growing sense . . . that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way (p. 91).

They are:

> . . . those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one (p. 91).

Smith's postulate of the maximizing individual in a relatively free market and the successful application of this postulate to a wide variety of specific questions is our basic paradigm. It created a "coherent scientific tradition" (most notably including Marx) and its persistence can be seen by skimming the most current periodicals. Presumably the addition of the principle of variable factor proportions, or the notion

of the consumer with relatively stable transitive preferences, is "further articulation." Its "specification under new . . . conditions" which constitutes "normal research" can be indefinitely illustrated by analyses of monopoly and competition, tariffs and free trade, money and government deficits, excise and income taxes, unions and minimum-wage legislation—the list is long and well known.

Kuhn's analysis allows for subparadigms within a more basic model, and within economics some of our subparadigms have been overthrown. The replacement of classical wages fund doctrine by the generalized marginal productivity theory would presumably be an example. The establishment of utility theory on the one hand and its overthrow (despite the retention of the term) by Pareto and his followers may be others. On the whole, I think the extent to which we have had revolutions in the history of analysis may easily be exaggerated by the failure to distinguish analysis from policy. Just where the dividing line lies between a major further extension of the basic model and the revolutionary overthrow of a submodel may involve an arbitrary element, and this arbitrariness may have its source in a certain ambiguity in Kuhn's conception of the paradigm.[4] But economics has never had a major revolution; its basic maximizing model has never been replaced. Whether this is good or bad (and the fact is the despair of some), it is, I think, remarkable when compared to the physical sciences that an economist's fundamental way of viewing the world has remained unchanged since the eighteenth century. Since economic theory has obvious connections with economic policies over which economists' passions are so easily aroused and considering that these policies have been so vigorously debated by economists past and present, it is a tribute to the supremacy of purely positivistic intellectual forces that such has been the case.

On the other hand, lacking basic revolutions, we have had major, if unsuccessful, rebellions. Certainly the historicists of the nineteenth and the institutionalists of the early twentieth centuries were a (not so) "narrow subdivision of the scientific community" who had a "sense . . . that an existing paradigm" had "ceased to function adequately in the exploration of . . . nature." As in science, "competing articulations . . . explicit discontent, and the recourse to philosophy and to debate over fundamentals" were all symptoms of a crisis. But the profession has never permitted the dissent to get out of hand. The potential revolutions have been suppressed—some would say because of the stupidity and obstinacy if not the venality of economists. As in the sciences,

[4] Cf. Dudley Sharpere, "The Structure of Scientific Revolutions," *Philosophical Rev.*, July, 1964, pp. 383-94.

the dissenters have to some extent almost been read out of the profession. Little wonder that some have become embittered.

Some perspective can be obtained on the depth of our present consensus by comparing a current dispute—say between two theorists' views about what variables should be included in a demand function for money—with a hypothetical one during the crisis of the 1920's between a Pigou and a Veblen. In a sense the latter two would have had nothing of significance to say to one another, beyond disputing philosophical fundamentals. For the acceptance of Veblen's thoroughgoing alternative paradigm would have meant the abolition of economics as it was known, as alchemy was abolished by chemistry; or the indefinite establishment of two separate disciplines, as astrology was separated from astronomy. Again the consensus is illustrated by the fact that economic history (so often in the past a source of subversive movements and at best maintaining a cold war status relative to theory) shows signs in the "new" economic history of coming into the fold. Needless to say, this is no guarantee that a major revolution will never occur. While I think the consensus may be broader and deeper than it has ever been, it is not universal, and some, like Herbert Simon, call for a major reorientation of economics along psychological lines—as did Veblen.

It might be argued that the consensus of which I speak follows tautologically from the definitions of "economists" and "economic theory" which I am using. In a trivial sense this is, of course, true; but it is also an empirical proposition. It is a prediction concerning the kinds of teaching and research done by the faculties of departments (so named) in what we consider major institutions of learning. Such definitions, therefore, are not so much personal and arbitrary, but those of the recognized profession.

If it is not necessary to an understanding of modern economic theory, why then do we study the history of economic analysis? The question reminds me of Schumpeter's explanation of why true militarists such as the Saracens fought aggressive wars. If asked, according to Schumpeter, the Saracen might spontaneously reply, "The Word of the Prophet"; but if he had a philosophical bent, he might after a moment's reflection reply, "Because I am a man." To ask why academic institutions study historical origins is to question a major part of academic activity; and among those historical origins is the history of economics. We do not need to justify that history simply as a method of teaching economic theory, even if some of that teaching is an inevitable by-product. The better justification may be in the fact that, to vary the dictum of an older scholar, man is a scholarly animal. If we

even raise the question we are half lost; like Mallory and his mountain, we study history because it is there. It is ironic that, while the history of science is burgeoning as an academic study, historians of economic thought appear to have doubts concerning their justification and to be seeking it in the wrong place.

I conclude that economic theory is very much like a normal science and that, like a normal science, it finds no necessity for including its history as a part of professional training. But I know no reason to suppose that the study should or will disappear. Why then does it appear to be declining? I conjecture that this is a decline from what might be called an "abnormal" level. During the "crisis" which was apparent in American economics as late as the 1920's, competing models flourished. As in other fields where there is no consensus, it is natural for people to turn to the classics of the field. Edgeworth held that "for the mastery of a speculative and controversial science a certain multiplication of authorities is desirable . . . [to] counteract the false tendency of teachers to inculcate, and pupils to learn by rote. Hence the history of theory is particularly instructive in political economy as in philosophy."[5] With the reestablishment of a consensus, with the decline in controversy, it was only to be expected that historical interest would decline to what might be called a normal level for a normal science. I repeat that we have no reason to apologize for scholarly interests, nor do I expect that such interests will disappear.

In closing let me make two pleas against what are apparently current tendencies. If because of our insecurity we look upon the history of economic analysis largely as a method of teaching economic theory, we are very likely to view that history as an accumulation of present truth, as was the tendency in traditional histories of science. It is certainly desirable that we have as accurate a record as possible of the sources of modern theory, but exclusive concentration upon this has some serious drawbacks. Historical distortions are created when, for example, Quesnay's significance is viewed as the discoverer of input-output; when Ricardo's value theory is viewed as an explanation of relative prices; or when Viner's theory of the firm is identified as Marshall's. It also neglects some fascinating intellectual puzzles. In the history of science, scholars have recently tended to look upon past scientific concepts, not so much as "false," but rather as impressive scientific accomplishments for their time, which were accepted for good reasons, although they were later rejected for good reasons. More serious efforts to see logical coherence in past theoretical concepts—productive and unproductive labor, Mill's propositions on capital, or the wages fund—might lead us to find some logical validity even

[5] As quoted by M. Blaug, *Economic Theory in Retrospect* (Irwin, 1962), p. viii.

if the concepts were rejected for good empirical reasons. It would be surprising if past scientific theories were good science while rejected economic theories were just nonsense. No doubt some, maybe many, of them were nonsense; and excessive sympathetic relativism has led to some atrocious economics. A "shortage of money" has been used to justify mercantilism and England's "monopoly" of manufacturing is held to explain the rise of the theory of comparative advantage and its alleged subsequent irrelevance. But the proper rejection of nonsense may lead us to reject more serious consideration of neglected concepts.

Finally, I question the recent emphasis upon the history of analysis at the expense of the history of doctrines. Not only is the scholarly study of economic doctrines a legitimate historical inquiry, but it has, if we follow Edgeworth, what might even be called a practical value. Whatever we intend, we train in fact not only positive economists but makers and teachers of economic policy. On economic policy we have no consensus; on the contrary we have profound differences. They arise, not so much from theoretical differences as partly from fundamental values, and perhaps more importantly from differences in views on the nature of governments and the possibilities of government action. We all survey the same history of government policies, but some of us come out "empiricists" and some come out "optimists." Since we have no consensus, we cannot define and therefore cannot teach "sound" economic policy as we can define and teach "sound" economic theory—regardless of what may be suggested by the emphasis upon high school courses in economics. But if Edgeworth was right concerning the teaching of controversial sciences, exposure is desirable to a variety of thoughts on economic policy together with their inevitable relationships to broader intellectual currents. If economists are to talk glibly about government policy—and there is no doubt that they will—it would be some comfort to know that they had been at least exposed to the wide variety of what we might call basic paradigms concerning the nature, role, and possibilities of the state. It might be reassuring if facile teachers and makers of policy were aware, not only of Cournot's second order conditions or Pareto's argument for nonadditive utility functions; not only, further, of Malthus' case against private charity or Walras' views on the nationalization of land; at least as important might be some inkling of the political philosophies of Burke and Paine, or, for that matter, of Plato and Polybius. Driven, perhaps, by a misplaced desire to be scientifically respectable, historians of economic thought appear to be tending the other way.

ON THE HISTORY OF THOUGHT AND POLICY

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What we would like to know about the history of economic policy is what was said, done, and why—what it was that governments were urged to do and refrain from doing, what they actually did, what the problems were that policy was meant to manage and how well it did, how much the decisions were influenced by positive economics, by ethical values, vested interests, and how much was done for no good reason at all.

It is not fair to look for all of this information in the history of economic thought, but we may fairly look for an explanation of the positive economics and for some indication of the ethical values. Reporting the factual side of policy is the work of history, but when economists have undertaken it we may ask that they be accurate.

I

This is what we may fairly look for. What do we find? I want to answer the question with reference to British thought and policy.

First, although economists have written much about policy, they hardly ever have put their ideas in the form of a reasonably complete theory; i.e., a statement of the values that direct policy, their origin and order of importance, the economic means of realizing them, and a defense of why the values and means are appropriate. The first classical economist to make such a statement was Mill, and he was the last great member of the school. He said those who previously had contested over laissez faire and intervention had "seldom declared, or apparently decided in their own minds, how far they would carry either principle."[1]

Second, recent economists have taken a more comprehensive view of policy. A few, like F. H. Knight, have tried to get to its normative foundations. Others, like Tinbergen, have taken the ends of policy as given and tried to put them together in an analytical framework. There also is the application of welfare economics to the normative side of policy.[2] And there are the many studies of specific measures,

[1] John Stuart Mill, *Principles of Political Economy* (7th ed.), Book v, Chap. xi.

[2] Frank H. Knight, *The Ethics of Competition* (London, 1936); *The Economic Order and Religion*, with Thornton W. Merriam (New York, 1944), Pt. I; and *Freedom and Reform* (New York, 1947). J. Tinbergen, *Economic Policy: Principles and Design* (Amsterdam, 1956). E.g., William J. Baumol, *Welfare Economics and the Theory of the State* (Cambridge, 1952).

especially their positive features, such as those made of fiscal and monetary measures. I do not mean that more economists are interested in policy now than in the past but that those who are address themselves to it in a more thorough way.

Third, we find that economists are poor historians—at times wrong about facts, incomplete, unclear, oblivious to what is significant, or stressing what is trivial.

II

There is not time here to prove these observations. Some of the evidence is in what I have written and some in a forthcoming book on economic liberalism.[3] But I can illustrate them by describing the kinds of things economists have written about policy (i.e., the classes of statements they have made).

One kind addresses itself to a particular problem of policy but ends with a contribution to positive economics. Malthus in writing about the Corn Laws developed the idea of diminishing returns. So did Edward West. Ricardo in writing about them made interesting use of price elasticity.[4] This writing tells us about the history of theory. It does not tell us much about policy.

A kind that does is Smith's defense of the Navigation Acts or Hume's saying that as a British subject he prayed for commerce to flourish in Europe.[5] Both men placed a high value on the national interest, and Smith placed it before the national wealth. But neither was entirely clear. Smith did not explain how much wealth was to be sacrificed to power and did not make clear how power was related to the other ends he assigned to policy. Neither did Hume.

Another kind of writing seems to imply a principle of policy while in fact it does not. Senior wrote a pamphlet against the ten-hour day bill of 1837 and he was understood to be against all such regulation. More than anyone, he is responsible for the mistaken belief that classical policy opposed the Factory Acts. Actually he supported the acts of 1833 and 1841. The economists' writing about poor relief seems to denote an opposition to it in principle. But in fact they differed among themselves on particular measures, some opposing intervention and others supporting it. They did, to be sure, declare they were opposed to inter-

[3] "Adam Smith and the Economic Man," *J.P.E.*, LVI, 4; "On the Politics of the Classical Economists," *Q.J.E.*, LXII, 5; "The Liberal Elements in English Mercantilism," *ibid.*, LXVI, 4; *The Manchester School of Economics* (Stanford, 1960), and *Economic Liberalism* (to be published by Random House, spring, 1965).

[4] Thomas Malthus, *An Inquiry into the Nature and Progress of Rent, etc.* (London, 1815). Edward West, *Essay on the Application of Capital to Land, etc.* (London, 1815). David Ricardo, *On Protection to Agriculture* (4th ed.; London, 1822), pp. 18-19.

[5] Adam Smith, *The Wealth of Nations* (5th ed.), Book iv, Chap. ii. David Hume, "Of the Jealousy of Trade," *Writings on Economics*, ed. Eugene Rotwein (Edinburgh, 1955), p. 82.

vention in principle. But they really were not clear, as M'Culloch in his literal way revealed. "The laws," said he, "which regulate the prosperity and decay of nations are as certain as those which govern the celestial bodies; but more interesting, inasmuch as man may modify them by his interference." M'Culloch was not the greatest mind of nineteenth-century economics. Marshall may have been and he was even more confusing. "So I cry," he said, "Laissez-faire; let the State be up and doing."[6]

There is a perverse streak in the view the world has taken of the classical economists. Senior wrote the New Poor Law of 1834, and Carlyle called them the dismal scientists. In the same decade they opposed the repeal of the Corn Laws and disdained the Manchester School. Yet they did become known as opponents of immediate and universal free trade, as in fact most of them were. Some, like Ricardo, wanted a permanent duty on corn.[7] This does not mean he was opposed in principle to free trade but simply that he did not make a complete statement about it.

Another kind of writing describes the principles that have ruled a nation at a particular time. Cairnes said that in his day Britain was directed by laissez faire. He was mistaken, because there was a significant amount of intervention. Smith reported, also inaccurately, that the mercantilists believed money was wealth, and John Stuart Mill repeated the mistake. Both illustrate another failing of economists when they deal with history. Both were unaware of some of the important economic writing that preceded them. Smith seems not to have noticed Turgot's theory of diminishing returns. Mill had read little in the twenty years preceding the writing of his *Principles* and took small notice of the free-trade movement of his time. Smith seems not to have been aware of the technology that was emerging in his day. It did not become clear until the end of the century, but even then so careful an observer as Frederick Eden did not notice important parts of it.[8]

There is a kind of writing that opposes a principle of policy in a way that suggests the writer is opposing the policy of his time. Smith de-

[6] N. W. Senior, *Letters on the Factory Act, As It Affects Cotton Manufacture* (London, 1837). See also Lloyd R. Sorenson, "Some Classical Economists, Laissez Faire, and the Factory Acts," *J. of Econ. Hist.*, XIII (1952), 3. M'Culloch's inaugural lecture as professor of political economy, University of London, reported in *The* [London] *Morning Chronicle*, Jan., 1829. Alfred Marshall, "Economic Chivalry," quoted in D. H. MacGregor, *Economic Thought and Policy* (London, 1949), p. 69.

[7] Ricardo, *op. cit.* Three years earlier he proposed a temporary duty to the House of Commons. *The Works and Correspondence of David Ricardo*, ed. Sraffa and Dobb (Cambridge, 1951), V, 34.

[8] J. E. Cairnes, "Political Economy and Laissez Faire," *Essays in Political Economy. Theoretical and Applied* (London, 1873), pp. 248–49. Smith, *op. cit.*, Book iv, Chap. vi. Mill, *op. cit.*, "Preliminary Remarks." Michael St. John Packe, *The Life of John Stuart Mill* (New York, 1954), p. 310. Grampp, *The Manchester School of Economics*, p. 35. R. Koebner, "Adam Smith and the Industrial Revolution," *Econ. Hist. Rev.*, Second Series, XI (1959), 3, p. 382.

plored the regulation of trade in a way that has made many believe he was arguing against his age. If he was, he must have had the Elizabethan Statute of Apprentices in mind. When enacted it provided for extensive regulation, but after 1660 it was administered in a way that weakened both the principle and the practice of regulation; and by 1700 the courts refused to enforce it. It was not repealed until 1814. Its history illustrates the distinction between enactment, administration, and enforcement.[9]

The writing that is best remembered consists of declarations made in favor of grand principles and made in a way that could imply (but does not) that they were meant for all people, all places, and all times, like the army of unalterable law before which Lucifer sank. The famous example is the invisible hand. Actually, Smith used the words in a passage that is ambiguous. It argues that businessmen do not have to be forced to keep their capital at home but will do so voluntarily, "upon equal or nearly equal profits," because it is more secure at home than abroad.[10] Does this mean the invisible hand will reduce foreign trade? Or that it will send capital abroad when the return is substantially higher there? Or simply that capital will go where the return, adjusted for uncertainty, is highest? The last is an informative statement, but need not imply faith in a natural order.

III

These, then, are samples of the kinds of things economists have written about policy. Despite its shortcomings, the writing does tell us some of the things we want to know. It is almost the only source of information about the positive aspects of policy and also tells us enough about the normative side to make a careful reading worthwhile. We should not take a statement of principle—no matter how doctrinaire, how forcefully expressed, how often repeated—as a summary statement of an economist's views. Mill said "every departure from [laissez faire], unless required by some great good, is a certain evil." But he found many great goods. Still, we may not infer an entire policy from the exceptions, as some commentators have done.[11] What an economist has written about a particular measure probably is best understood to apply only to that measure. When M'Culloch opposed government construction of the Caledonian canal he probably was not op-

[9] Two authorities have believed Smith was arguing against the practice of regulation in his day. J. M. Clark, "Adam Smith and the Currents of History," in *Adam Smith, 1776-1926* (Chicago, 1928), p. 66; and Jacob Viner, "Adam Smith and Laissez Faire," *ibid.*, p. 154. That there was not much regulation is stated by Conyers Read, "Mercantilism," in *The Constitution Reconsidered*, ed. Read (New York, 1938), p. 72; by R. H. Tawney in the introduction to his edition of *A Discourse Upon Usury* by Thomas Wilson (London, 1925), pp. 12-13; Donald O. Wagner, "Coke and the Rise of Economic Liberalism," *Econ. Hist. Rev.*, VI (1935-36), pp. 36-37, 44; and E. Lipson, *The Age of Mercantilism*, Vol. II of *The Economic History of England* (6th ed.; London, 1956), cxxxv-vi.

[10] Smith, *op. cit.*, Book iv, Chap. ii.

[11] C. J. Ratzlaff does in *The Theory of Free Competition* (Philadelphia, 1936), pp. [illegible]

posing public works in general. When the Ricardians opposed a particular corn law, they were not declaring themselves for free trade in corn. The report an economist has made about the mistaken policies of other people is usually an accurate reflection, in the sense of a mirror image, of his own views. The report itself may be completely inaccurate. When Cairnes denounced the Victorians for believing in laissez faire, we may infer that he did not believe in it but not that they did.

What the economists have written needs to be supplemented by other information. The work of economic historians is especially helpful. It has reported what measures were adopted and some that were urged, the problems they were meant to solve, and their effectiveness. It has called our attention to the difference between enactment, administration, and enforcement and warned us to take precautions in using information. Simple as they are, we still need to be warned of them. The historians I have found most helpful are those who attend more to events than to their meaning, more to particular facts than to explaining them, those who are more like Lipson and less like Heckscher. That is because I have found their interpretations are not always reliable. The failing is noticeable when the historians have related measures, such as child labor laws, to what they took to be the policy of the economists of the time, such as the supposed laissez faire of the Victorians. Even Lipson said the factory laws were a repudiation of classical economics.

In order to understand the history of policy, I am suggesting that we read the economists prudently, that we consult all they have written including their letters and other personal papers, that we notice what their contemporaries said about them, and that this information be verified and enlarged by a prudent reading of economic history. At times we will want to go farther—to political philosophy, general history, literature, and, if we are studying a particular measure, to the primary sources like government documents and newspapers.

The way this information is put together is important. We should try to discover the intention of the writer and keep it before us. That is like framing a hypothesis about him, although I do not care to use the word for such an imprecise activity. With an assumption about him, we proceed to sort out our information. If the assumption fits the bits and pieces together, makes them sensible, and directs us to additional information which when found also fits together—if all of this happens, the assumption probably is correct. But if it does not accommodate the information, or makes us interpret it in an eccentric way, or makes the ideas of the writer appear disjointed or inconsistent, then the assumption probably is wrong and should be put aside for another. Of course, the writer may in fact have been inconsistent, forgetful, or careless. But that assumption is best used when all others fail.

There is nothing in these suggestions that is singular, and I feel about them as Lewis Namier felt about a topic in history—that there would be little to say about it if so much that was wrong had not already been said. If we were to reexamine economic policy in some such way as I am suggesting, our view of it would be changed.

We would change our view of some major topics: for example, the ideas and practices of mercantilism, the place of the market in the doctrine of Hume, Smith, and the eighteenth-century classicists, the foreign trade policy of the Ricardians, the ideas and the motives of the Manchester School, and the theory and practice of policy in nineteenth-century Britain. I name these because they are topics with which I am familiar.

We would be more helpful to others who want to understand policy; e.g., historians, political scientists, sociologists. They now present to their readers the ancient, the revered, and, usually, the mistaken conceptions. One of the respected books of our day is McNeill's *Rise of the West,* and it states Smith believed self-interest automatically maximized welfare. Norbert Wiener's *Cybernetics* is a seminal book of our time and it derides the "simple-minded theory" that self-seeking in a free market works for the good of all and that "free competition is itself a homeostatic [self-regulating] process." One of the most influential books about liberalism is Dicey's *Law and Public Opinion,* and it is terribly confused about Bentham, the classicists, the Manchester School, and laissez faire. Henry Adams called John Stuart Mill "his Satanic free-trade majesty" for opposing protection which in principle Mill did not do.[12]

If we knew we were the prisoners of these misconceptions, we would change our view of the present also. Welfare economics, for example, has pointed to a number of departures from optimality in a perfect market, but despite what is so often said they do not necessarily discredit classical policy but may some of them be a rediscovery, in rigorous form, of what a classicist observed.[13]

Our misconceptions about policy are not to be explained by a lack of information. There has been a sensible article on laissez faire in Palgrave's *Dictionary of Political Economy* for fifty years. Forty years ago Jacob Viner explained the reservations Smith had about the idea of a natural order. Lionel Robbins, about ten years ago, lectured on the ideas of policy in classical economics.[14] The information is

[12] William H. McNeill, *Rise of the West* (Chicago, 1963), p. 687. Norbert Wiener, *Cybernetics* (New York, 1948), p. 185. A. V. Dicey, *Lectures on the Relation between Law and Public Opinion in England during the Nineteenth Century* (London, 1940), Lecture VI. Henry Adams, *The Education of Henry Adams* (New York, 1931), p. 72.

[13] Baumol, however, is aware of the historical antecedents. *Op. cit.*, p. 143.

[14] Viner, *op. cit.* Lionel Robbins, *The Theory of Economic Policy in English Classical Political Economy* (London, 1953).

available in some of the texts on the history of economic thought, to say nothing of the original writing. The mystery is why it is not used. Perhaps there is a will to believe—a myth in the Bergsonian sense. If Adam Smith really did not believe in a natural order, he should have. The mercantilists should have believed that money was wealth, Cobden should have believed in universal free enterprise, the Ricardians should have had no reservations about free trade, the nineteenth century should have been the golden age of laissez faire. But none of this is true, really. As Bozzle said, facts is facts, and facts get known.

Once aware of the Bergsonian myths, we would notice the ironies that are in policy debates today. Consider a proposal to fix a specific price. Those who today call themselves classic liberals probably would oppose the measure, and their opponents would say the twentieth century must not be governed by the ideology of the eighteenth. Now what the classicists believed about price fixing in principle is clear—they were against it. But they made exceptions and were not explicit about what should govern them. If the classicists could be consulted, they might favor price fixing in a particular instance. So that today what is advocated as classic liberalism may be anticlassical while what is opposed to it may actually be classical.

How much of today's debate is confused by historical misconceptions I do not know. The appeals to precedent may be only ritualistic. But if they are going to be made at all, let us be accurate about them. As everyone knows, a debate over policy can be improved by someone interjecting, "Let's get the theory straight!" Why not get the history straight, too?

In getting it straight we would uncover some curious information about the temperament economists have displayed toward policy. They have not had as much interest in it as in positive economics, and the interest they have had has been about particular problems more than about systems or theory. They have, moreover, been inclined to wait for problems to present themselves instead of anticipating them. Smith wrote about rent, but it did not become important until the Napoleonic Wars intensified the redistribution of income in favor of landlords. Malthus may have detected something in 1803,[15] but the idea of dimminishing returns did not appear until 1815, the year of Waterloo. Or consider more specific problems: poor relief, factory labor, utility rates, the gold stock. The economists usually did not notice them until the problems became fairly serious and some they did not notice at all. Or consider that the first comprehensive statement of classical policy was made in 1848, which was about forty years after the British gov-

[15] T. R. Malthus, *An Essay on the Principles of Population* (2nd ed.; London, 1803), p. 7.

ernment had begun to intervene in the economy in a modern way. The relationship is even more vivid in this century. The depression of the 1930's brought macroeconomics and policies for full employment. The postwar inflation elicited extensive work on price stability. The growth of trade-unions in the 1930's brought labor economics. The decline of colonial power since 1945 has been accompanied by the economics of development. The anxiety about the Soviet Union has created the field of Soviet studies. What next? The Communist countries now have a lively interest in the price mechanism, as "a homeostatic process" indeed. Will we return to it one of these days?

The response to problems usually has been skeptical, especially the problems of the poor. We do not care to be called "the dismal scientists"; I much prefer Balfour's description of economics as "the chilling skepticism." But its outlook usually has been pessimistic. Malthus was wrong about the capacity for population growth in the West. Ricardo believed that if the state provided homes where poor children were treated with "humanity and tenderness, there then would be no check to that increase which is so apt to take place among the labouring classes."[16] Smith was wrong in thinking the public never would accept free trade in corn. He and others told the workers their unions would either be ineffective or harmful. Senior and his colleagues were grossly wrong about poor relief, and not all of their mistakes may have been honest.[17] The hand loom weavers were turned away by a House of Commons that, as Cobden said sarcastically, had been reading political economy.[18] He might have added the House at about the same time helped insolvent textile firms by guaranteeing loans on their inventories.

I do not mean the classical economists believed the world could not be improved or that they were callous about the common people. Most in fact had humane and generous feelings. But they were too skeptical. At times they acted as if they meant to prove they really were the apologists for the bourgeoisie. Knowing this helps us to understand the attitude of others toward us. "To hell with economics—let's build a better world," it was said recently. I am disgusted by such ignorance and alarmed. But I am not surprised. We have not made it altogether clear to the world what we think about its prospects, and what we have said has not always been accurate. That is shown by the place of economics in the history of policy.

[16] Speech before the House of Commons, March 25, 1819, *Parliamentary Debates*, Vol. 39, pp. 1158-59.

[17] Mark Blaug, "The Poor Law Report Reexamined," *J. of Econ. Hist.*, XXIV (1963), 2, p. 243.

[18] Richard Cobden, *Speeches on Questions of Public Policy*, ed. John Bright and J. E. T. Rogers (London, 1870), I, 23.

THE RELATION OF THE HISTORY OF ECONOMIC THOUGHT TO ECONOMIC HISTORY

By Frank W. Fetter
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This topic involves one of the hardiest perennials in the whole field of the social sciences: the importance of the impact of ideas upon events, as compared with the impact of events upon ideas. It is a subject to which there is no answer of the neat, clean-cut type that the mathematician and the natural scientist find in their own fields, and that they sometimes think those who deal with human behavior should also find in their fields. Great names have been associated with both approaches. Lionel Robbins, at the beginning of his lecture, "On the Relations between Politics and Economics," first quotes Cournot on the limitations of ideas in changing events; and then from the closing paragraph of Keynes's *General Theory* on the causal importance of ideas: "Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back."

The function of the economist approaching this topic is not primarily to tell his audience on which side of the argument they should line up, but in the spirit of the recent election, to suggest to party regulars and to independents the circumstances under which ticket splitting may be in order. An economist's conception of the relation of the history of economic thought to economic history is of necessity influenced by his judgment about the important forces in economic thought. The more closely one associates economic thought with technical analysis whose validity is timeless, and largely independent of the behavior of individuals, the greater is one likely to consider the effect of economic thought on history, and the less the effect of history on thought. The effect of economic history upon the theoretical conclusions reached by practitioners of geometry, differential calculus, and matrix algebra, or even by nonmathematicians who deal solely in propositions of logic that, at least ostensibly, are divorced from human behavior and political and social institutions, is at best limited. Of course particular problems, whether of interpreting history or of trying to chart the course of history, may stimulate analysis of this type. But once such theory is developed, those who have developed it are likely to consider it good for all time. This is economic thought in the tradition of Walras. But when economic thought, instead of being timeless, attempts to explain economic relations in a setting of human institutions, motivations, or

prejudices, that thought reflects its historical setting. The question whether such thought has relevance to other ages or to other countries then becomes a question to whose solution mathematics has relatively little to offer but to which sociology, psychology, history, and political science may make a rich contribution. This is economic thought in the tradition of Adam Smith.

These different approaches to economic thought almost inevitably affect the evaluation by the economist of today of the greatness of an economist of an earlier age. This situation appears to explain why to Schumpeter, Walras ranks so high; why Adam Smith barely gets a passing mark in Schumpeter's grade book. If one considers Walrasian analysis the prototype of economic thought, a close relation between such thought and economic history is likely to be tenuous. I doubt that different conclusions by Cournot, Gossen, or Walras would have altered appreciably the outcome of the controversy over international bimetallism or the Irish land question, hastened or prevented acceptance of the Bagehot principle, or made the countries of the Western world decide differently on public versus private ownership of railroads. On the other hand, different technical analysis might well have affected the course of economic history where decisions by administrative officials were important: say tax legislation, monopoly policy, railroad rates, or the relation between a central bank's open market operations and its discount rate.

If we take a more relativist view of the history of economic thought, then to understand the dominant economic thought of a period—to interpret the significance of Adam Smith, Ricardo, Friedrich List, Keynes—we have to set that thought in the economic history of the time. I find it hard to place the birth of Ricardian and Malthusian analysis in the United States of the 1830's; or to envisage the rise of Keynesian economics in the United States between 1896 and 1919, or in Germany in the years of hyperinflation.

It is a common practice of those whose interests run to the history of economic thought to talk about "neglected" economists. A notable example was Professor Seligman's two articles in the *Economic Journal* over sixty years ago: "Some Neglected British Economists." It is equally a practice for those who have little taste for the study of the past to suggest that it would have been just as well had they remained "neglected." I shall bypass this issue with the comment that neglected economists are of two main types. There were those who produced original analysis that influenced the thought and controversy of their day, but who have been largely forgotten. Some were important in that they provided the ideas that others developed; some were important in that they were leaders in a controversy which with the passing of time

came to be associated with a single man. In this sense Francis Horner, John Wheatley, and to a lesser degree Robert Torrens and Henry Thornton are "neglected economists" of the era of Ricardo and Malthus. Their contemporaries did not neglect them—they were stout fellows in the intellectual debates of that time, but the luster of the names of Ricardo and Malthus led later generations to neglect them. In the same spirit economists of today, who are packing the larger meeting rooms of this Association, and whose ideas are the center of controversy in graduate courses from Cambridge to Palo Alto, may be neglected by the economists of 2100.

There is another type of "neglected" economist: the man whose ideas, no matter how original and brilliant judged by the standards of today, had little if any impact on the thought of his own day. Not time but his contemporaries neglected him. Cournot and Gossen had little influence in their own day. The most extreme case of such neglect would be where no one but the man himself saw his writings, but a century later, when the manuscript came to light in a dust-covered trunk in an old attic, it was found to have anticipated many of the theoretical discoveries of the last two decades. In the field of genetics the story of Mendel's work, for all practical purposes unknown for thirty years, approximates this model of "neglect."

The student of economic thought should be wary, criticizing the analysis of an earlier generation, lest he simply indicate his own lack of familiarity with the course of economic history. I have in mind two specific and related situations: the discussion of Say's Law by economists of the last thirty-five years; and the picture that Keynes gave in an oft-quoted passage in the *General Theory* of the obtuseness of economists to the problems of unemployment in the century between Malthus and Keynes: "The great puzzle of Effective Demand with which Malthus had wrestled vanished from economic literature. . . . The completeness of the Ricardian victory is something of a curiosity and a mystery" (p. 32).

I am not here to defend Say's Law, but I would suggest that much current discussion of Say's Law misses the point that its principal support came from men who, by the standards of today, were forward-looking on social welfare and political democracy. Except for a short period after the Napoleonic Wars, unemployment was not, in the eyes of people who were to the left of center by the standards of the times, the great economic problem of the nineteenth century. Certainly there was unemployment, and at times it was serious. But the rising industrialism, the growth of financial institutions, and the economic and political privileges of the existing order during most of the century presented problems that seemed more important.

Against the background of the economic history of England from the early 1820's until 1914—and particularly until the 1880's—I find no such curiosity or mystery as Keynes felt that he had found for the virtual disappearance of the theory of effective demand from English economics. But it would have been a curiosity and a mystery had English economists, no matter how welfare-minded or how brilliant their intellectual equipment, given much thought to problems of aggregate demand and full employment when so many other problems had a higher priority on the agenda of challenging ideas and of social and economic betterment: adequate wages when employed, repeal of corn laws, regulating working conditions of women and children, abolition of tithes, controlling the abuses of banking, increasing educational opportunities, abolishing the sinecures of a government of the aristocracy, widening the political franchise. I find it hard to believe that the English handloom weaver barely able to survive on the earnings of a twelve-hour day, the English woman on hands and knees toiling in the mines, the child chimney sweep, or the domestic servant on a seven-day week, felt that the major shortcoming of economists was their failure to grapple with the problem of unemployment. In the early weeks of the beginning course in economics we tell our students that economics deals with the choices involved in making effective use of scarce resources, and that human time and energy are scarce resources. To have expected economists between 1820 and 1914 to have given a high priority to full employment is to deny our own teachings and to miss entirely the economic and political setting of the times.

In the same vein, the development of banking theory in the United States for some fifteen years before 1913 reflected concern with the need for short-run flexibility in the monetary supply, and particularly in currency, to take care of seasonal movements and panics. In the light of the banking problems that the country had faced and the banking practices of the time, the idea of tying Federal Reserve advances to short dated paper was not as foolish as it appears when we see what such a commercial loan theory of the proper quantity of money did to inhibit the Federal Reserve System from dealing more effectively with the tragic years from 1929 to 1933.

My remarks so far have stressed the impact of history upon thought, and the need to interpret the economic thought of the past in the light both of the problems of the time and of the institutional setting in which economic life was carried on. But once an economic theory is developed, it has a tendency to take on a universality of time and place as its originators and intellectual custodians acquire a vested interest in it. A theory that seems to have been good for one generation in Great Britain has an entrenched position in the next generation, as

against newer and more appropriate theory. The same may be true of transfer in space, as when the French economist, J. B. Courcelle Seneuiel, came to Chile as economic adviser in the 1850's and gave Chilean economic theory for several decades a laissez faire tone that exceeded that prevailing in England and the Continent. However, in the present era of almost pathological nationalism and anticolonialism in many smaller countries the reverse may be true: a theory developed in a more industrialized country of higher incomes, no matter how relevant it may be to other situations, is likely to face conditioned opposition on the ground that, at best, it does not apply to local conditions; at worst, that it is an imperialistic snare. There is no neat formula to give the right judgment in such cases, but those who are trying to record and evaluate the economic thought of the past and those who are trying to record and interpret economic history must be alert to the possibility that the causal sequence may be in either or in both directions.

Closely allied to the impact of economic thought upon economic history is a different concept: the use that the economic historian of today should make of economic theory—old or new—in interpreting the past. There are two aspects of this problem. The first is to explain legislation or other decisions of economic importance: the English Bank Act of 1844, the Federal Reserve Act, the Hawley-Smoot Tariff, or the Employment Act of 1946. To do this it is important to grasp the economic thought of the time insofar as it was a causal influence on policy. In the case of the Bank Act of 1844 the body of doctrine, commonly known as the currency principle, was the result partly of economic developments from 1825 on: bank runs by noteholders and the feeling that it was particularly important to put the Bank of England in a position to withstand such runs; and partly of the rising tide of free-trade philosophy that made an increasing proportion of the articulate public feel that, on general grounds, the less discretion the Bank of England had, the better. And I would suggest that an important reason, rarely mentioned by historians, economic or political, for the refusal of Parliament to repeal the Bank Act after the crisis of 1847 was the failure in that crisis of the Governor of the Bank of England and two other directors of the Bank. Regardless of the weakness of the theory of the Bank Act, a move to restore greater discretion over monetary and banking affairs to a group whose own members had not been able to manage their own finances faced a heavy emotional handicap. To shift to the American scene a century later: we cannot explain the wording of the Employment Act of 1946, that if read literally was a mandate for inflation, no matter how great, if that inflation would reduce unemployment by one person, except as against the background of a body of theory, born of the Great Depression, that proclaimed that

with the coming of peace massive unemployment was just around the corner.

Those economic historians who in the language of the trade are now known as "new economic historians" are likely to make clear that economic history is not a chronicle of legislation, of administrative decision, or even of the building of textile factories, railroads, and steel mills. Such details, so I sense from listening in on the spokesmen for "new economic history," can be left to the political historians, or to the dying race of "old economic historians." A primary task of the new economic history, as I gather it, is to measure and to interpret measurements. To do that it is important, not to know what men thought about such relations in 1750, 1810, 1876, or 1929, but what modern economic analysis tells us about such relations. If David Hume conceived of international adjustment primarily in terms of price changes brought about by exports and imports of specie, that does not mean that income changes, structural changes, or cost-push inflation may not be useful, even essential, concepts in explaining balance-of-payments adjustment in the age of Hume.

The views of Hume, Smith, Ricardo, Torrens, Bagehot, Marshall, or Keynes may be essential to explaining why policies were adapted that affected the course of economic history; but the assumption that their theory explains events that followed is a quite different matter. Hume's failure to give weight to income changes; Marshall's assumption that, for all practical purposes, savings would be automatically transmuted into investment; and Keynes's failure to develop a permanent income hypothesis or to recognize adequately the role of expenditures on consumer durables, do not mean that relationships envisaged by these concepts were not at work, even though nobody at the time knew that they were. To draw an analogy from medicine: contemporary medical belief that a disastrous epidemic was caused or cured by particular conditions may be very important in explaining its history; but if today we want to have the full story of that epidemic, it is necessary to draw upon ideas unknown to men when the disease was raging.

All this is tricky business: to know on what points a knowledge of history of economic throught is essential in explaining events; on what points the relevant economic thought is that of today. But writing history of any sort, if well done, is tricky business. The increasing sophistication of economic theory on the relationship of economic variables and the miracles of modern computers have opened new horizons to economic historians. But they also create pitfalls in the journey toward those horizons. Sharper tools, driven at higher speeds, increase greatly the versatility and the output of the worker in wood and metal, but they increase the chances that the workman may damage both himself

and his material. And so with the economic historian, and the computers and new concepts at his command. No matter how brilliant the analysis or how extensive or accurate the calculations, the conclusions of the economic historian can be misleading if not related to the economic throught of the period of his study. An anecdote told by Adlai Stevenson at the recent dedication of Northwestern's new lake front campus is relevant. The enthusiastic young representative of one of our larger manufacturers of data processing machines, when asked whether the machines were always accurate, replied with conviction: "Yes, right or wrong, they are always accurate." I like to consider myself a fellow traveler of the new economic historians. But I hope that they will not excommunicate—or purge—me for clinging to ancient fallacies if I say that I would rather be right and inaccurate than wrong and accurate. Data processing machines draped in the mantle of Clio are an aid to thought; but not a substitute for thought, nor for a knowledge of the economic thought of earlier generations.

DISCUSSION

WILLIAM R. ALLEN: Some of my best friends take me as a friend only in spite of my interest in dead economists. It is better in the eyes of these colleagues to be even a traditionalist in labor or public finance than to have an active interest in the evolution of economic theory. They look upon the historical literature of our discipline as consisting of quaint and curious volumes of better forgotten lore, to ponder which makes them weak and weary. They feel, it seems, that the only proper person to be a theory historian—if any is proper—is one who is not bright enough to be a theory innovator or theory applicator.

And yet, this denigration of historicism—which Professor Gordon finds to be increasingly common—is not quite universal. Only three years ago the elected leader of this Association devoted his presidential address to ruminations on the history of economic theory and the men who made it—after, to be sure, letting us know that he chose the topic only because his audience could hardly hope to comprehend the much weightier matters teeming in his brain. Other distinguished scholars have contributed greatly to the study of the history of theory without apology: one thinks immediately of such as Knight, Robbins, Schumpeter, Spengler, Stigler, Viner—all of whom (except the distinguished Englishman) have been presidents of the Association. The suspicion emerges full grown that a subject so attractive to so many of so much competence and achievement can scarcely be all bad.

But just what is there about it which is good? Professor Gordon has investigated the hypothesis that study of the history of theory contributes to comprehension (and, we may hope, utilization) of contemporary theory. And he concludes that while such historical study is unlikely to do absolute harm and may typically do some gross good, the gain probably will be less than the alternative cost of foregone immediate study of today's theory. One is reminded of the common schoolboy argument that the study of Latin is not the optimal way to learn the uses of English—and I have considerable sympathy with both the schoolboy and Professor Gordon.

Schumpeter, in his *History of Economic Analysis,* concedes that "there is much to be said" for the contention that "current work . . . will preserve whatever is still useful of the work of preceding generations." But on balance he rejects the claim. He was, it seems to me, justified in both the concession and the final rejection. The basis of his rejection is that, since scientific progress does not come in "straight-line fashion," the "present state of the science" can be "meaningful," "fully grasped," and "satisfactorily conveyed" only by explicit consideration of its historical conditioning.

My own ground for eschewing sole reliance on "current work," or high-level textbooks, if you please, is rather different from such esoteric historicism. Schumpeter himself indicates the more straightforward reason; namely, how can we put implicit faith in the textbook writers? Who are to be the authors of these (momentarily) definitive textbooks? I shall believe that they

have been appointed by the Deity only when they appear to me from out of the mountain mists carrying their opera writ on tablets of stone. Until then, is no one to check up on these seers to make sure that no pearls of any sort from the past are lost? Even those in charge of summarizing, improving, and teaching the best (and only the best) thought will occasionally find useful the contemplation of some things written prior to, say, last Thursday. How far back are we allowed to go before entering the realm of "history"? Is it really never nothing more than sheer antiquarianism—to be both pitied and scorned—to read the writings of Keynes himself? And Fisher? And Wicksell? And Smith? Can we be justified in feeling sure that the latest exegeses have efficiently incorporated all, both suggestive and substantive, that these giants offered? Is it a sin (if so, hopefully only venial rather than mortal) for me to go to the originals and see for myself?

And suppose that I consistently find in my trips to the classics that contemporary saviors have succeeded, so far as I can judge, in saving all the pearls and nothing but the pearls. Was my own investigation a waste of time? One gain from such historical excursions by the "privates" is that they may exert a bit of pressure to keep the "generals" honest. Furthermore, if I enjoyed the exercise and the experience of communing for a time with deceased friends, my living friends—all honorable men and staunch advocates of consumer sovereignty—are in poor position to fault me. And it may even be more than just wholesome fun, adequate though that excuse is. I may learn from it; indeed, I may learn more that way than through any alternative use of my time and energy. What I learn may not be additional tricks of analysis, formal gimmicks, and esoteric procedures—current literature keeps us well supplied in those respects—but rather the gain may consist in greater humility and in a more adequate orientation in the role of the economic scholar.

It is my impression that among the more "scientific" members of our group, including those most explicitly, deliberately, and avowedly dedicated to formal logic and positive analysis, many are strikingly doctrinaire, inflexible, and ungenerous to alternative interests, emphases, and techniques. I should be foolish to put my entire reliance upon such to tell me the "present state of the science" and how to advance it. There is no guarantee that the study of intellectual history will itself generate a more sympathetic and less arrogant mode of professional and personal behavior—good manners must come more from the early home than from graduate school—but I suspect that interest in such history is highly correlated with more reasonable and genteel and less supercilious and cynical behavior.

The potential humanizing and civilizing gains from study of intellectual history and the gains in breadth of conception and rejection of simplistic conceptions of the universe (however amenable are such conceptions to computerizing) seem so great that any purely formal, technical gains constitute gravy or, to alter the menu, frosting on the cake. But upon occasion and in some areas, the gravy or the frosting can be quite substantial. Perhaps the outstanding case is the theory of international trade: for the trade specialist not to be well acquainted with Ohlin, Graham, and Marshall is not only un-

civilized but is to run a very considerable risk of missing useful materials for his own work.

There are, indeed, issues to continue to explore concerning the best manner to teach and study the history of economic theory. There is the matter of relative emphasis between history of "thought" and "analysis." There is also the matter of when the study should be undertaken: should it be early in one's academic career as a means to learning theory or later in order to use theory to round out appreciation of the history? Or should the more conspicuous goal in studying the history of theory be to learn more of economic history than to learn more of economic theory? Professors Fetter and Grampp have convincingly argued today that the studies of economic history and of the history of economic theory are each incomplete without the other. And that, alone, is sufficient reason for at least some economists to be literate in our accumulated literature.

WARREN J. SAMUELS: Professor Grampp's paper nicely surveys the problems of assessing the influence of ideas on the development of policy. It is a suggestive contribution to research technique in the history of thought as well as the companion field of policy analysis.

I think it particularly important to emphasize Grampp's differentiation between "the kinds of things economists have written about policy." Differences of this kind in the meaningfulness of statements are of great practical concern in evaluating these statements as evidence in the process of interpretation. The misconceptions of which Grampp later writes are at least in part a product of earlier failures to discriminate and interpret correctly. But the drawing of distinctions is a help and not a solution to the basic problem of interpretation. Thus while it is important to recognize that neither a forceful doctrinaire statement of principles nor a list of exceptions should be taken as "an entire policy," the correctness of such or any particular discrimination or interpretation, and the misconception of some other, is often precisely the issue in question. The historian of thought will still have to exercise judgment, but this discretion should be used carefully—and it is in this respect that Grampp's differentiation is important.

It should also be pointed out that while economists have hardly if ever presented a systematic theory of economic policy, still it is true that formal economic analysis, considered as an approach to policy, has been the main contribution of the profession. The significance of this is apparent in Frank Knight's policy schema, but is not limited thereto.

Grampp also seems to suggest—and correctly so in my opinion—that the classical economists were much more sophisticated than the caricature and mythology that has passed for their theory of policy has implied or allowed, Viner and Robbins notwithstanding. I have reference here to the usual oversimplified interpretation of harmony of interests and laissez faire, which may be good ideology but is poor analysis. I completely concur in his skepticism about advocacy which derives false luster from classical liberalism, though I know that what people believe is scarcely less important than what would be

accurate for people to believe. Certainly the cosmology of Western economic policy is greatly indebted to the classical economists however misinterpreted are much of the specifics.

But as important and suggestive as the basic thrust of his paper is, I would go beyond what Professor Grampp has undertaken. The contribution of the history of economic thought to the history of economic policy can involve more than analysis of the place of economic thought in the development of economic policy, which is recognizedly important. The relation, it is suggested, should be broad enough to include the study that the historian of thought can devote to problems of policy analysis. In a manner somewhat parallel to the field of comparative economic systems, the history of thought is in part the history of policy systems, actual or proposed, each of which is a framework within which policy decisions would be made and/or analyzed. The comparativist and the historian of thought, each using his own particular materials, are in splendid positions to identify and develop the fundamentals of a general model of economic policy. Similarly the policy analyst can reinterpret and illuminate the theory of economic policy as one strand in the history of economic thought, utilizing the insights of such a general model. In the field of law and jurisprudence, for example, policy analysis has been facilitated by work in comparative law and legal history.

What I am suggesting is that one major contribution that the history of economic thought can make to the history of economic policy, both considered as analytic disciplines, is the construction of a general model of economic policy or at least the identification of the fundamental issues or problems of such a model. My own research, in the areas of policy analysis and the history of thought, have reinforced each other in a way suggestive of the outline of such a model. It appears that any theory of economic policy ultimately has to do with the structure of the economic decision-making process as a whole; i.e., with the distribution of decision-making participation; its central problem is how the economy is to be organized and controlled; and it is thus a theory of the structure and operation of a particular and/or general total economic decision-making process, including both legal and nonlegal forces of control and private participation. It is involved with the problem of order, i.e., the continuing resolution of the dual basic social problems of freedom and control, and continuity and change, and necessarily comprises elements of both a theory of social control and a theory of social change. If this is the case, a theory of policy also encompasses a theory of knowledge in relation to social policy, in which I would specifically include a theory of both the determination and role of knowledge in respect to social policy and consideration of both the deliberative and nondeliberative forces.

The classical economists, for example, did come to grips with the fundamentals of economic policy as identified above. Their sophistication on these matters is a basis on which the historian of thought and policy analyst can explore the fundamentals of a general model of economic policy. My own research, part of which on the classical theory of nonlegal social control appears in the *Southern Economic Journal* for July and October, 1964, confirms these

speculations. Important contributions have already been made by Professor Grampp, Joseph Spengler, Lord Robbins, Karl de Schweinitz, O. H. Taylor, Nathan Rosenberg, and others who have contributed to the more sophisticated treatment of the history of economic thought in this matter and in others. If we were to reexamine the history of thought on economic policy along the lines that Professor Grampp and I have suggested, although we direct our attention to different problems, our view on many things would surely be changed.

One of the central strands in the history of economic thought is the theory of economic policy. Certainly one viable contribution that the historian of thought can make is the delineation of its fundamentals and through this the reinterpretation and more complete understanding of historic policy systems such as the classical theory of economic policy in terms of these fundamentals. Through such efforts the historian of thought would be not only a more sophisticated chronicler of the past but also an important participant in the formulation of policy analysis itself.

WARREN C. SCOVILLE: For the first time in my professional life I have been asked to discuss a paper with which I am in complete agreement. Attempting to fulfill the discussant's oath of hypercriticism, I have read Professor Fetter's paper at least ten times in the hope of finding some idea, some sentence, or even some phrase with which I might take exception or which I might wish to recast in a slightly different form; but I must confess that this search has been fruitless. At each of his major points I found myself secretly wishing that it had been I who had phrased the idea so felicitously. It is my hope that you will not consider the few general remarks I shall now make about the history of economic doctrines and economic history a superfluous commentary or appendage to his brief but excellent paper.

I have never been able to comprehend economic determinists—those who have espoused theories of determinism as the only true and relevant theories which help man understand and interpret history correctly. From Marx to those of the present day, economic determinists appear to be the most avid social and economic reformers and are frequently most intolerant whenever others exhibit some skepticism or reluctance to accept their views. I have never been able to understand how a determinist—no matter how anxious he may be to cast history or economics into an impersonally "scientific" mold—can purposefully attempt to reform what he believes to be inevitably determined nor how he can reach the conclusion either from observing behavior patterns in others or from introspective contemplation that man is a completely deterministic being. That such a conclusion can have any real meaning or that a believer in this doctrine can at the same time be a social reformer baffles me. It appears self-evident that it is only because man is not completely determined by his physiological-biological traits and by his external environment that he can come to know himself, and that he can observe common behavior patterns and from these construct useful tools of analysis for the social sciences. If man could forecast future economic or social events

with absolute certainty, his "forecasts" would have neither meaning nor usefulness. It is only because he is reduced to gauging the future in terms of probability rather than certainty that any useful social science exists at all.

Once we recognize that man has something to do with his own destiny, it follows that human thought—and, particularly for our purposes, the evolution of economic theory—can influence and has influenced human history and especially economic history. We probably will never be able to measure the relative importance of ideas as a causal force in history, but what is essential in my opinion is that we cease to believe (or better still, that we cease acting as though we believe) ideas do not help shape events. On the other hand, everyone will agree, I suppose, that economic history has conditioned and influenced (but not completely determined) the development of economic thought.

Neither economic thought nor economic history is inherently the independent or the dependent variable; they mutually interact, as do all forces in this world. In particular cases and for specific purposes we may be justified in treating one as the independent variable and the other as the dependent, in much the same way economists use marginal analysis at times to sharpen their focus on the causal relationship between what are really mutually interdependent variables. Since neither economic thought nor economic history as a unity can be accurately quantified and meaningfully subjected to computer techniques or to less sophisticated statistical analyses, "new" economic historians and theorists may have little interest in probing whether economic thought or economic history has affected the other. The new techniques in economic history have already yielded some substantial improvements in our understanding of the past, and their fruits in the future will doubtless be even more rewarding. I am delighted that the bright young minds in our profession are becoming increasingly interested in testing models and hypotheses with current and historical data. But I think that it will be unfortunate if we economists limit our concern only to problems or to time periods which offer quantifiable data, many of which, by the way, may be inaccurate. Some of the most vital relationships in economic history and some of the most meaningful problems today may have to be probed speculatively and may never lend themselves to some statistically testable hypothesis or model. Economic historians and those interested in the history of ideas, in my opinion, should never forget that wisdom and intuitive judgment may often be the only useful guides in understanding the esoteric.

The great majority of economists who have shaped the development of economic inquiry were vitally concerned with the problems of their day: the mercantilists with the role of government in the economic life of developing states and with the expansion of foreign trade after the voyages of discovery; Jean Bodin with the inflation which accompanied the influx of American treasure into Europe; Boisguillebert and Vauban with agricultural and fiscal reform of France's *ancien régime;* John Law with the widespread underemployment and stagnation which plagued the French economy at the end of Louis XIV's long reign; and the physiocrats with the freeing of trade and manufacturing from the crippling legacies of the post-Colbertian system and with land

and agricultural reforms. To understand Adam Smith's interest in economic liberalism one must know the England and Europe of his day; Ricardo's emphasis upon distribution reflected the growing conflict of interest between the agricultural classes and the expanding new industrial and wage-earning classes; and Malthus was deeply impressed by England's expanding population and the disequilibria which accompanied the occupational and geographic shifts in her population which the land enclosures and the rise of industrial towns necessitated. The Historical School reflected the rise of the German state and its belated industrialization. Much more recently, as Professor Grampp has already suggested, the depression of the 1930's yielded Keynes's *General Theory* and stampeded the profession into macroeconomics, full employment policies, and controversies as to whether the rate of interest could approach and remain at zero. "Stagnation economics" then suddenly shifted to "wartime," "inflation," and "catastrophe" economics, with renewed interest in comprehensive planning, price control, and theories of economic growth. Perhaps we shall soon give special attention to "poverty" economics. Surely these widespread shifts in interest, which at times come perilously close to being faddist, will be incomprehensible to economists in A.D. 2100 if the trends are not studied against the backdrop of what will then be the economic history of the mid-twentieth century.

The effect economic doctrine has had upon economic history is much more elusive than the effect history has had upon economic thought. The impact of Adam Smith's ideas upon the economy and government policy in England was delayed by more than a half century; and the free-trade economists in this country since the first World War have not been the ones who have played a major role in formulating our foreign trade policies. On the other hand, few will deny that Keynesian economics has had considerable impact upon government policy and upon economic development since the 1930's or that John Law had a similar but briefer influence upon France's recovery after 1717. Both British and American economists during the nineteenth and twentieth centuries have participated in formulating government policies, in drafting legislation, and in some cases helping to administer the law. Perhaps only determinists will deny that we and our predecessors have had some effect upon economic development, but whether the effect has been altogether salutory is much too controversial an issue to raise at this time.

THE EVOLVING INTERNATIONAL MONETARY MECHANISM

THE REPORT OF THE GROUP OF TEN

By J. DEWEY DAANE
Board of Governors of the Federal Reserve System

The general topic for this panel, "The Evolving International Monetary Mechanism," itself suggests what is to me the essential achievement of the *Report of the Group of Ten.* For the report represents a further contribution to the orderly evolution of the international monetary mechanism rather than to a revolutionary and drastic break with the past. It constitutes both an approving look backward by financial officials at how the system has evolved and a questing look forward at how it can and may evolve in the future—stressing both new and old elements which may serve to give form and substance to an appropriately evolving mechanism. Not the least of those elements is the growth of international financial cooperation and the mutual acceptance of responsibility for the shaping of the system.

With this particular audience, I do not need to review in detail the *Report of the Group of Ten* (Ministers Statement and Deputies Annex) issued last August. Briefly, the most significant conclusions and recommendadtions of the report are:

First, a conclusion that the present system as it has evolved until now has shown impressive flexibility and adaptability, with a reaffirmation of faith in the proven value of its underpinnings in terms of fixed exchange rates and the present established price of gold.

Second, a judgment that international liquidity defined as the entire spectrum of resources available for financing payments imbalances, and taking into account a recommended and prospective increase in IMF quotas, is fully adequate for the present and near-term future.

Third, to ensure this, a call for a moderate general overall increase in IMF quotas, plus selective increases for those countries whose quotas are clearly out of line—thus adding considerably to the credit facilities segment of the liquidity spectrum.

Fourth, to further strengthen international cooperation, formalizing some of the more recent innovations and techniques, through the process of "multilateral surveillance of bilateral financing and liquidity creation."

Fifth, to make a new study with a view toward improving the adjustment process, so importantly determinative of liquidity needs.

xth, and finally, to undertake a study of the possible need for ad-
nal owned reserves and of the various ways in which that need
it be met.

this panel's limited time and space for the *Report of the Group of*
, I can only touch upon some of the more significant aspects of
of these major facets of the report.

Proven Value of Fixed Exchange Rates and Established Gold Price

irst of all, it seems to me that some of the philosophy underlying
Group of Ten Report with respect to fixed exchange rates and the
blished price of gold is worth noting. In outlining the frame of ref-
ce for the study, the Ministers and Governors of the Group of Ten
stressed the proven value of fixed exchange rates and the estab-
d gold price. The Deputies of the Ten examined the principal rele-
considerations and in their own discussions reiterated the need for
. As to the gold price, they concluded that changing it would be a
e and arbitrary way of trying to increase liquidity, would run the
risk of reducing liquidity, and would be definitely damaging to
present monetary system.

s to a system of fluctuating exchange rates, the Deputies agreed it
ld be undesirable for a number of reasons. In their view it would
rsely affect internal monetary stability because of the absence of
nce-of-payments discipline and the effects on the general price
when the exchange rate changes. They also felt that such a sys-
would introduce an additional element of uncertainty (and cost)
international trade and investment; in practice, would stimulate
ulation and disequilibrating capital flows; and would seriously en-
the risks inherent in foreign trade. Its broad effect, they agreed,
ld be to restrict rather than to expand international transactions,
to encourage national isolation rather than increased economic in-
ation internationally.

Functioning of the Present System and Present Adequacy of Liquidity

cond, as to the functioning of the present system, the G-10 report
hasizes that it "has shown a great capacity for adapting itself to
th and change, has facilitated the remarkable economic progress
eved since the war, and has withstood with success periods of po-
l and other strain although many countries are still faced with
tionary pressures and others still have unemployed resources."
report points up how the reserve currencies have, in an evolu-
ry way, become an important supplement to gold, not as a result

of deliberate planning, but through a gradual process reflecting first private and subsequently official practices. The report further stresses the integral role of growing international cooperation in the functioning of the system, with the International Monetary Fund providing a focal point. Related to this—and one of the more significant products of the studies of the Ten—has been the progress on the conceptual front concerning international liquidity, with general recognition in the report of the broad spectrum of resources available to the monetary authorities, ranging from gold and foreign exchange reserves to a wide variety of credit facilities. In particular there was special recognition of the reserve character of IMF gold tranches and of Roosa-bonds.

Reflecting this favorable view of the development of the present system and recognizing the increased role of credit facilities,[1] both present and prospective, the *Report of the Group of Ten* reaffirmed the view that the overall liquidity of the system seemed fully adequate for the present and immediate future.

Added Credit Facilities

Third, and related to the greater recognition of the role of the credit facilities component of liquidity and the central position of the IMF, the G-10 report called for a moderate general overall increase in Fund quotas plus selective increases for those countries whose quotas are clearly out of line. Such increases are already in process of implementation in the Fund Board as a result of the resolution adopted by the Fund's Governors at their Tokyo meeting. This will reinforce the Fund's resources in usable currencies which have fallen to relatively low levels. It will place the Fund in a more adequate position to cope with expanded credit needs associated both with the substantial enlargement of the world economy that has taken place since the last general increase in quotas in 1959 and with the larger needs of a convertible currency world. Most consistently a 25 percent increase in Fund quotas has been mentioned. Together with selective increases, this would add some $4 to $5 billion to the resources of the IMF.

Further Strengthening of International Cooperation

Another major recommendation in the G-10 report—also in process of implementation—was the proposal to continue and enhance international financial cooperation, already the hallmark of the postwar international monetary system, through "multilateral surveillance of bi-

[1] Of the $20 billion growth of total world liquidity since the advent of the era of currency convertibility at the end of 1958, only about two-fifths took the traditional form of gold and foreign exchange, while other similar type assets and credit availability accounted for three-fifths.

lateral financing and liquidity creation." In the first instance, "multilateral surveillance" simply means a decision among the Ten to exchange information more promptly and regularly regarding means of financing any surpluses or deficits. But it means more than this against the background of the increased financial cooperation that we in the United States, along with our European colleagues, have been experiencing and benefiting from during the past several years. Such cooperation reflects the close consultation and mutual appraisal of financial policies impinging on international payments flows in Working Party-3 of OECD, in meetings of the Ministers and Governors of the Group of Ten, and in monthly meetings at Basle of the Central Bank Governors.

The results of this increased financial cooperation are apparent in the agreement among the Group of Ten to participate in the "General Arrangements to Borrow," providing a sizable addition to Fund resources; in the *ad hoc*, Basle type, arrangements in which central banks have provided assistance bilaterally; in the network of Federal Reserve swaps and stand-by swap arrangements, now totaling over $2 billion; in the investment of over $1 billion in Roosa-bonds; and, most recently and dramatically, in the $3 billion of credit to the United Kingdom arranged by eleven countries.

Multilateral surveillance adds to and strengthens this cooperative process; in a sense we have substituted for the sometimes harsh and disruptive discipline of the gold standard a process of multilateral review and cooperative assistance that assures possible further elaboration and use of the new types of bilateral and other credit facilities that have been developed in recent years. The recent United Kingdom situation provides a clear illustration of this process in action. In early November there was a searching review of the British economic and financial situation, both domestic and external, in the Economic Policy Committee of the OECD and in Working Party-3. Following this review, agreement was reached among the Group of Ten looking to an activation of the GAB so as to provide supplementary resources to the IMF for use in meeting any drawing by the United Kingdom under its existing stand-by arrangement with the IMF. Later, when speculation against sterling threatened a major crisis, the other countries were sufficiently informed to move promptly without the need for further formal consultation.

This most recent action clearly demonstrates, even to the most skeptical, the reality of international financial cooperation and the value of *ad hoc* credit arrangements in supporting the stability of the international financial system. For despite the sting of the British action imposing a 15 percent import surcharge, the principal countries were able

within a twenty-four hour period to raise a fund that more than doubled British external resources in support of the U.K.'s determination to defend the pound sterling against a massive speculative attack.

Adjustment Process

At an early stage in their studies, the Group of Ten took cognizance of the close interrelationship between the need for liquidity and the speed and efficiency of the process of adjusting imbalances. It is widely recognized and was emphasized in the Group of Ten report, that there is a close, two-way connection between the adjustment process and the amount of international liquidity needed. The trick, of course, is to insure that the international financial system can and will provide individual countries with enough liquidity to facilitate an orderly process of adjustment, without recourse to undesirable deflationary actions or "beggar-my-neighbor" policies, but not so much as to enable countries to ignore the need for internal adjustments to restore equilibrium.

Recognizing these interconnections, the Group of Ten report recommended that Working Party-3 of OECD undertake a study of the process of adjustment involved in correcting imbalances in international payments, with a view to determining appropriate policies for avoiding or minimizing such imbalances. Again, this study is already underway with WP-3 reviewing its scope and frame of reference.

Creation of Reserve Assets

Finally, the group set up, within its own framework, a Study Group on the Creation of Reserve Assets. Although there was some difference in view as to the extent to which future needs for international liquidity could and should be met largely through continuing expansion of credit facilities rather than of owned reserves, there was general agreement that in the longer run some new form of international reserve assets might possibly be called for. But it was made clear that a long-run view is involved, and that the decision to undertake the study implies no commitment as to its findings.

As the Group of Ten report indicates, discussion among the Deputies centered around two types of proposals: (1) The establishment of a collective reserve unit (known as the CRU) among a limited group of countries, presumably the Ten; (2) the acceptance and development of gold tranche or similar claims on the IMF as an international reserve asset.

The CRU proposal would involve creation of this new international reserve unit, as a supplement to gold, in amounts to be determined by the participating countries. This proposal was probably developed from a plan first put forward over a year ago by Dr. Bernstein. In the

plan considered by the G-10 Deputies, each country's share of these units would be determined by its gold holdings, relative to the total gold holdings of the whole group. Thus a country having 10 percent of the total gold would receive 10 percent of the units. Each country would agree to hold the new unit in fixed proportion to its gold holdings—that proportion being equal to the ratio of the total amount of the new units to the total gold holdings of the group. The process of reestablishing the agreed ratio, say every quarter, by exchanges of gold and collective reserve units among the members would assure that over time the composition of each member's holdings would seldom deviate very far or very long from the agreed fixed proportions. By unanimous agreement, the group could alter the total amount of units outstanding, as a method of altering their total holdings of gold plus collective reserve units—or, alternatively, of preventing a change in that total.

Without attempting to anticipate the outcome of the work of the Study Group, I would like to explore with you some of the implications of this proposal. A major question that naturally arises as one looks at this approach is: Would the new unit supplement or supplant holdings of reserve currencies in the monetary reserves of the participating countries?

If it is intended to replace reserve currencies, it could be characterized as a proposal for a slightly modified gold standard in which gold-cum-units would be used to settle imbalances among the participating countries. Such a development would represent a sharp break with the evolution of the international monetary system, which has been moving away from heavy reliance on gold. There is also a danger that this approach would not be sufficiently flexible to provide for reserve increases at a rate adequate to accommodate the growth of world output and trade; for the procedure suggested for creating such units, involving individual member country veto, clearly implies a cautious and restrictive approach.

If, on the other hand, the scheme is intended to supplement reserve currencies, serious questions arise as to whether coexistence is in fact possible. Would not the basing of the distribution of CRU's—both at the outset and in later augmentations—give member countries an incentive to maximize their acquisitions of gold in preference to foreign exchange? And how would the reserve currency status of the dollar be affected for countries outside the limited group? In general, what would be the impact of such a substantial departure from present procedure upon that important segment of total world liquidity consisting of reserve currency holdings (at present over a third of total reserves—inclusive of IMF gold tranche positions)?

A second and related question involves the size and character of the participating group. For a number of reasons, both political and economic, there are objections to abandoning a truly multilateral framework for dealing with world liquidity problems and confining such a scheme to the ten leading industrial nations. Leaving aside the political problems, a number of thorny questions would need to be resolved concerning the economic relationships between the in-group and the rest of the world.

The main alternatives to the CRU proposal would work through the International Monetary Fund, and would involve the creation of an international reserve asset in the form of gold tranche or other claims on the Fund. The IMF gold tranche—or, more broadly, that part of the quota which is available on a virtually automatic basis—is an important example of the evolutionary possibilities that exist in the present international monetary system. For years, people tended to think of the gold tranche as fixed in amount without paying much, if any, attention to the rising gold tranche positions of countries whose currencies were being drawn by other countries. These positions are reserve assets in a very real sense; since 1957 the United States has been able, in financing its own balance-of-payments deficit, to utilize roughly $1.7 billion of its gold tranche. Most of the amount so used originated in previous dollar drawings by other countries, not in the U.S. gold subscription. And the United States is not the only country to have become aware in this way of the very tangible value of the gold tranche as a reserve asset.

Thus, semiautomatic claims on the IMF may and do originate from regular Fund transactions. But they could also be deliberately created. Two of the main techniques for doing this would be, first, to allow member countries to substitute, in connection with quota increases, something other than gold, such as a gold certificate, for the "gold tranche" portion of the increase; and, second, to make a larger portion of the quotas nearly automatic. Either of these techniques could be applied to all member countries across the board or selectively on the basis of certain criteria; but even uniform application to all members would be selective in effect, because countries that had already drawn beyond some point in their quotas would not benefit—at least, not immediately.

Either of these techniques would utilize familiar mechanisms of the Fund and would not be expected to have marked repercussions on the use of reserve currencies or on the composition of reserve holdings. They would clearly be a supplement to the existing monetary system. There are, of course, some problems even in these techniques, but they

have the advantage of developing rather naturally from familiar Fund policies and mechanisms.

Another main technique of reserve creation through the Fund would be via Fund investment, of one sort or another, in member countries. This approach would be a more marked innovation, and would raise more complex problems.

All I wish to suggest at this point is that various means for expanding reserve creation through the IMF exist and will receive careful consideration in the Study Group as well as in the Fund itself. If it becomes desirable to adopt a new approach to reserve creation, there is much to be said for using the already-tested Fund framework and for extending that framework, if and when necessary, in the light of experience gained.

Concluding Comment

This brings me full circle in my brief attempt to capture some of the flavor and substance of the *Report of the Group of Ten.* I began by emphasizing its contribution to the evolutionary development of our international monetary mechanism, and this is indeed the essence of the various ingredients in the report. It visualizes a monetary system marked by even greater international cooperation, by expanded credit facilities largely centered in the International Monetary Fund and with bilateral facilities rapidly expansible in case of urgent need, by an improved adjustment process, and finally, if needed, by the provision of additional owned reserve assets—most appropriately, in my judgment, within the already tested Fund framework—as part and parcel of the full spectrum of availabilities that currently constitute international liquidity.

THE REPORT OF THE INTERNATIONAL MONETARY FUND

By JACQUES J. POLAK
International Monetary Fund

I

At its annual meeting in Washington in September, 1963, the Managing Director of the International Monetary Fund indicated that the Fund would, in the coming year, intensify its study of the problem of international liquidity. The work done during that year was published, in August, 1964, as Chapters 3 and 4 of the Fund's *Annual Report*.[1] Chapter 3 gives a broad analysis of the general issues of international liquidity; Chapter 4 analyses the Fund as a source of liquidity and indicates the further contributions that the Fund can make towards the provision of international liquidity: in the immediate future by an increase in quotas and, potentially, by a variety of new techniques that are briefly sketched at the end of this chapter. These two chapters of the Fund's *Annual Report* and the liquidity report of the Ten[2] are in many respects related documents. Not only were they, by design, released to the public on the same day, but there had also been close links between those who participated in the writing of these two documents. There are, to be sure, important differences between the two reports. The report of the Ten is a document written by high officials of ten governments. The report of the Fund was, in first draft, written by the Fund staff for an international organization with a membership of 102 countries, and then discussed, amended, and issued by its Board of nineteen Executive Directors (in which of course the ten directors from the main industrial countries have great weight). But in the longer run, these two attempts by those responsible for international monetary management will no doubt prove important primarily for what they have in common. Both represent important steps towards a fuller understanding of the problem of international liquidity and towards constructive action with respect to this problem.

These two reports do not represent the final stage of the intensive work on international liquidity; the subject has been carried considerably further by what happened since their release. The Fund meeting in Tokyo last September should be noted as a particularly important event in this connection: the public speeches of leading Governors

[1] Int. Monetary Fund, "1964 *Annual Report*" (Washington, D.C., 1964).
[2] *Ministerial Statement of the Group of Ten and Annex, Prepared by Deputies* (Paris, 1964).

gave for the first time a clear indication to the world at large of the positions of important countries and thus made it possible for informed public opinion to participate more closely in the shaping of official attitudes.

I would hope that the presentation and discussion of some of the main issues before this large group of economists will contribute towards enlisting the thoughts of many of you to this fascinating subject, and I have, therefore, responded with enthusiasm to the invitation to participate in this discussion. Studies are going on in the Fund and elsewhere; at the same time the practical work of creating and managing international liquidity goes forward. The Fund is now engaged in sorting out the many problems that arise in connection with an increase in its quotas of some $4 to $5 billion; we would hope that this increase could be decided in a matter of weeks, to go into effect, after the necessary parliamentary approval, perhaps by the time of the next annual meeting. The Fund has also had occasion earlier this month to bring into operation, for the first time, its General Arrangements to Borrow. This was done in connection with a one billion dollar transaction for the United Kingdom. The activation of these General Arrangements to Borrow had at least two important liquidity implications. First, it created a new international asset, in the form of a loan claim on the Fund, which eight countries now hold as part of their reserves for a total amount of U.S. $405 million. Similar in most liquidity respects to the "gold tranche positions" in the Fund (of which over seventy countries hold a total of about U.S. $4 billion in their reserves), these new reserve assets may point the way towards further means of liquidity creation by the Fund. Second, and perhaps of larger immediate importance, the first actual operation of the arrangement proved that it could be worked and thus raised by a number of notches the liquidity value of this U.S. $6 billion arrangement. The size and the rapidity of the U.S. $3 billion central bank support operation for sterling staged just a month ago proved once more the cohesion that has been created among the monetary authorities of the industrial countries. These recent developments form a valuable setting in which to appraise the liquidity work of the last year or so.

II

One of the major achievements of the process of international discussion that has led up to the two reports mentioned has been the increased understanding gained. If one compares the content of these reports or the Tokyo speeches with official utterings on international liquidity of only one or two years ago, one notices a great clarification of the issues.

One evidence of this is a far more sophisticated terminology. Thus, liquidity is clearly seen as a broad spectrum of resources available to meet balance-of-payments deficits, ranging from readily available assets to resources that may be available only after extensive negotiation. Within this spectrum, a distinction is now generally made between, on the one hand, liquidity in the form of reserves, or "unconditional liquidity," and, on the other hand, that part of international liquidity that consists of countries' access to credit facilities, the latter usually of a "conditional" character. Not only is the distinction made, but views are also increasingly cryptallizing as to the relative merits and roles of the two types of liquidity. Possession of an adequate amount of unconditional liquidity allows countries to engage in policies that may entail balance-of-payments difficulties with the knowledge that they will be able to meet such difficulties should they materialize—but, of course, with the concomitant risk for the world at large that policies may be pursued with too little regard for the possible inflationary effects on others. Conditional liquidity is better able to ensure that the international point of view will be brought to bear on the action of countries with respect to their balance-of-payments policies; but this may entail the risk that countries may needlessly restrain expansionary policies for fear of having to submit to the views of international agencies or of other countries when they may need balance-of-payments assistance. The discussion on this point would appear to have produced a certain measure of agreement that there is a need, and, with an expanding world economy, a growing need for both types of liquidity in some balanced way—which does not necessarily mean that the present proportions between the two should be preserved.

Further along the same line, a consensus appears to have arisen to the effect that the level of reserves (as well as their composition and distribution) is a matter of importance to the world economy even though there remains considerable disagreement or uncertainty as to how the need for reserves should be measured and whether the present level of reserves is too large or too small. This consensus then leads to the next step; namely, a tentative, but probably an irreversible, recognition that if the "natural forces" on which the world has so far relied for its supply of international liquidity do not produce an adequate supply of such liquidity in the future, it would be proper and necessary, in some appropriate way, deliberately to proceed to the man-made provision of reserves. It has been accepted for decades that the level of the domestic money supply is a matter for decision by the authorities. A position somewhat approaching to this (I would not want to go further) appears now to have developed among international policy-makers with respect to the supply of international liquidity.

It is important to stress, however, that the emerging willingness to act on international liquidity does not imply acceptance of the somewhat naïve (though widely held) view that there is a close parallel between international and domestic liquidity. The truth is, as the Fund has pointed out for years in its annual report and in public speeches, that the theory of domestic money has only a quite limited validity as a model for a theory of international liquidity. Domestically the regulation of the money supply is a means by which central authorities act to induce a desired reaction in the economy. They expect the millions of firms and households to respond to the tightening and loosening of monetary conditions, influencing them in this manner rather than by direct controls or by fiscal action. The control that can be exercised domestically by monetary action is by no means perfect, but it works tolerably well.

The management of international liquidity is different in many relevant aspects.

First, there is not the same distinction between the authority that determines the amount of liquidity and those that are to respond to this decision. Any decisions on liquidity will be taken jointly by the same small group of people who then, in their national capacities, are expected to adjust policies in the light of the amount of liquidity at their disposal.

Second, it is by no means obvious that the regulation of international liquidity will be the most effective instrument to induce countries to follow policies that are considered desirable from an international point of view. There is no lack of examples of countries which, even though they have no reserves left and no access to credit except perhaps on frankly political grounds, continue the most crudely inflationary policies. And one further point that is of general validity but has perhaps been most vividly brought home by recent developments: liquidity measures can change the degree of ease felt by a country, but it requires a great deal of further understanding among countries—be it by general rules or by *ad hoc* negotiation—to ensure that financial tightness leads to corrective action that is of a generally desired character.

III

It is for reasons such as these that a discussion on international liquidity cannot sensibly be limited to the more or less technical problems of how much liquidity to provide how, but must inevitably spill over into the broader field of international economic policies. It is no accident, therefore, that the reports on international liquidity devote a good part of their discussion to problems that might appear to lie out-

side the scope of that subject, narrowly conceived. One must indeed realize that the debate on the international liquidity question is to a large extent a discussion of wider issues of international economic collaboration *sub specie liquiditatis*.

It has of late become customary to designate that wide field by the label of "balance-of-payments adjustment," and this has been helpful up to a point. Thus, it is useful to be reminded, as is done in the report of the Fund, that "the principal effects of changes in the level of international liquidity are likely to be those exercised through their repercussions on the policies followed by national governments in relation to balance-of-payments situations." But it should also be realized that "adjustment" is at best an intermediate objective of policy. The objective which we hope to achieve by means of proper arrangements in the field of international liquidity is not adjustment as such, but includes the more ultimate aims of economic policy such as growth, full employment, price stability, freedom of international transactions, etc. The analysis of the "adjustment process" on which Working Party-3 of the OECD has now embarked as a sequel to the liquidity study of the Ten, will no doubt come into proper focus; i.e., an analysis of the means to be taken to bring national economic policies more closely into international harmony.

Whatever way this question is approached, it is clear that in the end progress on the question of international liquidity will to some extent be determined by the rate at which a consensus over a much wider area of economic policies can be reached rather than by the solution of "technical" problems alone. Moreover, as the Fund stated in its report, "a collective judgment as to whether the available supply of liquidity is adequate or inadequate is particularly difficult to arrive at because the balance of advantage, at any rate in the short run, may be different with respect to, and in the opinion of, different countries. Action in the liquidity field which absorbs unemployment in one country may promote excessive demand in another, and any change in the supply of international liquidity is likely to involve some transfer of resources, at least temporarily, between countries." In these circumstances, it might well be found that progress was initially most feasible where the particular technique of liquidity creation served at the same time some other policy objective of the countries most directly concerned.

For a while it may have appeared that necessity alone would force governments to agree on new arrangements for the creation of international liquidity. If gold production did not rise, if dollar balances ceased to be accumulated, would not lack of liquidity put the world economy into a squeeze where circumstances alone would virtually dictate agreement on some suitable remedial action? In the last few

years, however, these quasi-Malthusian predictions about liquidity appear no longer quite as threatening. The Fund's quota increases of 1959-60 demonstrated that large liquidity increases for all countries could be agreed by international action. The recent ingenious devices which we owe above all to Mr. Roosa show that it is after all exceedingly easy for a small number of central banks and governments to create, on an *ad hoc* basis, facilities by which they can pay each other. The problem before us has, therefore, undergone a subtle change. The question is not only whether there will be enough international liquidity, but also, increasingly, whether the international or intergovernmental action to create liquidity will be systematic or merely *ad hoc,* as part of a code of behavior that will promote the balanced growth of the world economy or merely to bail out problem situations. To the proper solution of these problems the Fund can—as its *Annual Report* stated—"make an essential contribution."

IV

Within the framework of the Fund one can envisage two broad methods of increasing unconditional liquidity—apart from the conditional liquidity of which the Fund is of course the major source of international supply. One method is to raise countries' essentially automatic access beyond its present range, which equals what each country has contributed in gold or balances of its currency drawn by other countries. The extension of this automaticity some distance into the Fund's "credit tranches" would be tantamount to the creation of unconditional liquidity for all members, except initially to those that had already drawn that part of their credit tranches on a conditional basis.

The second method would be by what has been called the "investment" technique. Under this technique the Fund would, at its own initiative and in the light of general conditions in the world economy rather than in response to balance-of-payments needs of individual countries, acquire assets in member countries (directly or via some agency such as the IBRD). As the counterpart to such assets, the liability side of the Fund's balance sheet would show additional claims on the Fund by members. Such claims would be liquid and would constitute additional reserve assets essentially similar to the gold tranche positions and GAB claims on the Fund to which I referred earlier.

The most interesting technical questions arise with respect to the characteristics that these Fund liabilities should have in order to ensure that they are effective reserve assets for countries holding them. Important questions of equity, on the other hand, arise in connection with the asset side of the Fund's balance sheet—the country distribution of any investment made—and in connection with the process of decision

making as regards the amount of liquidity to be created. On the latter point, it may be, as mentioned above, quite difficult to reach agreement on the amount of international liquidity that should, at any moment of time, be created. To a far greater extent than applies to national decisions on the appropriate rise in the money supply such decisions at the international level will represent a compromise between conflicting interests rather than a compromise between divergent views. Difference in interests may arise not only from differences in economic structure of countries—developed and less developed, for example—but also from differences in economic position at a particular moment of time, which may determine whether the risks of inflation or those of balance-of-payments deficit loom larger in the eyes of certain governments. In view of these differences, equity requires that the interests of all countries are properly represented rather than that this be a matter for exclusive decision among a small group of countries.

Any investment action to create international liquidity, whether it passes through the accounts of the Fund or through that of a more limited international organization, will make liquid purchasing power (reserves) available to the countries in which investment is made. These reserves will be matched for each receiver by a liability to the agency—but a liability which (like the banknote liability of a government or a central bank) is most unlikely to be called as long as the world economy expands and the need for liquidity continues to rise. Indeed, while there may be some room for cyclical short-term investment for liquidity purposes and while investments may well be made in short-term paper, the main function of international liquidity creation must be such as to assure the receiver of investments that he can safely consider the money as part of his reserves because it will not normally be called. This rather obvious point appears to have been overlooked by those who advocated a system for liquidity creation among and for the main industrial countries. But a system that adds purchasing power exclusively to the reserves of the countries with the highest per capita incomes must appear grossly inequitable to the rest of the countries who are left to earn through balance-of-payments surpluses such reserves as they feel they need for their economies. One wonders, indeed, how a proposal of this nature could have been made in a world so preoccupied with the need to assist the less developed countries. One should note also the arbitrary and invidious demarcation line that any such proposal draws, not only between the rich countries with large reserves and the poor with very small ones, but also between the countries that happen to be "in" and those with similar economies, financial structures, and reserve holdings that are "out." Outside of the major industrial group there are quite a number of

ountries (certainly more than ten) that hold relatively large reserves; ıor can countries conveniently be separated for this purpose as to vhether they are industrial or nonindustrial.

A grouping of the more affluent countries—such as exists, for examıle, in the Fund's General Arrangements to Borrow and in the "Part " countries of the International Development Association—does arise nore or less naturally where this distinction implies the assumption of pecial obligations in the financial sphere. If new forms of internationıl liquidity are created, one would expect that these new reserve asets—unlike the matching amounts of investment claims held by the Fund—would predominantly be held by a somewhat limited number of ıigh-reserve countries, in the same way as at present creditor positions n the Fund are held by a small number of countries whose currencies ıave been used for drawings by the whole Fund membership; no par:icular question of equity arises in this connection. It would be neces;ary to ensure that the character and the manner of issuing of such ıew instruments of international liquidity would be such as to fit prop:rly into the reserve pattern of these countries, and the close attention hat these countries are now giving to these matters will be essential to he achievement of a practical solution that will be satisfactory all ıround.

The considerations that I have mentioned weigh heavily in favor of basing any solution for the further creation of international liquidity on the Fund and it would appear, for example, from statements made on this subject in Tokyo, that opinion in member countries is definitely noving towards this view.

THE REPORT OF THE NONGOVERNMENT ECONOMISTS' STUDY GROUP

By FRITZ MACHLUP
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The report[1] on which I am to speak was published in August, 1964, the same month in which the other two reports, discussed by the two previous speakers, were published. I take it that my assignment includes comparisons with these other reports and I shall, therefore, emphasize the major differences and similarities.

Differences in Design and Purpose

The first difference between the report of the Group of Thirty-two, and the reports of the Group of Ten and of the IMF lies, of course, in the affiliations of the authors. The Group of Ten is a group of governments; their statement and the Annex, the report of the Deputies, were written by representatives of national governments. The IMF is an international organization of national governments; its report was written by staff members and officers representing international government. The Group of Thirty-two, on the other hand, is an international study group of thirty-two economists not affiliated with national or international government, at least not directly and full time.

This difference in affiliation is essential: first, because the formation of this nongovernmental study group was induced by the announcement that the other two studies would be made solely by government people; second, because the private character of the economists' study permitted the consideration and discussion of proposals that were "out of bounds" in an official inquiry (such as proposals for changes in the price of gold or in foreign exchange rates); and, third, because economists speaking only for themselves, in an atmosphere like that of a university seminar, can more easily examine all sides of an issue than can experts speaking for their governments and instructed to defend more or less fixed positions.

Another difference deserves emphasis: the difference in purpose. The two official studies were designed to seek agreement on courses of action; the private study was designed to interpret disagreement. This

[1] *International Monetary Arrangements: The Problem of Choice* (International Finance Section, Princeton Univ., 1964). Later referred to simply by page references in the text and after quotations.

group included economists "with notoriously divergent views" (p. 8) and its assignment was to identify the sources of their disagreements. I quote:

Among the valuable results of a study undertaken by experts known for their diametrically opposite recommendations may be the specification of the particular judgments of facts and objectives which are responsible for the conflicting conclusions. Perhaps some of the differences in judgments of fact can be resolved by further study, and some of the differences in judgments of value may be reduced by a non-emotive analysis of their places in a common hierarchy of higher goals (p. 6).

Despite this clear declaration of purpose, the report has been criticized for its poor performance "as a guide for action."[2] The critic complained that the report would not help policy-makers "make rational decisions on alternative courses of action." This last clause is a disjointed quotation from the following passage in the report:

The consensus that mattered most to some of the members of the Study Group was not on the questions of what to do now and what to recommend for the future—an objective they considered inconsistent with the recognition of divergent value judgments and divergent assumptions about unknown facts—but rather a consensus on what were some of the things on which the consequences of particular measures or arrangements would depend. This consensus permits those who know what they want and who believe they know the most probable answers to the open questions of fact to make rational decisions on alternative courses of action. A consensus of this kind was achieved without reservations (p. 107).

In other words, the report leaves to the policy-makers explicit choices among the most plausible assumptions of fact and the most worthwhile social objectives. This would be genuinely rational decision making. Can one reasonably demand that economists who disagree on unknown facts and on competing goals come forth with the same prescription for policy-makers of all and sundry beliefs and values?

The Contents of the Report

For the benefit of those who have not seen the report of the Group of Thirty-two, let me furnish a brief outline of its contents.

Chapter I, entitled, "The Project and the Group: Purposes and Procedures," describes how the project was conceived, how the group was formed, how its members were selected, and how they proceeded with their task.

Chapter II, entitled, "Problems and Objectives," distinguishes the three major problems of our international monetary arrangements—adjustment, liquidity, and confidence—and reviews the major objectives and the conflicts between them.

Chapter III presents "More on the Three Problems and on Some Approaches to Solve Them." The question of the "desirable speed of adjustment" is examined with reference to various "types of distur-

[2] J. Herbert Furth, "The Machlup Report—A Critical Evaluation" (unpublished manuscript).

bance" and to the relative frequency with which they are likely to occur.

Chapter IV, entitled, "Assumptions Underlying the Proposed Approaches," is the core of the entire project, in that it formulates the different and often contradictory assumptions on the basis of which different approaches to the solution of the problems may be advocated. The four approaches selected for analysis are the semiautomatic gold standard, centralization of international reserves, multiple currency reserves, and flexible exchange rates.

Chapter V, called, "Toward a Consensus on Policy," states a few propositions on policy on which there was extensive, though not unanimous, agreement among the members. Given their high propensity to disagree, it was felt worthwhile to record the few issues of policy to which a majority of participants would give a consenting nod.

I regard the formulation of assumptions as the core of the study because a rational choice among different courses of action presupposes an evaluation of the factual and normative judgments on which their advocacy rests. This does not mean that reasonable people, including some economists, will ordinarily begin with their assumptions of fact and judgments of value, then engage in a process of sober logical deduction, and finally arrive at conclusions in the form of policy recommendations. I am quite ready to admit that most policy recommendations consist of ready-made prescriptions, prejudged solutions without full awareness of the major and minor premises on which they supposedly rest. Only when challenged by critics or confronted with conflicting recommendations will most advocates begin to rationalize their proposals by exposing the underlying suppositions. But precisely this is needed, since the validity of the solutions can be judged only by evaluating the validity (or comparative probability) of the relevant factual assumptions and the acceptance (or comparative weight) of the relevant value judgments.

Factual Assumptions and Normative Judgments

Instead of offering more methodological generalities, I had better furnish some illustrations.

The advocacy or rejection of flexible exchange rates rests to a large extent on assumptions regarding future attitudes of central bankers and private traders and investors. Concerning central bankers, the question is whether their resistance to inflationary pressures will or will not be reduced if the fear of dwindling international reserves is removed. The advocates of exchange rate flexibility assume that central bankers will fear drastic exchange depreciation under flexible rates no less than they now fear reserve depletion under fixed rates. The op-

ponents assume that a loss of reserves always impresses central bankers much more forcefully than a drop in the exchange rate would. Concerning private traders and foreign investors, the question is whether they will be discouraged more seriously by the cost of hedging against short-run variations in exchange rates and by the exposure to the risk of long-run variations against which hedging is not possible, than they are by the threats of trade and exchange restrictions so frequently instituted to combat payments deficits under fixed exchange rates. Opponents and advocates of flexible rates entertain opposite assumptions regarding these possibilities.

Questions like these cannot be answered with certainty. Empirical evidence of past behavior and present attitudes cannot give any assurance regarding future behavior patterns; after all, no one can deny that central bankers, foreign traders, and private investors may be able to learn from experience.

In any event, policy recommendations depend on factual assumptions, and one may reasonably disagree about which assumptions are the most plausible. Once it is clear where the crucial differences lie, discussion by the experts can be more sharply focused on the relevant points, and the range of assumed possibilities may eventually be narrowed.

Some of the assumptions are so closely correlated with conflicting political philosophies that it is hard to distinguish between assumptions of fact and judgments of value. I submit that the nineteenth-century liberal mistrust of government plays a distinct role in the disagreements on international monetary systems. This mistrust of government, however, does not offer an unambiguous guide to policy. Indeed, the two most extreme approaches—the semiautomatic gold standard and the system of freely flexible exchange rates—may both be interpreted as designs to reduce, in one area or another, the scope of governmental discretion, to substitute for a "misplaced" reliance on wise policy making a "safe" observance of fixed rules and "assured" operation of automatic mechanisms.

The advocates of the semiautomatic gold standard want to remove the central banks' power to meet a payments deficit by anything other than a sale of gold and to extend domestic credit to offset the deflationary effects of the gold outflow. The advocates of flexible exchange rates want to remove the central banks' power to intervene in the foreign exchange market by official sales or purchases and to interfere by restrictions on private transactions. The friends of the gold standard favor "automatic contraction" of the money supply in a deficit situation, because they assume that central bankers have too high a propensity to expand credit. The friends of flexible exchange

rates favor "automatic depreciation" of the currency in a deficit situation, because they assume that governments have too high a propensity to restrict foreign trade and payments. Both these assumptions may well be justified; but the conclusions are based on supplementary assumptions, which perhaps are unrealistic; namely, that the particular automatic devices would be allowed to function when the propensities which they are supposed to keep in check act up with great force. The institutional arrangements proposed in the respective plans include international agreements prohibiting interferences with the desired "automaticities," and the implied assumption is that these agreements would be faithfully observed, no matter what.

It may not be immediately clear that differences in value judgments—in the marginal rates of substitution between alternative social objectives—are inseparably tied up with the stated assumptions and conclusions. Those who favor automatic contractions of the money supply seem to place whatever ultimate values are derived from price-level stability above the value of maintaining higher employment levels in the short run. And those who favor automatic reductions in the foreign exchange rate seem to place whatever values are derived from free markets and freer trade above the values derived from protecting certain sectors of the economy against instability in prices paid or received.

Of this sort are the issues brought to the surface by the type of study performed by the Group of Thirty-two. Only by identifying the differences in factual assumptions and value judgments that underlie the plans and proposals advocated by different economists can the disagreements be fully understood and appreciated. It is on these grounds that the Thirty-two believed that their findings could contribute to more rational decisions by policy-makers.

Conditional Advocacy and Implied Criticism

It should now be clear why the report does not endorse any particular system or type of international monetary arrangement. The four approaches examined by the Thirty-two are treated without prejudice and without partisanship. The semiautomatic gold standard, centralization of international reserves, multiple currency reserves, and flexible exchange rates were studied as "pure types," and the findings are presented in the form of propositions which the advocates of the particular approach accept—or ought to accept if they want to be rational in their advocacy. Let me quote:

> The lists of propositions which, if accepted as pertinent, correct, and realistic, would justify the adoption of a particular system, cover essentially three questions: A. In what respects are the present-day system and the three other proposed systems inferior to the one under consideration? B. What are the essential arrangements which characterize the

system? C. What are some of the necessary conditions for the system to work in the intended fashion? (P. 71.)

Someone who rejects any of the important assumptions implicitly rejects the approach that rests on them. This has led some readers to the erroneous belief that the Group intended to criticize or oppose any of the four approaches. To be sure, the compilation of the necessary or relevant assumptions furnishes a fine collection of arguments to anyone who wants to criticize or oppose any of the plans under discussion. In order to demonstrate that a particular plan will not work, he has only to select from the list of required assumptions some that appear unrealistic. In addition, he may turn to the propositions in Part A of the lists of assumptions underlying the advocacy of alternative approaches. For:

Advocacy implies criticism. One cannot reasonably recommend a plan or approach without implying that it is in some respects better than what we now have and what others recommend. Hence, a statement of the assumptions which underlie the recommendation of any plan or approach must include critical propositions about both the present system and alternative approaches (p. 66).

Thus, the assumptions underlying any one approach include propositions critical of the others. In the opinion of an otherwise highly critical reviewer of the report, "the criticism of the flexible exchange-rate system included in the analysis of the 'centralization of reserves' proposal may be considered the best refutation possible in such narrow space."[3] The report, I repeat, was not intended to be more critical of any one approach than of any other. Criticism of all was implied in a comparative analysis that attempted to spell out the propositions on which advocacy of any of the systems could rest. Criticism, it seems, is more persuasive than advocacy, and thus our study may result in a strengthening of the opposition to all four approaches analyzed.

If this were to reinforce the defense of the *status quo,* the Thirty-two would probably regret it as an unintended effect of their work. They had been quite outspoken about their attitude toward the present arrangements:

Since none of the conferees preferred the present system to all proposed alternatives, no statement of propositions supporting its maintenance was prepared (p. 66).

They need not worry, though. There is no real danger that the present system will be maintained, no matter how eloquently it is defended in some quarters. With all due recognition of the contributions past and present arrangements have made to the highly beneficial "loosening of controls, liberalization of international trade and payments, and increased international cooperation in national policies" and indirectly to a remarkable "increase in international trade and investment," one

[3] Furth, *op. cit.*

cannot help realizing that these arrangements "are becoming increasingly inadequate" (p. 68).

On this point all three reports agree. There is at best a difference in language, the two official reports being, quite properly, more politic. The Group of Ten, frankly enough, speak of the "lines of future development," of "having recognized the uncertainties concerning the future supplies of monetary reserves," of various ways "to supplement the existing system by a new type of reserve asset," and of the need for studies of how countries "could in the future preserve a better balance-of-payments equilibrium and achieve a faster and more effective adjustment of imbalance."[4] And the IMF report regards it as "both desirable and timely to enter upon a broad exploration of the possible ways to meet any inadequacies in the supply of international liquidity to which the present system might otherwise give rise and to offset any undesirable fluctuations in it."[5]

In other words, the need for changes in the international monetary system is recognized by the official as well as the private experts who produced the three reports. There are those who think that the nongovernment economists are more impatient and more radical; and there is much naïve talk about "revolutionary" designs by the private economists, in contrast to "evolutionary" moves contemplated by the official experts. The fact is that academic economists speak more bluntly and are prepared to act their proper roles as gadflies, while government representatives are properly diplomatic and coy, and must pretend to yield only to necessity. Moreover, the international monetary system has been changing all along and will continue to change. Its evolution will surely include steps which some diehards have decried as revolutionary or, even worse, as impractical. I submit that the buzzing of the academic gadflies has been extremely important in promoting better understanding and official appreciation of the issues in question.

The Problem of Adjustment

All three reports have a good deal to say about the problem of adjustment of the balance of payments. There are many similarities in the views expressed, but also some differences. By and large, the nongovernment economists are more orthodox and classical than the official experts. Their stress is on relative prices, incomes, and exchange rates—that is, on endogenous market variables—whereas the government experts place much emphasis on the use of exogenous variables, policies designed to affect the flow of goods and funds by other

[4] *Ministerial Statement of the Group of Ten and Annex Prepared by Deputies* (Aug. 10, 1964), pp. 8, 10, 5.
[5] International Monetary Fund, *Annual Report 1964* (Washington, D.C., 1964), p. 32.

means. This "wide range of internal and external measures"[6] enumerated in a list of "instruments of economic policy"[7] differs in operation and in side-effects from the forces of adjustment described in classical theory. To treat them as if they were equivalent or even preferable substitutes for the classical mechanism can be misleading.

In my opinion, a basic distinction should be made between "real adjustment"—which refers only to a reallocation of productive resources and exchanges of goods and services under the influence of changes in relative prices, incomes, and exchange rates—and "compensating corrections"—which may affect the current account or the capital account by other means and, if successful, may remove the need for real adjustment. I cannot now elaborate on this distinction, but plan to do so on another occasion.

In one important respect, the Thirty-two are rather unorthodox: when they analyze the circumstances in which it might be appropriate to delay, retard, or inhibit the working of the classical adjustment mechanism (pp. 43-52). This analysis, I believe, represents an advance in the theory of the balance of payments, even if it is not yet, and may never become, practically operational.

The Problem of Liquidity

On the problem of liquidity, no less than on that of adjustment, one can find similarities and differences of opinion in the three reports. The similarities are greatest with regard to the future provision of liquidity—all the reports agreeing that something will have to be done to avoid inadequacies in the growth of liquidity—and with regard to the dangers of excessive liquidity. On the latter, the Group of Thirty-two has this to say:

(a) excessive liquidity may inhibit both the natural adjustment processes and the pursuit of appropriate policies of accelerating adjustment;
(b) the use of liquidity involves withdrawing real resources from other countries, or acquiring claims on their real resources, and so may be unwelcome to them (p. 30).

Similar statements can be quoted from the other reports.[8]

The most conspicuous differences concerning the problem of liquidity relate to the treatment of borrowing facilities. Like the other reports, the Thirty-two make various distinctions, such as between "owned reserves, borrowed reserves, and borrowable reserves" (p. 30), or between various sorts of borrowing arrangements. However:

For concreteness and precision the conferees agreed to confine the concept of the international liquidity of a country to the sum of owned reserves and unconditional drawing rights (p. 31).

[6] IMF, p. 28.
[7] Group of Ten, p. 5.
[8] See especially, IMF, pp. 26 and 33.

This deliberate limitation is likely to be regarded as a defect of the report of the Thirty-two, particularly in view of the importance the official studies, especially the IMF, attach to the supply of "conditional liquidity" as a means of "ensuring that appropriate corrective measures are applied."[9]

The decision to narrow the discussion to measurable liquidity can be defended by stronger arguments than the appeal to "concreteness and precision." The report of the Thirty-two, however, does not make these arguments, and I submit that we lack as yet a good analysis of the differences between credit facilities and credit balances, or between borrowing facilities, gross reserve assets, and net reserve position. I have the impression that some of the disagreements within the Group of Ten go back to the uneasy feeling of some central bankers that net reserves and borrowing facilities have rather different functions, and that their fusion in a vague concept of international liquidity serves confusion more than anything else.

I share these apprehensions. To secure the international monetary system against shocks and disturbances from payments imbalances of individual countries is one thing; it is another thing to secure the system against deflationary pressures due to a failure of international reserves to grow at a rate deemed sufficient to sustain the desired growth of national money supplies. Borrowing facilities can serve the first but not the second function. I submit that we are witnessing the recurrence of an old misconception, which on the domestic level has perplexed students of monetary theory and policy for a century and a half: the confusion between the supply of credit to borrowers who lack working capital and the supply of money to an economy confronted with a secular decline in prices and money rates of earnings. It is true that the commercial-banking system has in fact increased the supply of money by lending to those who lacked capital and by increasing, in the process, the total credit outstanding. It is also true that the same international arrangements that provide credit to monetary authorities suffering from payments deficits may, in the process, provide for the secular growth in reserves that may be needed to avoid deflationary policies in most countries. This does not justify, however, our continuing confusion in these matters.

The failure to separate, at least in theoretical analysis, the creation of money and the supply of finance (capital) to individuals, firms, and governments, may be regarded as the original sin of commercial-banking theory. Likewise, the failure to separate, at least in theoretical analysis, the creation of international reserves and the supply of for-

[9] IMF, p. 33.

eign funds to countries in deficit, is the original sin of central-banking theory. May we soon be redeemed from both!

The Problem of Confidence

The third major problem discussed by the Thirty-two—the problem of confidence—is not explicitly treated in the official reports. In this instance, therefore, I have to explain and defend the inclusion of an issue which the government experts apparently did not regard as an issue.

The report of the Thirty-two explains what it means by the problem of confidence. Foreign holders of dollars and sterling might, at any time, wish to convert their holdings into gold. This could happen even in the absence of basic deficits of the United States and the United Kingdom, and in the absence of a shortage of "international liquidity." While such demands for conversion would, of course, create new problems of adjustment and new problems of inadequate liquidity, the problem of confidence in some of the existing reserve assets can arise independently. To say that it can arise is not to say that it is likely to arise—but some economists would be reassured if the "overhang" of dollars and sterling were somehow funded or consolidated.

Others deny that this problem is urgent, and as evidence they point to the reassuring "growth of cooperation and consultation among central banks" (p. 61). As long as this cooperation continues and as long as the major countries realize their common interest in the viability and stability of the international monetary system, one need not fear that any country will precipitate massive flights out of one or both of the present reserve currencies. Indeed, negotiating an arrangement for "consolidation" might be riskier than leaving things alone.

In reply to these objections, those in favor of removing the threats of a "collapsing overhang" state that consolidation may take forms that would not imply an exchange of liquid funds against less liquid claims. Consolidation through interposition of a guarantor would increase the liquidity of the reserve-currency holders as well as of the reserve-currency issuers. Even if one argues that the nongovernment economists have exaggerated the urgency of solving the problem of confidence, one can hardly exaggerate the desirability of solving it.

Substitutes or Complements

The four plans, systems, or approaches analyzed by the Group of Thirty-two are commonly considered as disjunctive alternatives. This was also the presumption in the minds of the economists when they began their discussions. They soon discovered "a complementarity between proposals they had first thought to be rivals" (p. 105).

Such complementarity follows from the fact that particular arrangements are designed to deal with certain problems but not with others. For example, neither centralization of international reserves nor the use of multiple or composite currency reserves can contribute much, if anything, to the problem of adjustment. To solve this problem is the chief concern of those who advocate a semiautomatic gold standard or flexible exchange rates. With such differences in their functions, the different proposals need not be incompatible with one another, and various combinations seem possible. Thus, certain features of the semiautomatic gold standard, promoting "adjustment," may well be combined with multiple currency-reserve holding as a means toward increasing the growth of "liquidity." Similarly, centralized reserve creation, to increase "liquidity," may go together with limited exchange rate flexibility, securing faster "adjustment."

The possibility of such combinations may lead to unexpected coalitions and alliances in the debates about improved international arrangements. Since it is almost inconceivable that any one group or country will get its favorite plan accepted, the recognition that various proposals are not genuine alternatives but are mutually compatible may eventually promote compromises with which all of us can live.

The Utility of the Exercise

There are those who cannot see any value in a report about the sources of disagreement on economic proposals. They think that economists who make conflicting recommendations cancel out one another, leaving a sum of zero. And that, furthermore, if we descend to a lower level of the economists' disagreements, namely, to the underlying arguments, to the factual and normative assumptions, the result is the same: zero. It is sad that economists disagree; to know why they disagree does not improve matters—in the view of these skeptics.

This skepticism I regard as unwholesome, as sheer obscurantism. There is a world of difference, I submit, between unreasoned conflict and reasoned disagreement. Only by examining the underlying assumptions can we ever hope to resolve conflicts of opinion. This is not just an empty principle or a pious hope. The thirty-two economists have seen the principle in action: they saw how some members of their group changed their views about certain plans or about certain features of certain plans after they had isolated and dissected the previously hidden assumptions that have to be satisfied if these plans are to work in the intended fashion.[10] For, while some of the factual as-

[10] As an illustration, I may explain how I changed my views regarding unlimited, unmanaged flexibility of exchange rates. When I pondered the assumptions that governments would have to agree to stay out of foreign exchange markets and that strong public pressures opposing appreciation and depreciation would have to be overcome, I realized that limited flexibility was all that could reasonably be advocated at this time.

sumptions may not be testable now or in the near future, others can be appraised as more or less realistic on the basis of merely casual empiricism.

I conclude that the exercise on which I have reported was valuable to the economists who took part in it and that the results may be of value for both theoretical economics and political economy.

DISCUSSION

MILTON FRIEDMAN: Professor Machlup's paper, the report of the Group of Thirty-two, and conversation with members of the group all make me regret exceedingly that I was unable to accept Professor Machlup's invitation to be a member. It was clearly an extraordinarily exciting and productive experiment. The resulting report is a model of its kind. No student of international monetary arrangements, however profoundly he has studied the subject, can fail to be instructed by its careful unraveling of the strands that combine to form a judgment in favor of one or another policy.

I shall use my limited time to discuss two related points. The first is suggested primarily by the papers on the official reports and has to do with the significance of the recent rescue operation for sterling. The second is far more important and applies to all three reports and all three papers. It has to do with the political implications of the alternative monetary arrangements—a topic that receives almost no explicit discussion in any of the documents though, to my mind, it is the major consideration that renders the official proposals and two of the four alternatives considered by the Group of Thirty-two unacceptable to a believer in representative government and political democracy.

One respect in which governmental or intergovernmental agencies are unquestionably preeminent is their ability to snatch verbal victory from actual defeat. Agencies seldom if ever make mistakes; when bad things happen, it is because of the extraordinarily adverse forces against which the agency had to contend, and only the skillful use of the limited powers which the agency had prevented things from being still worse.

The comments by Governor Daane and Dr. Polak and by various official spokesmen on the recent rescue operation for sterling are of this genre. All point to the rescue operation with pride, as evidence of the strength and effectiveness of present arrangements, of the efficiency of the central bank cooperation that has been painstakingly built up over the years. It is indeed a tribute to the dedicated men who stayed up all night to make the panic phone calls, to the technical wonders of long-distance communications, to the central bankers in the capitals of the world, and to the ingenuity of their deputies that a fund which could be described as totaling $3 billion could be raised so rapidly.

Yet surely, by any disinterested evaluation, the need for such *ad hoc* action on such a scale for one of the two major currencies of the world is a symptom of weakness, not of strength. On many an occasion in American financial history, attempts have been made to organize a corresponding consortium of domestic banks to save a bank or group of banks endangered by a run; the attempt has often succeeded; it has often also failed—most notably when the Bank of the United States was closed on December 11, 1930; but never, to my knowledge, has the need for such action been regarded in any other light than as dramatic evidence of a basic weakness in the financial structure.

This time, the *ad hoc* action has succeeded—at least for the time being. But it has done nothing to remove the conditions which gave rise to the crisis; and there is no assurance that it has done more than postpone devaluation or some equally drastic alteration in the international position of sterling. Going beyond Britain, it has surely done nothing to prevent a similar emergency from arising. On the contrary, it may have made it more likely by encouraging governments to postpone adjustments because of the knowledge that if worst comes to worst they can count on rescue funds. And it has surely done nothing to assure that such an *ad hoc* operation will succeed next time, and the next time, and the next time. On the contrary. One of these days it is bound to fail, and, when it does, the resulting damage will be all the greater.

The present arrangements have, I must in candor confess, lasted longer, worked better, and produced fewer crises than I expected in advance. Differential degrees of inflation combined with differential governmental interventions into private international transactions buttressed by the tailoring of governmental international transactions to balance-of-payments needs have provided an adjustment mechanism with capacity to accommodate substantial swings. However, the mechanism is clumsy, has undesirable effects on resource allocation, and restricts international trade. In addition, I continue to doubt that it has sufficient resilience to last very long without occasional major readjustments of exchange rates. And it certainly offers no guarantee against liquidity crises.

The truth is that the Emperor is naked.

The official reports, and even the report of the Group of Thirty-two, only hint indirectly at what seems to me a major consideration in choosing among alternative international monetary arrangements; namely, what they imply about who shall have power over internal economic policy. The British crisis illustrates this issue dramatically. I happen to disagree with the particular policies that the newly elected Labour government apparently wishes to follow. I happen to believe that the policy changes that will be imposed on Britain by the central bankers as the price of the rescue of sterling may well be better for Britain itself. Yet I find myself in complete sympathy with those Labour supporters who regard it as nearly intolerable that the "gnomes in Zurich" should have a veto power over internal British economic policy.

Insofar as this issue is referred to in the various reports under discussion, it is in terms of the need for "multilateral surveillance" or the need for "international monetary cooperation" or in terms of the danger that "nationalistic monetary policies" will interfere with "the free play of international competition." It is implied that the defenders of the present system or of any of the other systems involving fixed exchange rates are "internationalist," whereas the proponents of flexible exchange rates are "nationalist."

This misconceives the issue. The key issue is not between "nationalism" and "internationalism." It is rather whether, without an explicit political decision, a country should, in effect, give extensive power over its economic policy to specific governmental officials of other countries, in whose selection its own people have no say, direct or indirect. It is not the announced intention

of our present arrangements or of any of the various proposals for centralized reserve and multiple currency reserve systems to delegate significant political power over internal economic policy to foreign central bankers or officials of an international agency. But that is unquestionably the effect. "Multilateral surveillance" and "strengthening of the arrangements for international monetary cooperation" are simply euphemisms for such a state of affairs. The British case makes that clear and so does our own experience over the past five or six years.

From this point of view, the relevant contrast is not between fixed and flexible exchange rates. It is between systems whose operation depends on explicit management by a collection of national central bankers or officials of an international central bank and systems that do not. Of the four ideal types considered by the Group of Thirty-two, the semiautomatic gold standard with fixed exchange rates and the flexible exchange rate system are of the same political type. The systems involving centralization of international reserves and multiple currency reserves are of the other type and so also are systems involving flexible exchange rates managed by an international agency.

Montagu Norman, the famous British central banker of the 1920's, was quite clear about the issue. To judge from the memoirs of Émile Moreau, the French central banker of the same era as well as from Henry Clay's biography, Norman was contemptuous alike of the vulgar masses and the monied classes. He envisaged a group of enlightened central bankers running the economic world as it should be run, largely free from domestic political control and powerful enough to dominate the private monied group. Though he never expressed it that way, his aim was a benevolent dictatorship by a technically skilled and disinterested oligarchy of central bankers.

Though today, also, no one expresses it in this way, in fact the central political issue is how much power should be given to such an oligarchy. I have great respect for the skill and competence of the central bankers and international money men, admiration for their devotion to their tasks, and great confidence in their personal disinterestedness. And, as it happens, I personally would very likely agree to a far greater extent with them about desirable economic policies than with the men whom the American political system has currently put in charge of governmental economic policy. Yet, delegating great economic power to the central bankers seems to me completely alien to our political system and to our tradition of representative government and ultimate responsibility of officials to the electorate.

However, if this is to be done, it should be done explicitly and through an avowedly political decision, not allowed to occur by inadvertence. Otherwise the result will be a fair-weather system that will collapse when it comes under real pressure. Britain is currently free, as we are and have been, to disregard the advice of the existing consortium. So long as the monetary arrangements are not part of a broader political structure, each case will be judged separately, and whenever the costs of compliance get very high, the system will collapse. It will smooth over minor difficulties, at the cost of a major crisis. What happened in 1931 to the system Norman so carefully constructed in the 1920's is a striking object lesson.

This political aspect of the monetary arrangements is the ghost that haunts the discussion. It needs to be brought out into the open if the American people are to make an enlightened choice and if any system which does develop is to have a real chance to work properly.

EDWARD M. BERNSTEIN: Although Professor Friedman's statement is not intended as a paper to be discussed at length, it raises fundamental questions about the usefulness of international monetary cooperation.

Mr. Friedman expects the international monetary system to break down because he does not believe in the efficacy of international cooperation. I predict a happy surprise for him. Mr. Friedman is so sure that every effort by the monetary authorities to deal in a rational way with monetary problems is doomed to failure that he is surprised when the situation does not deteriorate from crisis to disaster. I have known Mr. Friedman a long time and I find this pessimism about the Treasury, about the Federal Reserve, and about the International Monetary Fund is part of the dogma of his laissez faire faith. It is not based on an adequate analysis of the problems and policies of these institutions.

Mr. Friedman and I were colleagues at the Treasury in 1942 when he was making a study of the inflation problem. He was convinced that the United States was destined to have a frightening degree of wartime inflation. The actual experience was quite different. In his *Monetary History of the United States,* he reports on the behavior of prices in time of war: "Somewhat surprisingly, our entry in the [first World] War brought, if anything, a slowing down of the pace of inflation rather than the speeding up one might a priori expect. And perhaps equally surprising, the end of wartime deficit financing saw a speeding up of the pace of inflation. Apparently the pattern was more than an accident, since it recurred in World War II." But this is Friedman reporting history; it was not Friedman analyzing the prospects for wartime inflation.

Now Friedman is surprised that the international monetary system has not broken down, but he seems confident that it will. Admittedly, war and postwar developments placed a great strain on the gold standard system in the past. All belligerents and neutrals experienced war and even greater postwar inflation. Because the inflation was uneven, the competitive position of some large trading countries was weakened. To restore the historical gold parity of their currencies, they undertook severe deflation. Furthermore, the gold standard exhausted the money-creating power of countries in which the money supply was closely linked to gold reserves. This is the famous postwar shortage of gold of the 1920's. Thus, every postwar period (until the present) generated centers of deflation in the great trading countries and ultimately the economic system did collapse in a wave of worldwide deflation.

But this did not happen after the second World War. In part this is because the International Monetary Fund embodied a new principle—that gold parities should be adjusted to the international economic position of the country rather than the other way about. Perhaps more important, the firm tie between gold (and foreign exchange) reserves and the supply of money was sev-

ered, either formally or in practice. Finally, there has been a high degree of cooperation among central banks, through the Fund and outside the Fund, in the provision of reserve credits and in the coordination of monetary policy. There is closer cooperation among central banks today than there was between the Federal Reserve Bank of Chicago and the Federal Reserve Bank of New York in 1933. During the recent sterling crisis, the central banks and the Bank for International Settlements provided credits of $3 billion in addition to a drawing of $1 billion by the United Kingdom from the Fund. In 1933, the Federal Reserve Bank of Chicago refused a credit of $150 million to the Federal Reserve Bank of New York, even though the gold was needed to keep the reserves above the legal minimum.

I predict that Mr. Friedman is going to be pleasantly surprised by the survival power of the present international monetary system. As noted above, there have been basic changes in the old gold standard, starting with the International Monetary Fund. Furthermore, the area and the extent of cooperation among central banks have been greatly expanded in recent years, notably through some of the new techniques introduced by Robert Roosa, the retiring Under Secretary of the Treasury for Monetary Affairs. The international monetary system is still in the process of evolution, and I think that this evolution will keep pace with the emergence of new international monetary problems and will, in fact, anticipate them.

Having said this regarding Milton Friedman's comments on the papers of Jacques Polak, Dewey Daane, and Fritz Machlup, I should like to present my comments on their reports. There is a problem of providing for the orderly growth of monetary reserves. This does not assume a mechanical link between the amount of world trade and investment and the amount of monetary reserves. But in a world in which trade and investment are growing rapidly, in which measures restrictive of international trade or of domestic economic activity cannot be applied with the abandon of the past, and in which short-term capital movements are as likely to be of a disturbing as of a stabilizing character, countries need growing reserves to enable them to meet payments deficits while surer but slower corrective measures are taken to restore their payments position.

The increment of monetary gold in the reserves of all countries outside the Communist bloc averaged $700 million a year from 1961 to 1963. It was substantially less in 1964 and it may decline further in 1965, now that the large gold sales of the Soviet Union have stopped. Foreign official holdings of U.S. dollar reserves increased by an average of $700 million a year from 1961 to 1963. To the end of October, 1964, however, such holdings actually decreased by nearly $200 million, although they may have risen during the sterling crisis of November. The U.S. balance of payments is definitely stronger and it is unlikely to provide foreign exchange reserves for other countries on a large scale in the future. One does not have to hold a mechanistic view of the relation of reserves to world trade and investment to conclude that it is desirable to devise means for supplementing the growth of gold and foreign exchange reserves in the future. Furthermore, too much dependence is being placed on the limited stock of monetary gold as the ultimate reserve asset in the present international monetary system.

The International Monetary Fund is the most important source of supplementary reserves at present. Its gold and currency holdings are available for use by its members on the basis of the quotas agreed with them. The Fund has already engaged in exchange transactions of over $9 billion in the eighteen years in which it has been operating. Much can be done to strengthen the role of the Fund as a reserve center. First, quotas can be increased periodically, as they will be this year. Second, access to the use of Fund resources can be made less restrictive, at least for the first credit tranche. Third, the liquidity of the Fund can be improved by raising the quotas of the large industrial countries and by borrowing from them—a practice just begun and which should become a normal feature of Fund operations. Fourth, more can be done to meet the special reserve needs of the underdeveloped countries in the form of compensatory credits when their export receipts fall off.

This would improve the facilities for reserve credit. It would not provide countries with reserves of their own—a matter of special interest to the United States and other large industrial countries. Nor would it relieve the gold reserve stringency which is becoming much greater. This problem of owned reserves, as contrasted with borrowed reserves, has been given a great deal of attention by the Group of Ten. A Committee has been formed to explore the creation of new reserve assets to supplement gold—the ultimate reserve asset. Dewey Daane has pointed out that the supergold tranche of members of the Fund—a net creditor position in excess of one-fourth of their quotas—is equivalent to owned reserves. That is so, because a country can draw on the supergold tranche without any obligation to repay the Fund.

Unfortunately, it is not possible to provide the large industrial countries with a significant increment of reserves through the supergold tranche. This is essentially a matter of the arithmetic of the world economy. Of the total quotas of all members of the Fund, the large industrial countries have $10.1 billion, other high-income countries have $1.6 billion, and the less developed countries have $3.2 billion. If all of the underdeveloped countries were to maintain a net debtor position equal to one-half of their aggregate quotas—and this would mean that most of them are continually in debt to the Fund—it would result in supergold tranches of $1.6 billion divided among all the large industrial countries. At the end of October, 1964, the outstanding net drawings of the underdeveloped countries was $1.3 billion. Of course, as quotas are increased, the amount of the supergold tranche that can be accumulated by the large industrial countries (excluding their own drawings on the Fund) can be larger. But it will never amount to much as a source of owned reserves.

Following the suggestion of Professor Triffin, but for some reason skirting his proposal, the Fund staff is now thinking in terms of creating reserves by so-called "investments." In my opinion, this is wholly impractical. An institution with 102 members cannot be given the power to create reserves. This is not because it would create too much reserves—the distribution of voting power would prevent that—but because it would create a source of conflict on reserves that would divide the Fund. The rules established at Bretton Woods are the right rules for the Fund. First, the Fund should be passive, providing reserve credit only at the request of members and only within quota limits,

except as these limits are waived under terms and conditions prescribed by the Fund. This should not prevent the Fund from being more liberal in its attitude on the use of its resources. Second, each member of the Fund has a limited obligation to provide resources to the Fund. This limit is determined by its subscription to the capital of the Fund (its quota) and by such credits it undertakes to provide through specific or general arrangements the Fund makes to borrow additional resources.

My own view is that the best way to provide for a supplementary growth of reserves and to diminish the excessive dependence on gold is through a composite standard of gold and reserve units. The reserve units would be created by the Group of Ten by depositing their own currencies with the International Monetary Fund, in agreed amounts, receiving in return an equivalent credit in reserve units. Thus, the reserve unit, equal to one gold dollar, would be comprised of the currencies of all of the large industrial countries. Once the system has been fully established, all of the participants would undertake to hold reserve units up to a stated proportion of their holdings of gold—say, one-half as much in reserve units as in gold—and would convert their currencies in transactions with each other in this ratio of two-thirds gold and one-third reserve units. Gold transactions of participating countries with the rest of the world would be on joint account, in much the same way as the gold pool is now operated.

I do not regard such a system as diminishing the role of dollars in world trade and investment or as a form of reserves. Participating countries that hold dollars would undertake not to convert them into gold until the increase of reserve units had reached the stage where they hold the prescribed ratio in reserve holdings—one-half as much reserve units as gold. The dollar has been successful in competing with gold as a form of reserves because it is a liquid asset of the highest security that yields a return of over 3.5 percent or more. I can see no reason for assuming that its competitive position will not become even stronger relative to a composite standard of gold and reserve units. Dewey Daane's criticism seems to be directed toward a form of this proposal put forward by France. It is by no means the same as my own proposal.

The composite gold standard I have proposed is a natural outgrowth of the system of swaps that the United States and other large industrial countries now have with each other which amount to about $5 billion (in both directions). These swaps are now available on a bilateral basis. Not all of them can always be used, because some of the participating countries may have payments difficulties and because even the currencies of some surplus countries cannot be used in exchange market operations—although they can be used in reserve settlements with such countries. Furthermore, the swaps are short-term credits that must be reversed in periods of three months to a year. What I am proposing is the simultaneous activation of these swaps by depositing the currencies with the International Monetary Fund and designating them as reserve units. Thus, all of the currencies in the swaps would be fully utilizable for international settlements. Furthermore, instead of the uncertainty regarding conversion of currencies into gold and the need to persuade

countries to take special nonmarketable securities denominated in dollars or in foreign currencies, all participants would convert their currencies partly in gold and partly in reserve units. This seems to me an efficient development of the arrangements initiated by the Treasury and the Federal Reserve.

Mr. Polak has objected that such a plan for the creation of reserve units is unethical. In fact, the problem that confronts us is not an ethical but a pragmatic one. The composite gold standard is a method of providing for an orderly growth of reserves and of diminishing the excessive dependence of the international monetary system on gold. About 86 percent of the gold reserves of the world are held by the large industrial countries. They are the countries that are responsive to the growth of reserves. The underdeveloped countries hold limited reserves in foreign exchange and even less in gold because they cannot afford to invest resources in this form. Far from prejudicing their interests, the strengthening of the international monetary system is the best assurance they have that foreign investment and foreign aid can be increased to meet their needs.

The composite gold standard does not provide for the creation of real resources for the benefit of the large industrial countries, as Mr. Polak charges. These reserves are not intended to increase their imports relative to their exports. The reserve units are created to be held. It is true that countries would be able to finance deficits in their balance of payments for a somewhat longer period; but they would be expected to restore their reserve position as soon as possible. I am against linking the creation of reserves with the provision of real resources, either for the industrial countries or the underdeveloped countries. This is a point quite properly made by Professor Machlup.

I dispute the view that the creation of reserves by the Fund and used by the World Bank for development loans would be beneficial to the underdeveloped countries. The loans would not increase their reserves, because they would be spent. Nor would such loans increase the aggregate real resources for development. The reserves that could be created through the Fund's investment operations would amount to about $800 million a year—a small fraction of what is now available from grants and credits. The United States argues, with more or less conviction, that the adverse effect of its aid on the balance of payments is negligible. With the system proposed by Jacques Polak it would be difficult to contest the view that if the United States gave less aid it would be able to earn more of the reserves created by the Fund's investment operations and channeled through the World Bank. I think that countries with a payments deficit—and they include two of the largest donors—might feel impelled to reduce their aid in order to give it by way of a balance-of-payments surplus.

I am in favor of providing for the capital needs and the reserve needs of the underdeveloped countries. I believe their capital needs can be best met by increasing the amount of aid now given to the underdeveloped countries, not only by the United States, but by other high-income countries. I believe that the reserve needs of the underdeveloped countries can best be met by providing better facilities for reserve credit through the Fund, including an improved system of compensatory credits to offset fluctuations in export re-

ceipts. The evolution of the international monetary system requires the creation of a new reserve asset. No good and much harm can come of linking the creation of this reserve asset with the provision of investment capital for the underdeveloped countries.

MILTON GILBERT: The report of the Group of Ten and that of the Bellagio group have at least one thing in common: neither of them presents an agreed view on what the changes in the international monetary system should be. The reason for this is that economists—since economists were responsible for both reports—do not agree on what the problems of the system are. Inevitably, they do not agree on the solutions. It does no good to suggest, as the Bellagio report does, that there is some useful consensus in the fact that all participants want some change or other in the system, when these changes go in opposite directions. In this situation everyone's satisfaction is apt to be maximized by no change at all. While modifications of the arrangements in the system are none the less likely, I believe they will come through *ad hoc* decisions to meet practical problems rather than through acceptance of this or that idealized model. As the system has evolved in this way up to the present, I am not aghast that it should continue to do so in the future.

The question I want to raise here, however, is what we are to make of the wide diversity of views among economists about the international monetary system. In the Bellagio report, the views of the thirty-two participants have been fitted into four groups (with variants), each of which represents a quite different conception of the model of the system. This is rather arbitrary, of course; there might just as easily have been ten groups. One may ask, however, how economists line up behind these models.

The basic split that one must start with, it seems to me, is into two groups: those who believe in the flexible use of monetary and fiscal policy to help in achieving the objectives of full employment, price stability, and external balance, and those who distrust government discretion, and would therefore limit policy to rather inflexible rules. There is really no reconciliation possible between these two viewpoints, and we must leave it that each group stays within its own world. Or I may say, perhaps, we stay in this world and they keep trying to create their own.

How do economists divide on this question? If I think of those I know who are against discretionary use of policy instruments—in academic, government, or business positions—I can count them up on my fingers. Yet, in the classification into four groups in the Bellagio report, they account for the semiautomatic gold standard system and part of the flexible exchange rates group; that is, flexible rates with no official intervention in the market. This seems to me to be gross overrepresentation.

Let us take the "semiautomatic gold standard." As you know, the idea here is that the authorities should hold only gold in official reserves and only intervene in the market with the resources provided by gold. What is more, domestic liquidity is to take the brunt of changes in gold reserves. By taking the element of truth in the rules of the game to such an extreme, they want to

give the external balance an overriding priority among objectives. As an advocate of this system says in an appendix note, "this may sound a trifle old-fashioned."

Of the thirty-two Bellagio economists, two support this system. That is 6 percent of the group; but they get 25 percent of the plans analyzed. I suspect, however, that if you canvassed the whole membership of the American Economic Association you might not find another one—I am sure in any case that they would make up only a fraction of 1 percent of economists. You see, therefore, that a random sample of economists would probably give a better reflection of contemporary thinking than the Bellagio thirty-two, since the probability of an advocate of this system turning up in the sample would be almost nil.

At the other extreme, there is the system of pure flexible exchange rates; that is, no official intervention in the market. This system also gives top priority to external balance, though the impact of external changes on the domestic economy comes not through domestic liquidity but through the volume of real flows of goods, services, and capital. The report does not say how many of the participants support this system; perhaps there was just a place of honor left for Milton Friedman. I cannot imagine, however, that it is a plan with great appeal to members of our profession. As I agree with the critical comments on this idea in the report, I may just stress one point about flexible rates.

In terms of what the operation of the system would be like, it is very misleading to classify managed flexible rates (or a system with wider margins) as a variation of pure flexible rates. Once management of the market is allowed for, flexible rates would be much more a variation of pegged rates. The reason is that, before allowing the rate to move, the manager would have to face all the questions that are faced now. He must consider, for example, the importance of the currency involved; whether using some other policy instrument would be more appropriate; how confidence would be affected; what would happen in the forward market; and how real losses or gains would be imposed on various groups in the economy. It is not obvious to me that the advocates of managed flexible rates would have strong enough nerves to move the rate around very freely if they had the responsibility of doing so in the light of questions such as these.

The other two plans set out in the report are called centralization of reserves and multiple-currency reserves, either of which could mean a wide variety of things. I believe that such catchphrases give us very little guidance about what the next step should be. In this business one really has to read the fine print. I am sure also that progress cannot be made by trying to do things behind the veil of money that the countries do not want to do in real terms.

These remarks are relevant to the system outlined in the report as "centralization of reserves." In my opinion, we are not going to get a system like that—at least not before 1984. I do not think that any country of the Ten would even begin a negotiation with such terms of reference. And they would not be rejecting it because they do not understand it, but rather because they do understand it.

Let me make one final point. Underlying much of the discussion of this subject there is an assumption that some functional relationship exists between the total volume of official reserves and the volume of international transactions—some kind of international quantity theory of money. This idea must come either by analogy with the quantity theory of money for a particular country or by simple observation of past trends. Neither of these can be adequate for us as economists. We must have a theoretical demonstration that establishes the relationship. And we do not have it. For this reason most of the talk about the adequacy or inadequacy of reserves, or of their required growth rates, or whether increases in reserves are haphazard, is not convincing.

To illustrate, if the money supply of a given country were to double, we would all believe that there would be a whacking good rise in prices. If, on the other hand, the reserves of the Group of Ten were to double, we would be at a loss to say what the consequence would be. It is possible, indeed, that not much of anything would happen. And to the extent that something did happen, would it not be appropriate to say that economic affairs were not being particularly well managed?

Neither the Group of Ten nor the Bellagio reports are quite explicit on this quantity theory assumption but it is not difficult to find frequent evidence of it. In the Group of Ten report there is a statement to the effect that at the present time there is no overall shortage of reserves. I once suggested to several of the Deputies, not entirely joking, that this statement should be amended to read that there was not at the present time an overall shortage of reserves—especially for countries in surplus.

I do not mean to imply by all this that the reserves which a country has do not matter or that a country cannot be short of reserves. But it is a matter of what individual countries have and need rather than a matter of the total, and involves different considerations from those we would use in judging the domestic money supply. To see this one has only to consider the role of gold in the international financial system. Gold is a very liquid asset, but it only flows in relatively small amounts. If too much of it moves, there is an adverse effect on confidence. To a lesser extent the same is true of foreign exchange reserves. During the entire period of large deficit in Italy, lasting about fifteen months, only $200 million of gold was used by the authorities, while there was large use of Euro-dollar facilities, the IMF, and central bank assistance.

It is evident that reserves and borrowing facilities must be adequate to maintain orderly conditions in the market, and at the same time they must be limited to keep any given country from continuously absorbing real resources from its neighbor. This is the inherent dilemma of the international monetary system, which can never be solved by a simple formula or to everybody's satisfaction.

DOMESTIC IMPLICATIONS OF THE EVOLVING INTERNATIONAL MONETARY MECHANISM

DOMESTIC AND EXTERNAL EQUILIBRIUM: EUROPEAN OBJECTIVES AND POLICIES

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The subject of this session is the "domestic implications of the evolving international monetary mechanism." The word "evolving" seems rather unnecessary here, because the recent evolution of the international monetary system, though providing for larger and more easily available credit facilities, has not changed the fundamental problem. In essence, the system is one in which the national currencies used to transact international business are normally traded against each other in the exchange markets at fixed rates. While the authorities have considerable leeway to support the rate, by allowing fluctuations in reserves and by lending or borrowing from each other, they are continually forced to give attention to securing external equilibrium. Hence, our question really is the implications for domestic objectives and use of policy instruments of the need to aim simultaneously at external balance. To see both sides of the matter, however, we should also ask the complementary question of what are the implications for balance-of-payments policy of domestic economic objectives.

First, a few words about policy objectives in Europe. These are, universally, the familiar trio of full employment, price stability, and external equilibrium. Among these objectives, full employment has a very high priority and tends to be defined with really minimum margins of unemployment. One could not say, however, that it has an overriding priority, since this objective has been relaxed from time to time when needed to assure other aims. Price stability is a more fuzzy policy aim, partly because the variety of price indexes usually show divergent results and partly because of a resigned acceptance that some rise in the cost-of-living index is difficult to avoid in a full employment economy. External equilibrium is also a rather nebulous objective, because the concept itself is not clear cut. In fact, it is more meaningful to say that the operating objective is a fixed exchange parity, with, at least, maintenance of gold and foreign exchange reserves.

There is no doubt that fixed exchange rates have a high priority. In recent years, there have been no cases of conflict between fixed rates

and full employment—except marginally in Great Britain, where the stability of sterling was given priority. Faced with a conflict between pressure on resources and an external surplus, on the other hand, many countries have accepted some price inflation rather than revaluation when the external surplus was large, but acted fairly strongly against inflation when confronted with a moderate surplus or a deficit.

As we are to discuss Europe's policy experience of a given time and setting, there are two aspects of the recent economic background that must be stressed:

1. The economic climate of Europe has been of a decidedly post-Keynesian character. For one thing, public investment demand has been high and constantly growing, so that there has been little tendency for *ex ante* savings to outrun investment. For another thing, wages not only do not fall but persistently rise in wage round after wage round. These two forces, usually augmented by an external surplus, have meant a level of overall demand that has pushed against the capacity of the economy.

2. On the external side, Europe has been sheltered in a sense by the U.S. balance-of-payments deficit. While this has contributed difficulties of its own, including that of inflationary pressure, it has made external deficits rare and the result of fairly obvious excesses. This has simplified the task of full employment policy but raised instead the problem of curbing inflation against a balance-of-payments surplus.

We may now consider recent developments in the countries where internal inflation was accompanied by an external deficit.

Italy. In the Italian inflation of 1963 the authorities had no conflict between domestic and external stabilization policy; there were both rising prices and an external deficit.

After advancing rapidly from 1959 to 1961, the economy leveled off in the first half of 1962. With the external accounts apparently safe and reserves ample, the authorities pursued an expansionary policy line, particularly by accelerating public-sector investment. Towards the end of the year, however, there was an unexpected burst of wage increases that was to raise total wages by about 15 percent in 1963. Even though unemployment had been largely absorbed by this time the wage increases were the result primarily of negotiation rather than of free market forces and indeed cannot be understood apart from the political context of the time.

These wage increases promptly produced an inflationary upsurge of prices and imports. This was due not so much to the direct effect of higher costs on prices as to the sheer impact of increased demand. The situation worsened as the year wore on, with the current account deficit for 1963 amounting to $900 million and the cost of living up by

about 8 percent. Confidence in the lira was impaired and a flight of funds added to the problem.

The authorities did not react immediately to these developments. For one thing, the political situation was not one from which a stringent program of restraining measures could readily emerge and, for another, the problem itself came so suddenly that it took time to size it up. Another factor, perhaps, was that the external deficit up to September, 1963, was largely financed by increased net foreign indebtedness of the banks, which had been authorized in November, 1962, to help ease the internal monetary position. However, effective action began to take shape just after the summer and rather quickly led to a dramatic change in the situation.

The initiative in the stabilization effort fell to the monetary authorities and, indeed, monetary policy was the decisive factor in the program. Various steps were taken to slow down the expansion of credit from the supply side, an early measure being to require the banks to reduce their net borrowing from abroad, which meant that the external deficit itself contributed to tightening the monetary situation. A very forceful tool, however, was the direct control the authorities had over all sizable extensions of credit by financial institutions and over new security issues, which was used with telling effect to limit investment from the demand side. In the first nine months of 1963 bank credit to the private sector had increased by nearly 15 percent, while in the same period a year later it declined by about 5 percent. It is hard to imagine what level of interest rates and what lapse of time would have been needed under Italian conditions to have produced a comparable result by more classic techniques of monetary restraint.

Credit restriction, reinforced later by measures of fiscal restraint, brought quick results. Little more than six months elapsed before the situation had decisively changed. The peak month for the external deficit was actually February, 1964; after April increasing surpluses were shown on both current and capital account. For the year 1964 imports will be no higher than the year before, while exports will have increased by over 15 percent. Thus, it is fortunate that the wage increase did not entail a strong cost-push on prices, which could have impaired the competitive position of industry.

Though wages were the key factor in the Italian difficulties, the authorities were unable to use wage policy to help manage the situation; instead an easier situation in the labor market and in production was required to dampen the inflationary flames. This worked so quickly on demand and the balance of payments that the first mild steps to ease restraint were started before the middle of 1964.

The Netherlands. The inflationary difficulties which the Netherlands

has experienced in the past year have a superficial resemblance to those of Italy; a sudden and substantial rise in wages was followed by rising prices and a current account deficit. However, the underlying situation of the two countries was much different.

As was the case in several other countries, there was a definite easing of the boom in the Netherlands in 1962 and the restraints on bank credit were taken off early in 1963, though without any positive stimulating measures being introduced. However, expansionary forces gathered momentum in 1963, with exports being unexpectedly strong because of inflation in some other countries and private investment again accelerating.

Here was a new demand boom coming on top of an economy already operating at full capacity. There was, inevitably, tightness on the labor market. At the end of the summer the dam burst when several firms offered wages above the scales fixed in the general wage agreement. As a result of this break in wage controls, a new wage agreement had to be negotiated in the autumn which provided for increases in pay scales averaging about 15 percent. Needless to say, no economy at full employment can take this kind of stimulus without inflation, which was reflected in rising prices and a current account deficit.

In contrast to the Italian case, the eruption of wages in the Netherands must be attributed essentially to the market forces generated by xcess demand. The Netherlands has well-developed wage-policy mahinery, which operates with the full cooperation of labor and employr organizations, but events demonstrated that such machinery cannot tand up to excessive demand pressure. Nonetheless, the fact of wage olicy had an important bearing on the market reaction to the nflationary incident. There was no fear of a continuing wage-price spial, no suspicion that the guilder was threatened, and no flight of capial. Hence, the remedial action by the authorities could aim at slowing own the boom rather than bringing it to an abrupt halt.

As usual, the authorities were prompt in taking steps to bring order nto the situation. In the monetary field, credit ceilings were reimposed n the banks, the discount rate was raised, consumer credit regulations ere tightened, and a government loan in excess of needs was issued to rain funds from the economy. In addition, taxes on cigarettes and soline were raised to reduce consumer demand, public investment lans were reduced, and investment allowances and advanced amortition were suspended to dampen private investment. As a result of ese measures, the inflationary symptoms eased in the course of 1964. But here we must look at a further development which has somenes been conspicuous, sometimes just below the surface in much of ropean experience since convertibility. Both the external deficit and

measures taken by the authorities led to a growing tightness of the internal monetary situation and to a sharp rise in interest rates. As was anticipated, the banks were able to ease their liquidity position by repatriating short-term assets from abroad—which, in fact, served to finance the balance-of-payments deficit. To prevent this movement from going too far, the central bank ruled in July that the banks should not allow their foreign assets to fall significantly below their liabilities.

The other development was less anticipated: as the yields on long-term securities got up towards 6 percent, they proved to be very attractive to foreign investors, who bought in such volume as to lower the rate below 5.5 percent. This capital import was not to the liking of the authorities, who felt rather that the Netherlands should be a capital-exporting country.

It is evident that this responsiveness of funds from abroad to internal monetary restraint imposes a definite limitation on the use of monetary policy for strictly domestic purposes.

We now turn to a few cases in which domestic inflationary pressures were accompanied by an external surplus. This was the typical situation on the Continent up to 1963.

France. In France two main influences led to the development of inflationary tensions. One was the ambitious growth target of 5.5 percent, which was strongly supported by fiscal means and credit facilities. The other was the external surplus, which fed internal liquidity and contributed to the build-up of demand forces. The result was a tight labor situation and evidence of inflation in wages and prices in 1962. However, the pressures were greatly intensified by the repatriation of close to a million persons after the end of the war in Algeria. Their added buying power gave demand a decided jolt, particularly felt on the housing market and on foods and light consumer goods. Thus, by the beginning of 1963, inflation was a quite active force, which in the course of the year pushed up prices by 6 percent and substantially reduced the current account surplus.

While some measures were taken in 1962, it was in early 1963 that the authorities turned decisively to a policy of credit restraint. Without increasing the discount rate at this time, they imposed a direct ceiling of 12 percent on the growth of bank credit for the year ahead. In addition, the banks' compulsory assets ratio (*coefficient de trésorerie*) was further increased from 32 to 36 percent. These compulsory holdings of assets are made up partly of Treasury bills, while the remainder consists mostly of medium-term equipment credits which would otherwise be automatically rediscountable at the Bank of France. Finally, for the first time in about five years, the Treasury launched a long-term loan. These actions initiated a new policy line

aimed at keeping the growth of internal liquidity at about the same pace as real output, regardless of the external surplus.

However, these monetary measures were not quickly effective against the kind of inflationary psychology that had developed and by the late summer of 1963 stronger steps were necessary. A wide range of new stabilization measures was introduced which laid much emphasis on controlling prices and retail margins, but included also action to reduce the budget deficit and to lower the credit ceiling to 10 percent.

In addition, the Bank of France's discount rate was raised in November. It is significant, however, that this step had been preceded by measures which limited the scope for inflows of short-term capital. In April the banks had been prohibited from paying interest on nonresident franc accounts, while in August stricter terms were imposed on foreign borrowing by franc-area residents.

In 1964 the growth of demand and the pressure on prices abated and output leveled off. But wages have continued to rise excessively. Wages policy has up to now been relatively successful only in restraining the growth of wages in the public sector, though an attempt is being made in the Fifth Economic Plan to lay down guidelines for incomes generally. Meanwhile, the weight of policy restraint has necessarily fallen on total demand, backed up by extensive controls on prices. The banks' compulsory assets ratio has been kept at 36 percent, except when eased for seasonal reasons, and bank liquidity has remained tight. In the first three-quarters of 1964 the banks' monetary and quasi-monetary liabilities increased by only 4.8 percent, against 8.9 and 11.8 percent in the same periods of 1963 and 1962. However, the emphasis of restraint is gradually being shifted to the budget.

Germany. After achieving a high degree of domestic and external equilibrium in late 1962 and early 1963, economic stability in Germany was increasingly threatened after the middle of the year. While this was partly due to a renewed advance in fixed investment, the much more important reason was a rising trend of exports stemming from inflation in neighboring countries. Superimposed on the growing export surplus, foreign purchases of German securities began to revive on a large scale. By the last quarter of 1963 the external surplus had swollen to major proportions, carrying a definite inflationary threat for the months ahead.

What to do about it was, however, a nice question. The authorities were not anxious to try monetary restraint in the face of the external surplus, as this course had proved abortive in 1959-60. So, apart from complaining about inflation abroad, they confined their reactions to measures designed to influence the movement of capital. In March, 1964, in order to encourage the banks to export short-term funds, the

Bundesbank began once again to offer them swap facilities at below market rates. In addition, minimum reserve requirements against bank liabilities vis-à-vis nonresidents were raised to maximum levels, and the banks were forbidden to pay interest on foreign time deposits. To discourage foreign purchases of securities, the authorities proposed to introduce a 25 percent withholding tax on dividends of German bonds held by nonresidents. Finally, to encourage the flotation of foreign loans on the domestic capital market, the existing 2.5 percent tax on new security issues was to be abolished.

These measures, as well as the restrictive policies being followed in other countries, fairly quickly brought the external surplus to manageable proportions. Thus, the authorities also found themselves with somewhat more scope for a tightening of credit at home. The first step in this direction was to raise reserve requirements as from August 1, 1964, by 10 percent. A complementary step provided that the rediscount ceiling of any bank would be reduced by the same amount as any increase in its total foreign liabilities. Main reliance in stabilizing the economy is being placed on fiscal policy, which aims at keeping public expenditure in check.

Switzerland. Since 1960 Switzerland's continuing overall external surplus has primarily reflected large capital inflows, which contributed to the strong domestic demand pressures and to a big current account deficit. In dealing with this situation, the authorities for some time followed largely along their traditional line of policy. Interest rates were kept low in an effort to curb net inflows of capital as well as to resist cost inflation via higher mortgage rates, which are closely linked with rents and farm prices. Moreover, the Confederation continued to realize cash budget surpluses, which were sterilized mostly through the acquisition of foreign assets. In addition, foreign issues were permitted on the capital market insofar as these did not place undue upward pressure on interest rates. Finally, the authorities pursued a liberal policy with respect to the entry of foreign labor.

It soon became clear, however, that further measures were needed. Already in August, 1960, in order to limit the entry of foreign funds, the authorities concluded a gentleman's agreement with the banks whereby Swiss franc deposits by nonresidents could be accepted only at three months' notice or more, without interest, and subject to a commission charge if at less than six months' notice. In April, 1962, by which time a major investment boom was under way, a new type of domestic credit agreement was brought into force, requiring the banks to restrict the rise in ordinary and mortgage credits to a certain proportion of the increase in 1961 or 1960.

In 1963, as inflationary tensions mounted, the authorities began to

shift their tactics so as to deal more positively with excessive demand. In March they decided to restrict the entry of additional foreign labor, hoping in this way to curb both investment and consumption outlays. Then, in the summer, when interest rates came under some pressure, the National Bank acceded to a rise in the rate on medium-term bank bonds, thus signaling further increases in longer-term interest rates.

The Swiss stabilization efforts reached a climax in March, 1964, with the adoption of legislation designed to increase the government's anti-inflationary powers. While in part this legislation merely made mandatory previous agreements with respect to domestic credit ceilings and the treatment of foreign funds, it went further by providing for these to apply also to nonbank financial institutions. It also led to the introduction of control over all capital issues and to a new ruling that the countervalue of new inflows of foreign funds, if not reinvested abroad, must be placed on special account with the National Bank. Other provisions enabled the government to make most new building subject to prior authorization and to retain the existing restrictions on the intake of foreign labor. Subsequently, in the spring and summer, the discount rate was adjusted upwards to market rates, credit ceilings were tightened and hire-purchase regulations made more restrictive.

Since the middle of 1963 longer-term interest yields have risen by nearly 1 percent. While giving way to this movement, the authorities' policy strategy, with heavy emphasis on selective restraints on investment, has sought to moderate it. For despite the steps taken to safeguard against inflows of foreign capital, the dilemma is that nothing stands in the way of repatriation of the huge volume of Swiss capital held abroad. The authorities recognize that the only obvious way around this difficulty is to obtain greater restraint through fiscal policy, where, as so often, there are political factors to be resolved.

United Kingdom. The United Kingdom is in the midst of balance-of-payments difficulties and, since one cannot anticipate how quickly the measures taken will restore balance, the current situation is difficult to talk about. However, as there have been similar episodes in the past decade, there is point in looking at the United Kingdom's experience over a longer period.

It is evident that the difference in the experience of the United Kingdom from that of the continental countries since, say, 1955 is indicative of quite different underlying conditions. Besides the present, there have been balance-of-payments troubles on three other occasions—in 1955, 1957, and 1961. These did not come from a sudden eruption of wages or highly excessive demand, such as we have seen in continental examples. In essence, it was rather that exports have not been dynamic and were continually outpaced by imports whenever do-

mestic expansion was allowed to gain reasonable momentum. In consequence, a policy of restraint has often been necessary and the average rate of growth has been low. Thus, the U.K. dilemma has been how to get reasonable growth in the face of external weakness.

The problem was, therefore, rather similar to that of the United States since 1958, except that the United Kingdom has not had a high rate of unemployment. Neither has it had the possibility of financing a continuing deficit. The United Kingdom differed also in being prepared to use monetary and fiscal policy with considerable flexibility; but while these instruments produced good short-term results, the longer-term dilemma has persisted.

From the point of view of a static analysis, one may say that the comparative cost levels gave industry on the Continent a competitive edge in international markets that made exports a dynamic factor in expansion, while the United Kingdom had a constant struggle against an inadequate growth of exports. Less obvious, however, is why a basic adjustment, both of the domestic economic structure and the balance-of-payments potential, failed to materialize over so long a period, especially as there was inflation on the Continent. Must one conclude that the mechanism of adjustment does not operate sufficiently in a modern industrial economy, or was there an independent factor which interfered with its functioning?

In our view there was an impediment to the adjustment process inherent in the practice of wage negotiation and arbitration which tended to prevent a basic improvement in the external competitive position, even in periods of demand restraint. Whereas real output over the past ten years increased at a rate of under 3 percent, hourly wage rates rose by about 4.5 percent, and hourly earnings by about 6 percent. Wages increased every year, even in years of recession, when there was significant ease in the labor market.

While it was soon enough evident that this problem could not be managed by the usual policy instruments, the politicians had an understandable reluctance to deal with the matter directly. It is to the credit of Mr. Selwyn Lloyd, Chancellor of the Exchequer at the time of the sterling crisis of 1961, that he had the courage to face this issue and to introduce incomes policy by a pause to wage increases. While he became a political martyr in the cause of effective policy management, the idea of incomes policy could not be brushed aside and has been recently reaffirmed by the new government with a significant agreement signed by labor and industry as well as by the government.

We have discussed the U.K. experience because it shows clearly another important aspect of the need for keeping the wage round consistent with the needs of the general economic situation. It is essential

in such a case of inadequate export growth to understand, as Mr. Lloyd did, what the point of the policy must be; namely, to hold back the tide of the wage round sufficiently so that the normal increase of productivity will lead to a more competitive position of costs and prices. Otherwise, there is the onus of wage policy without the benefit.

We may now come back to the two questions with which we started to see what "implications" of the recent functioning of the economy emerge from European experience. To the extent that they have a rather new look, these implications result from the two forces pointed out in various cases; namely, the continuous rise in wages that seems to be inherent in the present negotiation process and the size and sensitivity of international capital movements that have been in evidence since the return to convertibility.

We should say first that in Europe the aim of the authorities has been to achieve the two goals of full employment and external balance together, without abandoning either one, though with some shift in priorities according to the demands of the situation. With regard to the objective of price stability, however, greater compromise has been necessary. Thus, it is primarily the use of policy instruments that has been changing rather than the conception of the basic objectives themselves.

There seem to us five points that deserve to be emphasized:

1. The first is that there has been growing acceptance of, or groping towards, wages policy as an essential tool of economic management. This has come as a response to the fact of ever rising wages, at a rate not very closely related to the state of demand and not easily subject to control via the usual instruments of monetary and fiscal policy. Wages policy has been found necessary for meeting domestic aims alone as well as for minimizing the conflict between domestic and external objectives. Needless to say, effective wages policy must rest upon appropriate management of total demand.

2. Second, it has been found necessary to recognize, also with respect to domestic policy, that the use of monetary measures has had to be limited and adapted to what is consistent with the external position. In practice, this has meant giving a degree of priority in monetary policy to the external side and to finding ways of managing the rate of credit expansion without relying wholly on interest rates.

3. This limitation on the range over which monetary measures could be used has itself led to more flexible use of fiscal instruments for domestic purposes in cases of conflict between internal and external objectives. Forceful tax measures have been used frequently in this connection, not only to manage overall demand, but to influence specific categories of demand. Equally important, however, has been direct

changes in the volume of public-sector investment as a restraining or stimulating device.

4. Turning to the implications for balance-of-payments adjustment of a conflict of objectives, experience in Europe has shown wide use of specific measures to help correct the external disequilibrium directly, and thereby to relieve the domestic economy of some of the burden of adjustment. We have mentioned, in particular, measures taken to influence the inflow or outflow of capital funds of both domestic and foreign ownership. Other measures that have been used for this purpose include government debt prepayment, government current transactions, and changes in tariff rates.

5. The final implication to which we want to draw attention is, however, that a full reconciliation of objectives has not been found feasible, so that some adaptation of domestic costs and prices to the external position could not be avoided. This, indeed, has been a basic factor in the inflationary tendency in continental Europe in recent years, reluctantly acceded to as inherent in the adjustment process. It is for this reason that we said above that the aim of price stability has not been held as firmly as the other objectives, for it has been generally recognized that to resist all adaptation of costs and prices would mean perpetuating the external imbalance.

To conclude, it has been by inventing and adapting policy instruments that the European countries have sought workable domestic and external equilibrium with a minimum of conflict. While this has at times meant some limitation on the conception of convertibility, it has also meant recognizing that convertibility has its own logic in the use of policy tools for domestic objectives. We have not discussed the liquidity aspect of the international payments system as being outside our subject. The past few years have shown, however, that the difficult problems lie in the use of policy instruments for adjustment purposes and in assessing mutual adjustment responsibilities rather than in mobilizing liquidity in cases of need.

THE EVOLVING INTERNATIONAL MONETARY SYSTEM AND DOMESTIC ECONOMIC POLICY

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I find this an elusive subject for a number of reasons, one of which is the difficulty of defining the international monetary system.

Where does the international monetary system leave off and the domestic monetary system or domestic economic policy begin? We may say that the international monetary system is the system by which the quantities of different kinds of money that people want to hold and the quantities of different kinds of money available are adjusted to each other. If this is the definition, then what we commonly call domestic economic policy is a main component of the system. Even if we confine ourselves to the system by which the supply and demand for foreign currencies are adjusted to each other, domestic policy is a large part of that system. Wasn't the recent tax cut a measure of international monetary policy? Would a description of the present international monetary system have as one of its features that a country in the international and domestic position of the United States in 1964 should cut taxes? I suppose so.

It is difficult to draw a line between the international system and domestic policy because on the one hand there are hardly any important policies that are purely domestic in their effects and on the other hand there are hardly any important policies that are not mainly domestic or national in the responsibility for them and in their motivation. I think a good deal of the problem with which we are concerned here is wrapped up in this fact.

However, in order to conform to the title of our session and to avoid enmeshing myself in a general equilibrium system that I could not handle, I will adopt a certain definition of the international monetary system. I mean by the international monetary system as it now is evolving the arrangements by which some governments or treasuries or central banks extend credit to others for reasons other than the direct return on the investment. The nature of this system consists of the amounts of credit that will be extended, the circumstances, terms, and conditions on which it will be extended, and the institutional arrangements by which all of this is determined. The evolution of the system is the evolution of these characteristics.

I shall return later to the question of the reasons why some governments extend credit to others. The general reason for extending credit is to influence the borrower's behavior in a certain way, presumably in a way that is to the interest of the lender. The amounts, terms, and conditions are set by the lender on his side in a way that he thinks will influence the borrower to behave in a way that the lender prefers. This does not mean that the system operates to the disadvantage of the borrower. The borrower does not have to borrow; he borrows in order to be able to follow a policy that he prefers to any of the options that he would have available if he did not borrow. The borrower may consider that he would be still better off with more credit on longer terms and easier conditions, but the lender will not extend such credit if he thinks the result will be borrower policy that he does not prefer.

This description of the system may suggest that the lender is in the driver's seat, because he decides whether to lend and on what terms. But this is not necessarily the case. The borrower may get closer to the situation he prefers than the lender does. The lender has the power to lend or not. The borrower has the power to choose among a number of policies that have different consequences for the lender, and may confront the lender with alternatives that force him to lend. For example, suppose that the potential lender says to the potential borrower: "I would prefer that you cut your foreign aid programs rather than borrow from me." The potential borrower may reply: "I would prefer to borrow from you, but if you will not lend to me I will devalue rather than cut my foreign aid." If the potential lender believes this and if he prefers to lend rather than see his partner devalue, he will end up extending credit, even though he would have preferred something else.

To describe the system as it exists at any time is difficult. The description will not be found written down in the charter of the International Monetary Fund or anywhere else. The system consists of what the officials of about ten governments will do. They reserve ultimate discretion to themselves, do not tell anyone how they will use it, and perhaps cannot tell anyone how they will use it without giving up their discretion.

I do not think we can even say how much international credit is available under the present system. We know how much credit governments have said they will or may make available. But the amount of credit that governments can extend in their own currencies is certainly much larger; as far as I can see it is legally and technically unlimited, and we do not know how far governments will go.

Neither do we know much about the conditions on which credit will be extended. We all remember the flow of travelers returning from Zurich who told us that the Europeans would not give us more credit un-

less the U.S. balanced its government budget. Apparently this was a wrong description of the system, because we did not balance the budget and we continued to receive credit.

Perhaps something more can be learned about the system from its actual behavior, but even this gives only weak clues. Recently, when the pound was under severe pressure, in danger of devaluation, and after British bank rate had been raised from 5 percent to 7 percent, other countries announced a $3 billion line of credit for Britain. Does this mean that the system will provide credit whenever the pound is under severe pressure, or only after bank rate has been raised to 7 percent? We do not know which of the actions taken are required by the system, which are taken because they are believed to be required by the system, and which are taken independently.

Some day a retired treasury official may tell us what went on in the Paris and Basle Clubs and we will know what the system was like for a time. Until then, description of the system by an outsider must be speculative and impressionistic.

With these warnings, I would suggest that the evolution of the postwar international monetary system can be divided into three stages. The first was the Bretton Woods period. The second, beginning in about 1958 and beginning to fade out in about 1961, may be called the period of hard money. The third, in which we still are, I would call the period of credit extension.

By the Bretton Woods period I mean not the period of the IMF, which of course still continues, but the period of a certain philosophy of the system. This philosophy has hardly been applied in practice because during most of the period when it was the standard philosophy the real world was dominated by recognized exceptions from it—the postwar transition and the special problems of less developed countries. However, the Bretton Woods way of thinking is a convenient point of departure and may yet acquire practical importance.

As I see it, the Bretton Woods philosophy implied a rather formal and limited extension of credit. Deficit countries would be given credit to avoid the necessity for temporary restrictive measures, except with respect to capital flows, or for short-run changes of exchange rates. But a large and long-continued flow of credit was not contemplated. If in the time permitted by moderate, temporary credits, equilibrium was not achieved, exchange rates were to be adjusted. The object of the enterprise was to prevent a spiral of trade restrictions and deflation. But international extension of credit was not to bear the whole burden of achieving this object. The limited extension of credit was to be supplemented by control of capital movements, anti-inflationary policy and devaluation of the currency.

The system depended on a certain set of preferences on the part of deficit and surplus countries. The potential lenders had to prefer devaluation by the deficit countries to continued large-scale lending; otherwise they could be blackmailed into lending by a threat of the deficit country to devalue. The deficit country had to prefer devaluation to trade restriction. Otherwise one of the basic objectives—a liberal world trading system—would be endangered.

The system exerted discipline against inflationary policy by deficit countries only by ruling out the option of continuing credits. This might confront the deficit country with the choice between anti-inflationary policy and devaluation, and might induce it to choose anti-inflation. In any case, the surplus countries were not obliged to finance the inflation of the deficit country and could limit its spread to them.

When the postwar transition ended—say with the end of exchange controls in Europe in 1957 or 1958—and the time came to live by the Bretton Woods philosophy, it appeared that we did not intend to do that. Exchange rate devaluation was demoted from its role as the standard adjuster of continuing disequilibrium. Restraint of international capital movements was also moved down in the scale of possible corrections until it also became a measure of nearly last resort. With devaluation and capital controls moved down in the scale of priorities, something had to come up. There were three general possibilities: more extension of credit, more restriction of international transactions, private or governmental, and tighter domestic monetary and fiscal policies. As it turned out, monetary and fiscal restriction became the chief instrument that deficit countries were expected to use for adaptation.

The system into which we entered was one where deficit countries were expected not to devalue, not to control capital movements, and not to restrict imports or subsidize exports. At the same time the extension of credit—and particularly the assurance of future credit—was to be limited. This would force the deficit country to get its house in order, by monetary and fiscal restraint.

There were undoubtedly many reasons for the difference between the system of the late 1950's and the Bretton Woods philosophy. The main deficit country was the United States, and it was quite a different thing to consider the devaluation of the dollar than to talk about the devaluation of some unspecified currency which is one of fifty or sixty similar currencies of equal and small international importance. Also, the economic environment that we were looking at was quite different in 1958 than it had been in 1944. At the end of the war our standard picture of the peacetime economy was one plagued by unemployment and trade restrictions. The Bretton Woods philosophy focused on preventing a recurrence of this. By 1958, again with our usual over-the-

shoulder look, the big problem seemed to be inflation, whereas the maintenance of high employment and progress towards trade liberalism seemed reasonably assured. Moreover, the international financial system was in the hands of different people than fifteen years earlier, or in the hands of the same people grown more conservative and practical.

This was the heyday of discipline, which the disciplined not only accepted but even welcomed. In the United States we were being told that we could not have a more expansive fiscal and monetary policy because of our balance-of-payments deficit, and that we should not try to increase the expansibility of the international financial system because that might tempt us to have a more expansive fiscal and monetary policy.

I am not here to criticize this view of the system. I think that a period of anti-inflationary policy was good for the United States after 1957, to increase the possibility of subsequently having high employment without inflation. I also think it is sometimes useful to impose or accept outside limits on ourselves, even artificial ones, in order to save ourselves from our own folly.

But this system probably could not last long, and in any event did not. The system assumed that deficit countries, if they could not obtain credit, would be unwilling to use any alternative other than internal disinflationary or restrictive policy. But as persistent domestic restraint became more irksome, the deficit countries—which means the United States and Great Britain—became more willing to use or contemplate other measures. Restrictions of trade and capital movements became clear possibilities and even devaluation could not be definitely ruled out. This does not mean that plans were adopted which explicitly included either drastic restrictions of trade or capital, let alone devaluation. It means that the deficit country became willing to follow a course of domestic action which it wanted very much even at some risk that the result would be to continue or enlarge the deficit and might be to force strict control of international transactions, or devaluation, if credit were not forthcoming.

At the same time that this was happening—and I think at least partly because of it—the potential lenders discovered that it was really in their interest to lend. The potential lenders stand in a number of different relations to the deficit country. They sell to the deficit country and compete with it in third markets. They may be receiving long-term capital from the deficit country. They have a political relationship which places a certain value on good will. Also the potential lender must consider the possibility that it will sometime need to borrow from the country now in deficit.

For these reasons, the surplus countries lend. These reasons may not induce lending if the only alternative is that the deficit country follows a restrictive fiscal and monetary policy, especially if the lending decisions are made by people who tend always to think of financial restriction as a good thing. However, they do induce lending as an alternative to severe restrictions on trade and capital movements and probably especially as an alternative to devaluation.

The United States took a few small steps to reduce its balance-of-payments deficit by measures restricting international trade beginning in 1960. U.S. government foreign aid was tied to purchases of U.S. goods, the government became much stricter in trying to cut its expenditures in foreign currencies, even at the cost of larger expenditures in dollars, and permissible duty-free imports by tourists were reduced. However, these steps only brought the United States closer to the level of restrictionism already common in other countries and were not regarded as a departure from the prevailing international monetary system.

In 1961, with the advent of an Administration dedicated to "Getting America moving again," the United States began actively to seek arrangements for obtaining more international credit. This effort was highly successful. At the time the arrangements made were characterized by some as stop-gap and gadgety. But concentration on the form may have distracted us from the simple and important points. We no longer accepted the existing limits of international credit as a beneficial restraint on us but would try to expand those limits. When tested, the elasticity of the international credit supply turned out to be large.

The United States made a declaration of independence in 1963 with the proposal of a large tax cut. Reasons were offered for thinking that the tax cut would reduce the balance-of-payments deficit. This was a possible result, but a highly uncertain one. What was behind the tax cut was not a finding that it would assist the balance of payments but a decision to try to regain high employment even at a risk to the balance of payments. If the balance of payments deteriorated, that situation would have to be met in some other way than excessive unemployment. And when the balance of payments turned worse, in the summer of 1963 we responded with the interest equalization tax, to restrict the outflow of capital, and with the announcement of our willingness, previously in doubt, to borrow from the IMF. We have continued to receive credit, in the form of increasing accumulation of dollars in foreign official ownership.

I interpret the British as having made a similar declaration of independence, also in 1963, with a decision to try to accelerate their eco-

nomic growth. The means by which this decision was to be implemented have never been clear to me. However, the significant fact was the decision no longer to be inhibited, as they had been in the past, by fear of the balance of payments. When her international financial position deteriorated, in 1964, Britain reacted by imposing a surcharge on imports. And when the situation became critical, the big financial powers extended her credit.

Reporting on the aid to Britain earlier this month, an American newsmagazine said: "The huge sum that rescued the pound must be repaid within six months, and Britain's creditors thus hold a de facto veto power over government policy. In effect they will insist that Britain must regain solvency and international confidence before anything else is undertaken, including further expensive welfare schemes."

Perhaps reading this in Brazil was what made it seem so odd a conclusion to me. Brazil certainly has plenty of creditors, but they have no veto power over her policy. Brazil's creditors continue to lend to her, not because of confidence, but because of fear of the consequences of not lending. Britain's creditors lent to her because they feared that otherwise Britain would have to devalue or give up convertibility. Is it credible that Britain's creditors would not extend credit again if the alternative should be the same six months from now, or that the answer would depend on how many pairs of spectacles the British government had given away free.

Britain will not behave like Brazil. The reason will not be that Britain's creditors are more powerful or hardhearted than Brazil's. The reason will be that the British understand the evils of inflation better than the Brazilians do.

The present situation among the major countries, as I see it, is that surplus countries will probably extend credit to deficit countries when necessary to prevent the deficit country from imposing exchange controls or other severe restrictions on trade and capital movements or devaluing. However, this is only a probability, the formal commitment to extend credit being limited, and deficit countries have to consider the risk that the surplus countries will not lend. Nevertheless, deficit countries will run this risk for the sake of serious objectives of national policy.

In this situation economic policy addressed primarily to domestic objectives has considerable freedom from the so-called "discipline" of the international financial system. How much freedom depends on how the deficit country evaluates the possibility that the surplus countries will not extend credit in a crisis or to forestall a crisis. It also depends on the willingness of deficit countries to adopt the measures that would become necessary if they bet on the availability of credit and lose. In

both respects the freedom of domestic economic policy has increased in recent years. The potential lenders of course maintain a posture of reluctance, limiting their advance commitments in time and amount. But experience of the last few years suggests that nevertheless they will lend. Also experience suggests that deficit countries are more likely to restrict trade and capital movements than to foresake policies that are domestically important. The result may not be a great deal of such restriction but more pressure on lenders to extend credit to prevent such restriction.

Of course, some discipline remains because some risk to the deficit countries remains. Even though the risks are small, they will not be run for trivial reasons. The risks are probably greater for small deficit countries than for big ones.

If this interpretation of the system is correct—and I repeat what I said earlier about its tentative character—the international financial system will not significantly restrain us from what we strongly want to do. We should not fear that it will debar us from correct policies that we are determined to follow or hope that it will protect us from errors that we are determined to make.

ARE THERE ENOUGH POLICY TOOLS?

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I. *Introduction*

If I were asked to sketch the main features of the world economy as it exists today, with particular reference to the advanced industrial countries, I would describe the situation in the following way:

1. Each country insists on the free exercise of its sovereign authority to regulate its level of internal demand for the purpose of maintaining suitable economic conditions at home in terms of employment and the behavior of its internal price level. In addition, the idea of governmental responsibility for the attainment of a satisfactory rate of long-term economic growth is widely accepted, and some efforts are made to influence the composition of demand in favor of investment-type activities as a means of accelerating growth.

2. In many countries, fiscal policy is, as a practical matter, a relatively inflexible instrument, partly because of unsatisfactory administrative arrangements and in some cases also because of outmoded and unenlightened views about budget deficits. Monetary policy is administratively more flexible, but, in practice, freedom to use it for domestic purposes has become increasingly limited by balance-of-payments considerations.

3. Free international movement of goods and of capital as a means of achieving efficient use of resources is generally accepted as a goal, and substantial progress has been made in achieving it. In particular, since the advent of general currency convertibility in 1958, controls over the international flow of capital have been relaxed and investors have become increasingly inclined to shift funds internationally in response to differential changes in expected rates of return.

4. Subject to some important qualifications to be discussed below, trade is conducted under a system of fixed exchange parities, with actual exchange rates fluctuating only within very narrow limits around these parities.

5. Countries hold limited supplies of monetary reserves in the form of gold, foreign exchange, and lines of credit at the IMF. The reserves available and potentially obtainable set a limit—albeit a somewhat elastic one—on the cumulative size of a country's balance-of-payments deficit. For this reason, each country operates subject to a balance-of-payments constraint—not in the sense that the balance of payments

must always be in equilibrium but in the sense that there is some limit on the size and duration of deficits that can be tolerated. There is no corresponding limit for surpluses.

Some of the features in the above list need to be spelled out a little more fully. First, with regard to the goal of internal stability (item 1 above), countries are often said to seek the twin goals of "full employment" and "price stability." For some time, however, it has been getting increasingly clear that this way of describing the situation is quite out of touch with reality. Indeed, the concept of full employment, while perhaps useful as a slogan, is without precise meaning. A more accurate way to describe the situation is as follows: There is in each country a "trade-off" between employment (or unemployment) and price stability; that is, over a considerable range the more unemployment is reduced by policies to expand aggregate demand the higher is the price that must be paid in terms of inflation. This relation holds primarily because of the tendency of money-wage increases to outstrip increases in productivity even under conditions of substantial unemployment. The trade-off varies from country to country, depending on the organization, traditions, and aggressiveness of the labor movement, the price policies followed by industry, and so on, and from time to time depending on the attendant circumstances. The trade-off may be influenced by policy measures—wage-price guideposts, incomes policies, etc.—but I am not aware of any cases in which efforts to change it have been notably successful. Not only does the trade-off itself vary from country to country but so also do the relative weights attached to price stability and employment in the implicit social welfare functions that govern the behavior of the authorities responsible for economic policy in the various countries. As a consequence, to the extent that each country is left free to decide what combination of price inflation and employment to select from the many choices open to it, price trends may vary considerably from country to country.[1]

The other feature that calls for further comment is exchange rate arrangements (item 4 above). Although, as indicated above, fixed exchange rates appear to be one of the generally accepted goals of economic policy, we do not now have a system of really fixed rates. Indeed, the most serious difficulty with the present international monetary system seems to lie in the area of exchange rate policy. The present arrangements, under which exchange rates are fixed within very narrow limits at any particular time but are subject to readjustment from

[1] Much lip service is paid to price stability as a goal of national economic policy but in reality it is an extremely unrealistic objective, since no one is in fact willing to pay the price in terms of unemployment required to achieve it. And since it is a practically unattainable goal for individual countries, it is obviously an unrealistic objective for the world as a whole.

time to time to correct "fundamental" disequilibria in national balances of payments, seem ideally calculated to encourage speculation. Since opportunities for the investment of capital, viewed broadly, do not ordinarily vary widely as between major countries, even a mild suspicion that a country may devalue its currency can cause a speculative outflow of capital from that country. And, as more and more investors become familiar with the possibilities of transferring capital internationally, it seems likely that the potential size of speculative capital flows will become even larger. The result is that most countries will entertain the possibility of devaluation only in the most dire emergency, but the threat is nevertheless sufficient to induce speculation. And there is always the possibility that speculation will exhaust a country's reserves and force the devaluation that the speculators are hoping for.

It does not strain reality very much to describe the world economy we seek to achieve as one in which (1) there is complete freedom in the international movement of goods and capital, (2) exchange rates are fixed, and (3) individual countries are free to use monetary and fiscal policy—primarily the former—to attain their domestic price and employment goals. And the system is subject to the constraint that each country possesses a limited supply of reserves with which to cover deficits in its balance of payments.

The trouble with this system is that it is basically inconsistent. There are three possible ways of correcting a deficit or surplus through adjustment of the current account: through the use of trade or exchange controls, through an adjustment of exchange rates, and through internal price and income changes. Since all of these violate the principles of the system, they are ruled out. Consequently, when a country experiences a deficit, there is no assurance that the deficit will be eliminated before its limited supply of reserves is used up. And the difficulty is further compounded by the nature of the prevailing exchange rate system under which a persistent deficit creates a fear of devaluation, possibly leading to a speculative outflow of capital and an unlimited self-generating expansion of the deficit.

Thus, if the underlying principles are adhered to, there is no mechanism that can be depended upon to eliminate a balance-of-payments disequilibrium brought about by such disruptive forces as changes in tastes or technology. Beyond that, even if the system is in equilibrium to begin with, the diverse price trends inherent in the independent economic policies of the member countries may themselves in due course produce disequilibrium.

Of course, the system has worked after a fashion—although the road has been pretty rocky, especially in view of the fact that only six years have elapsed since the restoration of convertibility in 1958. And

it has survived only because its fundamental principles have been violated in various ways.

1. The underlying principle of free movement of goods and capital has been compromised through the imposition of trade restrictions for balance-of-payments reasons by Canada in 1962 and the United Kingdom in recent weeks. The United States has also persistently violated the principle by tying foreign aid, by discriminating in favor of American suppliers in its defense procurement policies, and by the recent enactment of the so-called "Interest Equalization Tax."

2. Minor use has been made of exchange rate adjustments in the Dutch and German revaluations of March, 1961. Such adjustments, however, probably do more harm than good by weakening confidence in the overall stability of exchange rates and encouraging speculation.

3. In practice, domestic monetary and fiscal policies have not been entirely unaffected by the balance-of-payments situation. In part, this is because, due to the less than perfect effectiveness of domestic monetary and fiscal tools, it has not always been feasible to offset completely the automatic corrective effects of deficits and surpluses on internal demand. But beyond that, deficit countries have found it necessary to adapt their domestic policies to the exigencies of the balance of payments—albeit reluctantly—when their international reserves have been seriously threatened. The leading example here is the United States, which has suffered from an unnecessarily high rate of unemployment and a resultant irrecoverable loss of output amounting perhaps to $150 billion in the last five years, partly as a result of its balance-of-payments deficit. This has not been due, in my opinion, however, to the fact that policies to expand aggregate demand have been held back by fear that they would worsen the trade balance. Rather it has been a result of the administrative inflexibility of fiscal policy (as a consequence, in part, of unenlightened views about budget deficits and growth of the public debt) combined with the fact that the need to avoid an accentuation of short-term capital outflows has acted as a significant constraint on monetary policy—the one flexible and acceptable instrument that might in the absence of the balance-of-payments constraint have been used to expand aggregate demand. No doubt similar considerations have to some extent operated in European countries to limit the use of restrictive monetary policies to check excessive inflation in the face of balance-of-payments surpluses. But it is quite clear that the system has an inherent deflationary bias. The limited supply of reserves sets some upper bound on the size of a nation's cumulative deficit, whereas there is no equivalent upper bound to the size of a cumulative surplus and the associated expansion of monetary reserves.

Techniques of central bank cooperation through the use of currency

"swaps" and intervention in foreign exchange markets to deal with minor speculative crises have been progressively developed and refined. To deal with more serious crises—the most dramatic of which, to date, is the sterling crisis of recent weeks—massive supplies of foreign exchange have been mobilized to support threatened currencies and combat the activities of speculators. Up to now, these efforts have been successful in fending off disaster, but the world lives in dread of a forced devaluation of sterling or the dollar, which would in all probability create a state of international financial chaos from which recovery would be extremely difficult. Moreover, the present situation gives the opinions of international currency speculators an entirely disproportionate weight in the determination of economic policy.

It seems clear that the present international economic arrangements are seriously defective. It is doubtful whether they can survive in their present form for very long. The question is: What changes should be made? In the extensive discussions of the international economy that have taken place in the last few years, much energy has been expended on the formulation of proposals for reform of the world's monetary institutions, and many ingenious schemes have been proposed. However, it is difficult for me to see how the mere establishment of an improved international banking arrangement for providing liquidity can be depended upon to yield a satisfactory solution to our problems as long as private international transactions are conducted by the use of national currencies whose exchange parities are felt to be subject to change. Even if gold were completely demonetized and official settlements between countries were carried out by the transfer of credits on the books of a reformed IMF, the problem would remain. Suppose, for example, that under such an arrangement the United States were to run a substantial balance-of-payments deficit. The dollar would decline in foreign exchange markets, and foreign central banks would have to buy dollars to prevent the exchange rate from moving outside the prescribed limits. The dollars would be deposited with the IMF for credit to the other country's account, and they would be debited to the U.S. account. If the latter account were to become exhausted, the United States would presumably have to arrange a loan from the IMF which would replenish its balance. As long as there was felt to be an effective limit to the credit line available, so that eventually devaluation (or direct controls) might have to be employed to correct the deficit, private investors would have essentially the same motive to speculate against the dollar as they have under the present system, and the speculation itself would help to exhaust the credit line. Since claims against the IMF would presumably be subject to an exchange guarantee, countries might be willing to extend sufficiently large credits through the IMF to make the effective supply of reserves available for meeting speculative

runs larger than it is at present. But there is no assurance that the fundamental problem of speculation would be eliminated.

Schemes to reform the financial system and increase the supply of monetary reserves can undoubtedly be of some help. But the more fundamental need is to introduce some workable mechanism for restoring and maintaining balance-of-payments equilibrium. Can this be done more or less within the confines of the present system by making more flexible use of traditional policy instruments? Or are some fundamental reforms of the system required, and if so, what reforms would be appropriate? These are the questions to which I shall devote the remainder of my paper.

II. *More Flexible Use of Monetary and Fiscal Measures*

I believe a considerable improvement in economic performance could be achieved within the framework of the present international monetary system if fiscal policy could be rendered substantially more flexible than it now is. For example, in the United States some arrangement, such as that proposed by the Commission on Money and Credit in its 1961 report or that recommended in the January, 1962, *Annual Report of the Council of Economic Advisers*, giving the President discretionary authority to change personal income tax rates for countercyclical purposes would be very helpful.

The idea would be to develop a policy arrangement under which the Western industrial countries would agree to rely on flexible fiscal policy, implemented primarily through tax adjustments, to regulate internal demand to achieve domestic goals. Monetary policy would then be assigned the task of maintaining balance-of-payments equilibrium by establishing interest rates at levels which would induce a sufficient inflow or outflow of private capital to cover the deficit or surplus on current account (including government military and foreign aid transactions) that would occur at target levels of income and employment. To be workable, such an arrangement would require that tax policy be rendered much more flexible than at present, not only in the United States, but in other countries as well.

Machinery would need to be set up to provide careful international coordination of the monetary policies of participating countries. The objective would be to establish a matrix of interest rate differentials among countries which would be sufficient to achieve approximate overall equilibrium in the balance of payments of each country. Marginal adjustments would need to be made in interest rates from time to time to preserve equilibrium in the face of changes in underlying conditions. Strong efforts would be needed in order to avoid competitive increases in interest rates which would raise the general level of rates without contributing to the maintenance of payments equilibrium. It

would be highly desirable that steps be taken to increase the freedom of capital movements—especially of long-term funds—in order to make capital flows adjust more sensitively to interest rate changes.

Under such a system, the mix of monetary and fiscal policies would be used to achieve internal and external equilibrium simultaneously. If, for example, it was necessary to raise interest rates for balance-of-payments reasons, any undesired restrictive effects on internal demand would be offset by a reduction in taxes. Fairly frequent adjustments in both monetary and fiscal policies would presumably be necessary. Since, at best, only an imperfect adjustment could be achieved, monetary reserves would, of course, continue to be needed to deal with temporary balance-of-payments deficits. But, hopefully, persistent large deficits resulting in heavy drains on reserves could be avoided more and more effectively as accumulating experience led to increased skill in the use of the available policy instruments and in the coordination of monetary policies.

One of the objections to such an arrangement is that the use of the monetary-fiscal mix as a means of dealing with the balance of payments precludes its use to regulate capital formation for economic growth. To enable the country to regulate capital formation and thereby influence growth in the face of the adjustments in interest rates that would be needed to maintain balance-of-payments equilibrium, a second flexible fiscal instrument could be introduced. The best possibility for this purpose would probably be an investment tax credit, along the lines of the 7 percent credit introduced in the United States in the Revenue Act of 1962. Provision could be made for flexible adjustments of the rate of tax credit when such adjustments were deemed desirable—as, for example, to offset the restrictive effect on investment of a rise in domestic interest rates called for by balance-of-payments considerations. It is in principle possible to have—within limits at least—any desired level of investment in combination with any desired level of interest rates through appropriate use of fiscal incentives to shift the marginal efficiency of investment schedule.

Would such an arrangement be feasible as a means of eliminating the contradictions in the present system? This I find difficult to judge. If price trends among participating countries diverged persistently leading to progressively larger current account deficits and surpluses to be covered by interest-induced capital flows, the arrangement would in due time prove to be unworkable. Some supplementary efforts to coordinate other policies to prevent this would therefore be necessary. The possibilities of success seem sufficient to warrant consideration. Of course, the political difficulties of obtaining greater flexibility of fiscal policy are undoubtedly substantial in some countries. But this problem will almost certainly have to be tackled anyway, because monetary

policy is already sufficiently hamstrung by the balance-of-payments problem to make it an ineffective instrument of domestic policy in many countries. More use will have to be made of fiscal policy to achieve domestic goals even if no effort is made to achieve such far-reaching international coordination of policies as that described above.

This means of achieving balance-of-payments equilibrium would not, of course, be entirely optimal, because the equilibrating adjustments would occur entirely in the capital account. It should be remembered, however, that a balance-of-payments surplus is a form of national investment, and a deficit is a form of disinvestment. If a country ran a persistent surplus, it could, in principle at least, use tax adjustments to trim its rate of domestic investment so as to achieve the desired overall division of national resources between consumption and capital formation. Similarly, a country having a persistent deficit could offset its adverse growth effects by employing a policy mix that would encourage domestic investment. Some inefficiency would nevertheless be present, because if domestic investment was viewed as more conducive to growth than an equal amount of accumulation of foreign claims, there would be no way to shift resources between the two. The current account surplus or deficit would be primarily determined by the level of overall domestic demand and there would be no policy instrument available to change it.

III. *Approaches to Fundamental Reform of the System*

The arrangements outlined above would be a possible way of making the present system work more effectively by means of a much more flexible and sophisticated use of monetary and fiscal policies. If such an arrangement is unacceptable—or, after a trial, proved to be unworkable—there are as far as I can see only two approaches to fundamental reform: currency unification or the adoption of flexible exchange rates. Experience gives us strong reasons for believing that the first of these is definitely workable; however, it would involve a substantial cost that countries might well not be willing to pay. The second might involve a less serious cost but is less certain to work effectively. The two solutions could be combined in various ways.

A. Currency Unification. One solution to the problem of lack of sufficient tools to achieve the desired goals would be through the establishment of a unified currency system with absolutely and permanently fixed exchange rates. In this way, the balance-of-payments problem could be eliminated entirely, but I shall argue that the price that would have to be paid to make such an arrangement acceptable and viable would be the surrender of sovereignty over monetary and fiscal policies by the nations involved to a central body.

Under a fixed exchange rate system, individual countries would from

time to time experience structural balance-of-payments deficits resulting from changes in tastes, technology, and so on, and some corrective mechanism would be necessary to restore external equilibrium in these cases. Since prices in industrial countries are characteristically rigid in a downward direction, deflation, which would serve primarily to create unemployment of labor and capital, is both an economically inappropriate and politically unacceptable means of dealing with a balance-of-payments deficit. Under modern conditions, the maintenance of high levels of employment and capacity utilization requires that such changes in relative prices as are needed to correct chronic balance-of-payments disequilibria be accomplished primarily through price increases in surplus countries rather than price declines in deficit countries. In addition, as indicated earlier in this paper, the procedures of wage determination are such in most countries as to produce some inflationary tendencies at acceptable levels of unemployment. Thus, with fixed exchange rates it is necessary that the international monetary system have a moderate inflationary bias built into it. However, under fixed exchange rate systems in the past the pressure on deficit countries to eliminate deficits has been much stronger than the pressure on surplus countries to eliminate surpluses, so that such arrangements have characteristically had a substantial deflationary bias. Unless some means could be found to eliminate this bias, it is doubtful whether a fixed exchange rate system would be able to survive in the modern world.

By means of a thoroughgoing currency unification, however, it would be possible, in principle, to devise a fixed exchange rate system which would not have the deflationary bias that has characterized past arrangements of this kind. The banks in each country might agree to accept for deposit at par checks drawn on banks in all of the other countries. Thus, exchange rates would be absolutely fixed (with no margin of fluctuation). To simplify the bookkeeping, it would be desirable to redefine the national units of account so as to permit all exchange rates to be set equal to unity. The banks of each country would give credit in that country's currency for all checks deposited, no matter where the checks originated or in what currency they were denominated. Each bank would send all checks denominated in other currencies to its central bank for collection. Settlements between central banks would be handled by reciprocal accounts, or, better yet, through an international clearing agency along the lines of our Federal Reserve Interdistrict Settlement Fund. All barriers impeding the free flow of capital among countries would be removed.

The key characteristic of such an arrangement is that the central bank of each country would have an unlimited credit line with the cen-

tral banks of the other participating countries. The availability of unlimited credit would eliminate the deflationary bias that has ordinarily characterized fixed exchange rate systems. However, if each country was left free to pursue an independent monetary and fiscal policy, serious difficulties might arise as a result of differences in economic structure or policies among the various participating countries. Suppose, for example, that an important country chose to follow an inflationary domestic policy—perhaps because cost-push pressures on its price level made such a policy necessary for the achievement of the desired level of employment. The inflationary policy would tend to generate a balance-of-payments deficit, through which the inflationary pressure would be transmitted to other countries. These countries could take domestic action to offset the inflationary pressures, but this would mean an enlargement of the inflating country's balance-of-payments deficit, the counterpart of which would be a surplus in the consolidated balance of payments of the other countries. If the other countries did not wish to run surpluses in their balances of payments and thereby provide a flow of goods and services to meet the rapacious demands of the inflating country, their only recourse would be to inflate their own economies in pace. In other words, by following an inflationary policy, a single major country might be able to force the rest to choose between balance-of-payments surpluses in its favor and domestic inflation. Thus, a fixed exchange rate system which provided unlimited automatic credits to deficit countries while leaving participants free to pursue independent national monetary and fiscal policies would probably have an immoderate inflationary bias and would certainly be unacceptable.

The necessary condition for currency unification to be workable is that the participating countries give up their sovereign authority to conduct independent monetary and fiscal policies directed at internal price and employment goals. Such policies would have to be conducted by a centralized monetary and fiscal authority charged with responsibility for internal stability for the group of countries as a whole. This centralized authority, in order to carry out its responsibilities effectively, would need to have sole power to regulate the supply of money and credit and to levy certain taxes and control certain categories of government expenditures. Under such an arrangement, internal price and income changes and interest rate adjustments would take care of the balance-of-payments problems of the individual countries. The arrangement would be very similar to the internal monetary system of the United States, and balance-of-payments problems would presumably no longer be a matter of concern.

The price that would have to be paid for this arrangement would be

the loss of sovereignty over economic policy by the individual participating countries; in this respect, their position would become similar to that of individual states or Federal Reserve districts in the United States. Thus, individual countries would no longer be able to choose their optimal levels of internal demand but would have to accept the levels that were associated with the overall policies judged by the central authorities to be appropriate for the group as a whole. In other words, some countries might find themselves in the position of depressed areas—a position very similar to that of a state like West Virginia in the U.S. federal system. The central government could, of course, alleviate localized distress by programs of expenditures and tax incentives aimed at the stimulation of production and employment in depressed areas, just as the federal government is able to do in the United States.

The above argument needs to be qualified a little. Actually, it would be possible for participating countries to engage to a limited extent in fiscal policy to stimulate or retard aggregate demand to influence their internal employment and price levels. However, monetary policy would have to be centralized, and countries would have to finance their deficits by selling their securities at interest rates that would make them acceptable to investors in the financial environment generated by the central monetary authorities. And, since they would not possess the power to create money, their securities would not be free of default risk; indeed, like the securities of our state governments, their rating would presumably depend on their financial condition. As a result, persistent deficit financing might be prohibitively difficult and expensive, especially for those countries in a depressed economic condition, for which it would be particularly important. In practice, it would no doubt be desirable for fiscal action to regulate demand to be conducted almost entirely by the central government, both because such action by the constituent members would prove to be difficult and costly and because there might otherwise be troublesome competition in economic policy and serious difficulties in the proper coordination of fiscal and monetary policy.

Since the power to create money is perhaps the fundamental element of national sovereignty, it would be difficult—although perhaps possible—for the participating countries to carry out independent military and foreign policies, especially those involving heavy expenditures overseas for national security and economic aid to underdeveloped countries. With monetary sovereignty eliminated, the other elements of national sovereignty would probably wither away, with the corresponding powers being shifted to the central government.

While currency unification might provide a satisfactory solution to the problems of economic policy, it would require, directly and indi-

rectly, such a sweeping surrender of the accepted and widely revered prerogatives of national sovereignty that it is hard to believe that it would be acceptable at the present time to many countries. The United States would be especially unlikely to be willing to accept it, given our sense of responsibility as the political and economic leader of the free world.

B. A System of Flexible Exchange Rates. A second fundamental reform that would eliminate the inconsistencies of the present system would be the adoption of a system of flexible exchange rates. This would, in principle, permit the participating countries to carry out independent monetary and fiscal policies directed at the maintenance of adequate levels of internal demand, with exchange rates adjusting in such a way as to maintain balance-of-payments equilibrium.

This is not the place for an extended discussion of the already hotly debated question of the merits of a system of flexible exchange rates. While I tend to be sympathetic to such an arrangement, I realize that it would be impossible to tell for sure how it would work until it had been tried. Moreover, the prevailing views of important officials and men of affairs are generally so hostile to the idea that I judge its general adoption to be impracticable.

C. A Mixed System. The two solutions described above—currency unification and centralization of responsibility for monetary and fiscal policy on the one hand, and flexible exchange rates on the other—can be combined. According to the recent work on optimum currency areas, countries having close trading relations might properly be combined into blocs within which currencies would be unified.[2] It should be clearly recognized, in my opinion, that to be workable this would require the acceptance by the members of each bloc of a common centralized monetary and fiscal policy. Then flexible exchange rates could be employed between the blocs—which would constitute areas between which trading relations were more limited. As an example, the countries of Western Europe might constitute one bloc and the United States and the United Kingdom another, with a flexible exchange rate between the bloc currencies. A solution somewhat along these lines was suggested in the Brookings report on the U.S. balance of payments.[3]

On the face of it, this sounds like a reasonable solution. However, I find it difficult to believe that, even in such an economically interrelated area as Western Europe, individual countries would be willing to give up their historic sovereign power to control money, as would, in my judgment, be absolutely vital to the success of monetary unification. In many ways, monetary sovereignty lies at the very heart

[2] See especially R. I. McKinnon, "Optimum World Monetary Arrangements and the Dual Currency System," *Banca Nazionale del Lavoro Quar. Rev.*, Dec., 1963, pp. 366-96.

[3] W. S. Salant *et al.*, *The United States Balance of Payments in 1968* (Brookings Institution, 1963) [illegible]

of national sovereignty in all fields, including foreign and military affairs. Moreover, in strictly economic affairs, the countries may differ very substantially in their trade-offs between price stability and employment, as well as in the weights they attach to these two competitive goals. This, too, might make them very cool toward accepting a group consensus with regard to monetary and fiscal policy.

IV. *Concluding Comments*

It seems to me that the present adjustable-peg exchange rate system is unworkable and has to be abandoned in favor of either firmly fixed rates or continuously flexible rates. I do not believe any of the proposed purely financial schemes for providing more reserves can be depended upon to shore up the present system and make it workable. The trouble is that with adjustable parities, the possible size of speculative runs is so vast—remember that the entire stock of private financial claims denominated in a particular currency is potentially available to finance a run on that currency—that unlimited, or virtually unlimited, supplies of reserves are needed to provide firm assurance that a speculative run could not succeed in forcing a devaluation.

What is vitally necessary is to introduce into the system some means of maintaining or restoring balance-of-payments equilibrium. Possibly this could be done by using monetary and fiscal policies in a flexible way to provide for a systematic offset of deficits and surpluses on current account with surpluses and deficits on private capital account—with some limited interim reliance on monetary reserves while the necessary adjustments were being brought about. This would have to be done, of course, without any use of exchange-parity adjustments so that over a period of time the system would become one of reliable *de facto* exchange rate stability.[4]

As I have indicated, the only other alternatives I can see are (1) currency unification combined with full unification of monetary and fiscal policies, (2) flexible exchange rates, or (3) some combination of the two. Of course, with suitable financial tinkering and *ad hoc* adjustments, the present system may survive for many years, even without a basic reorientation of monetary and fiscal policy. But its deflationary bias and its basic instability seem inherent and likely to constitute a continuing element of weakness in the world economy that may be especially dangerous in times of crisis.

[4] I suppose there are other possible ways of making the present system work, such as the introduction of exchange or trade controls to be employed under accepted rules to deal with balance-of-payments problems. However, I am convinced that such arrangements would prove in practice to be so cumbersome, unworkable, and subject to evasion and abuse that they would ultimately collapse under their own weight.

DISCUSSION

James C. Ingram: Most of my remarks will pertain to Warren Smith's paper, since his approach is closest to the one I would take. But first a brief comment on the papers by Stein and Gilbert-McClam.

To Stein, the system consists of the intergovernmental credit arrangements through which deficits have been financed in recent years. The size and nature of these transactions depend essentially upon the play of power politics. What a nation can do with respect to domestic economic policy, or what it may be forced to do, depends upon its bargaining position and its willingness to take (or threaten to take) actions that are unpalatable to surplus countries. If it is a good bargainer, the nation may obtain room to maneuver in domestic policy. As a description of what has been happening in the recent past, Stein's account certainly rings true; national balance of payments have become a matter for political bargaining at the highest level. But there is not the slightest hint of an adjustment mechanism in this account, and little opportunity to utilize economic analysis to specify directions or even limits of domestic policy.

To Gilbert and McClam, the system consists essentially of an exchange market where currencies trade at fixed rates, with the external objective being the maintenance of fixed parities with no loss of exchange reserves. This objective is clear cut, but the means of achieving it are not. Gilbert and McClam approvingly cite the BIS policy combination—tight money and easy fiscal policy for deficit countries with idle capacity; the opposite for surplus countries with inflationary pressure—but they do not investigate the extent to which such a policy combination would permit a nation to regulate aggregate demand and achieve both internal and external balance.

The Gilbert-McClam summary of recent European experience contains little evidence that the BIS prescription is in fact being followed in Europe. The countries they describe have used a wide variety of devices and *ad hoc* measures to deal with their problems, including direct controls of various kinds. But the Europeans show no inclination to specify a set of rules which can then be systematically followed. Improvisation and expediency are the rules of this game and discretionary government controls the main instruments. (The United States has of course played the same game in recent years.)

Smith has given us a valuable and forceful statement of the world monetary problem and the alternatives we have to choose from. I find his account an especially good point of departure because I agree with almost everything he says. One thing I do not agree with—or better, do not understand—is the sharp contrast he draws between the system of flexible monetary and fiscal policies described in Part II and the limiting case of currency unification. In both these cases, (1) trade and capital movements are substantially free, (2) exchange rates are fixed, (3) nations agree to devote monetary policy to the maintenance of external balance, relying on a high interest-elasticity of portfolio capital movements, and (4) flexible fiscal policy is used to regulate ag-

gregate demand. With all these features common to both systems, I do not understand why Smith treats one system approvingly and dismisses the other on the ground that it requires an unacceptable surrender of national sovereignty to a central authority. In the alternative Smith prefers, nations must agree to "coordinate" policies in various ways, and this coordination certainly involves limitations on domestic autonomy substantially similar to those which render currency unification unacceptable. Smith does not tell us what it is about the latter system that distinguishes it so drastically from the former and requires all that centralization of power and authority.

I am not myself urging full currency unification; I am simply objecting to what I think is a misleading distinction. Smith emphasizes that currency unification would necessitate a sweeping transfer of sovereign authority over monetary and fiscal policies to a centralized authority which must have sole power to regulate credit and levy some taxes. The reason why Smith insists on centralization in this case may lie in his statement that: "The key characteristic of such an arrangement [currency unification] is that the central bank of each country would have an unlimited credit line with the central banks of the other participating countries." If that were true, we would certainly be in for trouble, but I do not see why such unlimited credit lines are any more necessary for currency unification than for the coordination case. Another reason the latter appears less demanding is that Smith has not told us how the objective of permanently fixed exchange rates is to be achieved when nations are simply coordinating policy.

Turning now to the favored alternative of economic integration through coordination, I have three comments to make:

1. One issue is whether significantly different fiscal policies can be used in the several nations. It is possible that in a world in which Smith's assumptions hold in full force—i.e., a fully integrated economy with rigid exchange rates—harmonization of fiscal policy would also be forced by the process of economic integration. Furthermore, even if separate fiscal policies prove feasible, how much of an expansionary fiscal action would be washed out by monetary policy directed at external balance? For example, if a government deficit is programmed, with interest rates allowed to rise to attract external funds to cover any external deficit, rising interest rates will tend to offset the expansionary influence of the budget deficit. Can a net expansionary influence be achieved? Similarly for a surplus country, how much of a contractionary fiscal action will be counteracted by automatic monetary responses? Smith is cautiously optimistic, but study of this question is urgently needed.

2. Another issue is whether a group of nations could create such complete confidence in the existing exchange parities that portfolio capital movements would become stabilizing instead of destabilizing. Smith does not tell us how permanently fixed exchange rates are to be achieved. Most people think such a commitment is politically unacceptable, but speculation about political reactions would serve no useful purpose here. I will say only that nations might be less apprehensive about infringements on national autonomy if economists could demonstrate that they could retain a considerable degree of control over aggregate demand through fiscal policy. In assessing this matter, we

should not forget that even without the commitments in question nations do not have a free hand to conduct domestic policy.

3. The most crucial question is whether any long-run forces exist in a fully integrated world to correct a payments imbalance. Smith observes regretfully that "equilibrating adjustments would occur entirely in the capital account." I do not think this need be true. It now seems generally agreed that in the short run equilibrating shifts in secondary reserves of commercial banks and in other liquid assets can cover a balance-of-payment disturbance, given complete confidence in existing exchange rates. The long-run corrective is less clear, however. I think the most hopeful line of approach lies in applying recent work in monetary theory to international monetary adjustment. The influence of asset accumulation (wealth effects) and the influence of changes in the composition of a nation's assets upon its expenditure patterns may provide a basis for the long-run correction of payments imbalances, with the corrective influence operating on both capital and current account. Thus, as surplus countries acquire more assets, particularly liquid assets, individuals and firms will tend to restore portfolio balance by shifting to long-term assets, which shift may in turn involve a flow of long-term portfolio capital from surplus to deficit country. Households in the surplus country may also be induced to increase expenditure for current consumption as their wealth increases, and as absorption of goods and services increases the current account will tend to shift in an equilibrating direction. Opposite changes would be occurring in the deficit country. Such an adjustment process would be very slow and gradual, but the stock of wealth in Atlantic community countries is very large relative to any current account deficit that is likely to emerge in the circumstances we are considering. If private portfolio capital movements could be made to move in an equilibrating direction, long swings in the payments positions of individual countries could be tolerated.

If economic analysis can demonstrate that such a long-run corrective mechanism for payments imbalances is compatible with some degree of national autonomy with respect to aggregate demand, political objections should be less severe. But economics has a very long way to go, and policy may turn in other directions. The interest equalization tax is a case in point. Gilbert and McClam observe that France recently "initiated a new policy line aimed at keeping the growth in internal liquidity at about the same pace as real output, *regardless of the external surplus*" (my italics). Such a policy will tend to block the corrective process suggested above.

JAMES TOBIN: Our speakers have presented widely divergent views concerning the workability of present world monetary arrangements. Gilbert and McClam paint an optimistic picture of the system as it works in continental Europe. Exchange speculation is quickly contained; natural adjustments and national policies promptly correct imbalances in payments among European countries; at the same time, the governments succeed in sustaining full employment and rapid economic growth. If they do not at the same time achieve the degree of price stability they espouse, still their rates of inflation are in practice quite tolerable in view of their other successes.

Viewing a wider monetary universe, Stein also concludes that international monetary arrangements are no obstacle to the successful pursuit of national economic and political objectives. He interprets the recent experience of the U.S. and the U.K. as demonstrating that, whatever policies they pursue, the deficit countries can force the surplus countries to extend them almost unlimited credit.

Following these papers, Smith's pessimism comes as an unpleasant shock, for he appears to prove with inexorable logic that the system cannot work. Since the other speakers claim to observe that it is in fact working, Smith's logic may seem to place him in the posture of the scientists who proved by a priori reasoning that bumblebees cannot fly. In particular, although Mr. Smith and Mr. Stein both see asymmetry in the positions of deficit and surplus countries, Smith thinks that the surplus countries, not the deficit countries, have all the bargaining power.

So the issues are joined. I think much of the difference between Smith and Gilbert-McClam reflects the fact that the BIS speakers are concerned mainly with Europe while Smith is preoccupied with recent North American experience. The difference between Stein and Smith is largely a difference in standards of performance. To put it bluntly, Stein is much more satisfied with the recent record of the U.S. economy and with recent developments throughout the world in policies affecting trade and capital movements. It is one thing to ask of the international monetary system that it survive, that exchange rates be successfully defended, that cataclysmic deflations and inflations or drastic interferences with trade and commerce be avoided. It is quite another thing to ask, as Smith does, whether the system promotes the ends which are its ultimate *raisons d'être.*

I cannot help wondering whether the European success, detailed by Gilbert and McClam, has not been underwritten by American deficits. As they themselves recognize, when the aggregate monetary reserves of a group of countries are increasing, they have little difficulty in managing the imbalances that arise within the group. Indeed, they even have the financial help of the great deficit country whenever one of the group, as Italy, runs into exchange difficulties. The European experience may have quite a different moral from the one the authors draw; their conclusion is that liquidity is not an important problem. Why not arrange things so that the whole world—not just Europe—has in aggregate a significant positive annual net accretion of reserves? The adjustment process would be easier if the normal average payments position of a country were one of surplus rather than exact balance—or of deficit, as the disparate conservative methods of reckoning now tend to make it appear.

I wish I could agree with Stein that the threat of devaluation is as potent a weapon for extracting credit from surplus countries as he makes it appear. The bargaining situation is complicated, and I agree that the surplus countries do not want devaluation of the reserve currencies or for that matter any other measures that would impair their own competitive positions in world trade. But they know that the reserve currency countries abhor devaluation even more and have given many political hostages against it. The game seems to have worked out this way: In a speculative exchange crisis, sufficient

short-term credit will be available to defend currency parities. But long-term credit is not available. And the price of short-term accommodation is the adoption of measures that run counter to the domestic and foreign policy objectives of the deficit countries. This was true for Canada as well as for the U.S. and the U.K.

I cannot regard our tax cut as a declaration of independence of the balance-of-payments constraint. From the European standpoint, measures which may diminish the U.S. trade surplus are not unwelcome. What they wanted—and what they got along with the tax cut—was tighter U.S. monetary policy, higher U.S. interest rates.

Given that convertibility has enforced greater international conformity of interest rates, the surplus countries have been able to call the tune as to the general level of rates. As Smith points out, this feature of the system—reliance on tighter monetary policy to deal both with inflation and with balance-of-payments deficits—tends to produce for the industrial world as a whole a mixture of fiscal and monetary policies which is unfavorable to growth.

Even if Stein's estimate of the strategic power of the threat of devaluation were correct, this could be on his own showing only a transient phenomenon. Both he and the other authors point out that the system is evolving into one in which major currency parities are unalterable, even in the face of "fundamental disequilibrium." As experience makes this clearer and clearer, the basic asymmetrical strength of the surplus countries, as described by Smith, will be inescapable.

What are the prospects for making such a *de facto* single-currency system work for the major industrial countries? We may note that within the U.S. such a system makes its own interregional balance-of-payments compensations, thanks to the existence of common national money and securities markets. A region can "finance" an outflow of funds, due let us say to an import surplus, by private sales of securities to other regions. These are automatically acceptable at their national market prices. This sort of finance also works between countries, as indicated by several examples in the Gilbert-McClam paper.

But there is no corresponding trans-Atlantic market, and it is difficult to construct one so long as there are international discrepancies in marginal efficiencies of capital and in the interest rates which mirror them. Indeed, one of the main problems of recent years has been that convertibility at unchangeable parities was suddenly introduced in a world where—partly because of the long absence of mobility of capital between nations—the discrepancies in rates of return on investment were rather large. It was just not reasonable to expect current account balances to adjust with any rapidity to the capital flows which these discrepancies could generate.

In these circumstances, it seems to me, the obvious and most painless solution is to compensate for these flows by intergovernmental lending in the opposite direction. I think it is unfortunate that the monetary authorities of the various countries, both surplus and deficit, have not had the patience and vision to adopt this solution. Ultimately, movements of real long-term capital would bring profit rates into closer conformity, and greater perfection of in-

ternational capital markets would then permit private transfers of securities to do a major part of the compensatory work. The alternative is to retard the international movement of capital by controls, discriminatory tax measures (like our own interest equalization tax and tax credit for domestic investment), and the like. But these measures do nothing to eliminate the initial disequilibrium in the natural rates of interest of the various countries. And if they are permanent fixtures, they destroy one of the principal advantages of having a convertible one-currency system in the first place.

If this difficult transitional problem could be resolved, I think that Smith's picture may be unduly pessimistic. There are some adjustment mechanisms that can work in a firmly fixed parity system. They may take a long time, during which compensatory public or private finance must bridge the gap. While generous supplies of reserves and compensating credit do not perform the adjustments, they do buy the time during which orderly adjustments can occur without sacrificing basic policy objectives. The mechanisms include: differential rates of inflation between surplus and deficit areas, repatriation of income from previous capital outflows, equalization of profit rates and investment attractions, changes in the structure of prices, production, and trade. So long as barriers to trade and capital movement, inherited from the past, still exist in surplus countries, their removal would help to restore balance in international payments while being a good thing in itself. The main danger in the present system, in my view, is that the participating countries do not have the patient long view necessary to permit these orderly processes of adjustment to work.

Nor will they, as Smith points out, until they recognize a greater community of interest in the making of economic policy than they have so far. Clearly we are far from setting up the world economic government which, in Smith's logic, is required to operate a single-currency system. But our countries can go further than they yet have. Consultation about policies has advanced in the OECD, where the members have gone so far as to agree to a collective target for economic growth for the decade. We must be more detailed and explicit. If convertibility has made autonomous national interest rate policies impossible, we can try to work out an agreed international interest rate policy which takes into account the positions of both deficit and surplus countries. We may as well face the fact that literal price stability cannot be achieved at all times by both deficit and surplus countries. Since deficit countries cannot in practice deflate their price levels, prices will have to rise in surplus countries. This will mean a gradual rise in the world price level—the slower the process, the larger and longer the facilities that will be needed to finance deficits. In the absence of centralized supranational policy, the best we can hope is for a more explicit and symmetrical understanding of the rules of the game regarding the financing and adjusting obligations of surplus and deficit countries.

SURVEY RESEARCH: THREE SURVEYS: FINDINGS AND IMPLICATIONS FOR THEORY AND POLICY

CONSUMER ASSET PREFERENCES*

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This paper is concerned with consumer preferences for different types of assets as shown by cross-section data on the composition of wealth held by families. The data pertain to December 31, 1962, and are from the Survey of Financial Characteristics of Consumers which was conducted for the Board of Governors of the Federal Reserve System by the Census Bureau in the spring of 1963.

The sample design of the Survey of Financial Characteristics provides data especially suitable for an analysis of this kind. Families with large amounts of wealth were sampled at higher rates than is customary in consumer surveys in order to provide averages more reliable than those usually obtained for the top wealth groups. For example, 23 percent of those interviewed had wealth of $100,000 or more. Without differential sampling rates, it is estimated that only 2 percent would have had wealth of this size. As a result, means for the top wealth group, $500,000 or more, are based on about 280 cases and those for the $100,000-$499,999 group are based on about 320 cases.

The first portion of the analysis deals with the broadest definition of wealth that seemed feasible under survey procedures. The analysis is in terms of six major components of wealth or net worth: own homes; automobiles; savings in the form of life insurance, annuities, and retirement plans; investment in businesses and professions; liquid assets (exclusive of currency); and investment assets—mainly stocks, marketable bonds, and investment real estate. The debts secured by these assets have been deducted from the value of the assets.[1]

The second part of the analysis is confined to consumer portfolio; that is, liquid and investment assets. While the ownership of a home and automobile and investment in one's own business or profession are undoubtedly part of an individual's wealth, it is doubtful that decisions regarding these investments are governed by the same factors as

* The author is deeply indebted to Dorothy S. Brady and Gertrude S. Weiss for their guidance, encouragement, and criticism.

[1] For a detailed definition of net worth and its components, see the Federal Reserve *Bulletin* for Mar., 1964, p. 290.

govern the choice among such assets as savings accounts, stocks, and bonds. A decision to shift wealth from one's own business to another form of wealth, for example, may require changes in living patterns or occupation. Shifts among portfolio items can presumably be made more freely in response to changes in interest rates and prices. In this analysis of investment preferences, six major components are distinguished: checking accounts at banks; savings accounts; U.S. savings bonds; stocks; marketable bonds; and other investment assets.

The analyses are directed towards determining the forms of wealth selected as wealth increases, or, in other words, consumer preferences among different forms of wealth and investment. The major tool for the analysis of consumer preferences is the elasticity of each component of wealth with respect to the size of net worth. For consumption goods, elasticities are generally computed in relation to income. For determining consumer preferences among different forms of wealth, the analogous relationship is with total net worth rather than with income.[2] The interpretation is similar: low elasticities are interpreted as indicating low preferences, analogous in the classification of consumption goods to necessities; high elasticities delineate the preferred choices, selected in greater amount as wealth (or income) increases, and referred to in describing consumption goods as luxuries. While the distinction between necessities and luxuries does not seem as appropriate in the investment as in the consumption field, differences in elasticities do serve to show the investment choices with high or low order of preference.

The analyses deal with three age groups separately. Because the kind and amount of wealth accumulated are related to age, elasticities of wealth components without a control on age would reflect the effect of both age and wealth. The analyses presented here are based on preliminary tabulations. Review of the data suggests that for further analysis the 55 and over age group should be divided to show 65 and over as the top age group.

Composition of Net Worth

One way of expressing relationships between total net worth and its components is in terms of shares; that is, the proportion of total net worth invested in the various components (Table 1). The share of wealth devoted to automobiles, life insurance, etc., and liquid assets

[2] For example, John Spraos describes "An Engle-Type Curve for Cash," *The Manchester School of Economic and Social Studies*, May, 1957, pp. 183-89, and J. S. Cramer describes "Ownership Elasticities of Durable Consumer Goods," *Rev. of Econ. Studies*, Feb., 1958, pp. 87-96. Some theoretical aspects of expressing preferences in terms of stocks are discussed by Cramer in "A Dynamic Approach to the Theory of Consumer Demand," *Rev. of Econ. Studies*, Feb., 1957, pp. 73-86.

TABLE 1

SHARE OF NET WORTH IN SPECIFIED FORM BY AGE AND NET WORTH, DECEMBER 31, 1962

Age and Net Worth	Mean Net Worth (In Dollars)	Total	Own Home	Auto-mobile	Business, Profession (Farm and Nonfarm)	Life Insurance, Annuities, Retirement Plans	Liquid Assets	Invest-ment Assets	Miscel-laneous Assets	Less: Un-secured Debt
Under 35	7,943	100	29	7	13	9	9	12	27	5
0–$999	336	100	29	65	6	37	35	1	6	79
$1,000–4,999	2,663	100	44	22	4	21	23	3	2	19
$5,000–9,999	7,207	100	43	12	9	13	16	6	6	4
$10,000–24,999	15,465	100	45	6	24	7	9	10	3	5
$25,000 and over	93,559	100	15	1	10	5	3	16	50	*
35–54	24,237	100	29	4	22	8	9	22	9	2
0–$999	336	100	17	52	23	57	21	*	3	73
$1,000–4,999	2,852	100	47	15	7	24	19	3	2	17
$5,000–9,999	7,225	100	57	8	5	18	13	5	2	8
$10,000–24,999	16,215	100	53	6	9	13	12	10	2	4
$25,000–49,999	35,352	100	37	4	20	11	12	14	3	1
$50,000–99,999	67,380	100	22	2	31	8	10	27	2	1
$100,000–499,999	189,286	100	16	2	22	4	8	44	5	1
$500,000 and over	1,085,816	100	4	*	33	1	1	26	35	1
55 and over	33,365	100	24	2	14	4	13	40	3	1
0–$999	235	100	23	11	1	28	42	12	3	21
$1,000–4,999	2,902	100	45	7	*	12	29	5	4	3
$5,000–9,999	7,434	100	57	4	5	7	22	6	1	2
$10,000–24,999	16,543	100	55	3	9	6	17	11	1	2
$25,000–49,999	35,641	100	40	3	10	4	22	20	2	*
$50,000–99,999	66,540	100	22	2	18	5	18	34	2	*
$100,000–499,999	205,451	100	11	1	19	3	9	55	3	1
$500,000 and over	1,123,409	100	5	*	15	2	5	67	5	1

* No cases reported or less than ½ of 1 percent.

NOTE: Data are preliminary and subject to revision. Details may not add to totals because of rounding.

decreases with level of wealth; the share devoted to businesses and investment assets increases with wealth level; and the share devoted to homes first rises, then declines with level of wealth. The declining share of wealth devoted to automobiles, life insurance, and liquid assets implies elasticities of less than one at all wealth levels, or low levels of preference, while the increasing share devoted to businesses and investment assets implies elasticities greater than one at all wealth levels, or high levels of preference.

That elasticities greater than one for businesses and investment assets necessarily lead to larger shares of total wealth as wealth increases is easily seen by a consideration of the definition of elasticity. For an elasticity to exceed one at any wealth level, the proportion of a dollar of additional wealth invested in, say, businesses—that is, the marginal propensity to hold that asset type—must exceed the proportion invested in businesses before the dollar of wealth is added. This will necessarily lead to an increased share of total wealth invested in businesses at the higher wealth level. Similar considerations hold for elasticities less than one and the declining shares shown for automobiles, life insurance, and liquid assets.

The function used to describe the relation between family holdings of a particular asset and total wealth and to estimate elasticities was of the form $y = ax^b$. Least squares estimating procedures were applied to the logarithmic transformation $\log y = \log a + b \log x$. The estimated parameters $\log a$ and b are summarized in Table 3. The estimated parameter b is the elasticity of the particular asset with respect to total net worth. For example, for families headed by persons 55 years of age or more, holdings of liquid assets increase about 0.8 percent for every 1 percent increase in wealth. The estimates of b are all many times their standard errors (shown in parentheses) and are all significant at the .01 level.[3]

Consumer preferences for asset types as indicated by the ranking of elasticities have certain similarities across the three broad age groups.

[3] Group data were used for fitting the equations. Families were grouped by the amount of their net worth and mean amounts of the various asset types were computed. The basic data used are shown in Table 1. Thus for the equation relating liquid assets to wealth, the x values were mean amounts of total net worth for all families in the various wealth groups; e.g., $2,663 for young families in the $1,000-$4,999 net worth group. The y values were mean amounts of liquid assets for all families in the various groups; e.g., $2,663 multiplied by .23 equals $612 for families in the group just mentioned. The regressions were weighted with the population weights (number of families) shown in Table A. The standard errors are also weighted estimates and were estimated in accordance with the model described by Lawrence R. Klein, *Econometrics* (Row, Peterson and Co., 1956), p. 308. Estimates of standard errors under the alternative model described on p. 310 (using the number of groups for N) yielded practically identical results. The inference that the b's are significant at the .01 level is based on a two-tailed "t" test using the number of groups less two as the number of degrees of freedom available.

The following summary indicates the ranking from highest to lowest elasticity within each age group:

	Less than 35		35–54		55 and over	
	Elasticity	Rank	Elasticity	Rank	Elasticity	Rank
Investment assets	1.70	1	1.77	1	1.18	2
Businesses	1.20	2	1.03	3	1.55	1
Homes	1.04	3	1.04	2	1.00	3
Liquid assets	.65	4	.84	4	.81	4
Life insurance, etc.	.62	5	.62	5	.66	5–6
Automobiles	.36	6	.44	6	.66	5–6

Thus the preference for investment assets and businesses is very strong in each age group and that for automobiles very weak.

To analyze further these results, it is useful to recognize that ownership of some kinds of wealth—automobiles and own homes, for example—is widespread in the population, whereas other forms of wealth—such as investment assets and investment in one's own business or profession—are less widely held and are owned more often by those in the top wealth groups. Hence, differences in the group means, which are the basis of the elasticity estimates, may reflect differences in the proportion of the group owning an asset as well as in the amounts owned by those who hold the asset. And differences in elasticities may be the result either of changes in proportions of families owning or of changes in amounts owned, or of both.

The elasticities of the various assets with respect to wealth have, therefore, been separated into two additive components—n_1 which is the part of the overall elasticity arising from changes in the proportion of families owning the asset type and n_2 which is the wealth elasticity for families holding a particular asset type.[4]

[4] The overall elasticity n can be separated into the n_1 and n_2 components as follows. Assume that there are N families classified into k groups, $N_1, N_2, \cdots, N_k$, on the basis of their total wealth, x. Then

$$\frac{1}{N_j}\sum_{i=1}^{N_j} x_i = \bar{x}_j$$

is the mean wealth of the families in the jth wealth group.

Assume that there are r asset types. Let H_{mj} be the number of families in the jth wealth group holding asset type m and let the aggregate amount be A_{mj}. Then

$$\frac{A_{mj}}{N_j} = \bar{A}_{mj}$$

is the mean amount of asset m held by families in the jth wealth group,

$$\frac{A_{mj}}{H_{mj}} = \bar{A}_{mj}(H)$$

The parameters of the equations yielding the n_1 component are summarized in Table 4. The relation used to describe the proportion of families in various wealth groups holding a particular asset was of the form $p = a + k(1 - e^{-cx})$. When x is very small, that is at low wealth levels, the expression $(1 - e^{-cx})$ is close to zero and the value of p is near a. As x increases the expression $(1 - e^{-cx})$ approaches one and the value of p approaches $a + k$, the saturation level of ownership.

The parameters a and k were determined by inspection of graphs of the data points. For determining the c parameter, the equation was transformed as follows:

$\log_e (a + k - p) - \log_e k = -cx$, which is of the form $y = rx$. The transformed data points were plotted and estimates of c were determined from inspection of these graphs. The n_1 component was obtained by differentiating the equation $p = a + k(1 - e^{-cx})$ with respect to x and multiplying the result by $\frac{x}{p}$. The resulting elasticity is $n_1 = \frac{xcke^{-cx}}{p}$ and is a function of x.[5]

is the mean amount of asset m held by families in the jth wealth group who own asset m, and

$$\frac{H_{mj}}{N_j} = p_{mj}$$

is the proportion of families in the jth wealth group who own asset m. It follows that:

$$A_{mj} = A_{mj}(H)p_{mj},\ j = 1, \cdots, k.$$

Differentiating this function with respect to x and multiplying through by $x/\bar{A}_{mj}$ yields:

$$\frac{x}{\bar{A}_{mj}} \frac{d(\bar{A}_{mj})}{d(x)} = \frac{x}{\bar{A}_{mj}} \bar{A}_{mj}(H) \frac{d(p_{mj})}{d(x)} + \frac{x}{\bar{A}_{mj}} p_{mj} \frac{d(A_{mj}(H))}{d(x)}.$$

But

$$A_{mj}(H) = \frac{A_{mj}}{p_{mj}} \quad \text{and} \quad p_{mj} = \frac{A_{mj}}{\bar{A}_{mj}(H)}.$$

Therefore,

$$\frac{x}{\bar{A}_{mj}} \frac{d(\bar{A}_{mj})}{d(x)} = \frac{x}{p_{mj}} \frac{d(p_{mj})}{d(x)} + \frac{x}{\bar{A}_{mj}(H)} \frac{d(\bar{A}_{mj}(H))}{d(x)}.$$

Or

$$n = n_1 + n_2.$$

[5] The data used are shown in Tables 1 and 2. The p values are the proportions shown in Table 2 and the x values, mean net worth in thousands of dollars for the groups shown in Table 1. With one or two exceptions the asymptote chosen $(a + k)$ was the highest ownership rate reported for a particular asset type. In some cases—businesses, for example—this was the rate reported by the wealthiest families. In other cases—automobiles, for example—ownership rates rose with wealth to a certain point and thereafter declined. The decline in ownership rates at high wealth levels is not reflected in the estimated equations. The a parameter was necessary for those asset types such as liquid assets and life insurance which are held by a high proportion of families with relatively small amounts of net worth in order to obtain a reasonable fit of the data points at low net worth values.

TABLE 2

PROPORTION OF FAMILIES WITH SPECIFIED ASSET BY AGE AND NET WORTH, DECEMBER 31, 1962

Age and Net Worth	Own Home	Automobile	Business, Profession (Farm and Nonfarm)	Life Insurance, Annuities, Retirement Plans	Liquid Assets	Investment Assets
Under 35	42	86	13	61	81	21
0–$999	11	70	2	43	62	1
$1,000–4,999	44	97	7	63	89	17
$5,000–9,999	70	97	22	76	98	39
$10,000–24,999	84	97	32	84	95	45
$25,000 and over	76	89	59	72	100	79
35–54	70	85	21	70	83	33
0–$999	8	45	2	35	31	*
$1,000–4,999	55	80	8	52	75	8
$5,000–9,999	81	89	15	71	90	24
$10,000–24,999	88	92	20	86	94	40
$25,000–49,999	84	98	40	86	99	60
$50,000–99,999	88	97	67	80	99	87
$100,000–499,999	97	98	70	77	99	96
$500,000 and over	69	86	73	66	100	76
55 and over	68	59	19	51	79	34
0–$999	9	22	4	22	34	4
$1,000–4,999	49	38	3	42	65	8
$5,000–9,999	76	51	9	52	75	20
$10,000–24,999	87	69	20	54	93	31
$25,000–49,999	89	83	29	68	97	62
$50,000–99,999	82	82	49	63	95	81
$100,000–499,999	79	84	46	61	99	93
$500,000 and over	93	79	66	79	99	100

* No cases reported or less than ½ of 1 percent.

NOTE: Data are preliminary and subject to revision.

TABLE 3

NET WORTH AND PORTFOLIO ELASTICITIES BY AGE

Parameters of $\log y = \log a + b \log x$

Asset	Age under 35		Age 35–54		Age 55 and over	
	Log a	b	Log a	b	Log a	b
Net worth components:						
Homes	−.57	1.04 (.08)	−.57	1.04 (.10)	−.44	1.00 (.09)
Automobiles	1.46	.36 (.05)	1.13	.44 (.03)	−.09	.66 (.04)
Businesses	−1.84	1.20 (.15)	−1.08	1.03 (.13)	−3.57	1.55 (.19)
Life insurance, etc.	.55	.62 (.03)	.72	.62 (.02)	.24	.66 (.02)
Liquid assets	.48	.65 (.06)	−.22	.84 (.03)	.07	.81 (.03)
Investment assets	−3.96	1.70 (.05)	−4.30	1.77 (.06)	−1.64	1.18 (.11)
Portfolio components:						
Checking accounts	.55	.57 (.10)	.67	.54 (.05)	.75	.53 (.04)
Savings accounts	−.31	.95 (.05)	−.19	.93 (.05)	−.01	.88 (.05)
U.S. savings bonds	−.57	.87 (.11)	−.43	.81 (.06)	−.52	.87 (.07)
Stocks	−2.46	1.46 (.09)	−2.90	1.53 (.06)	−3.49	1.64 (.10)
Marketable bonds	−1.82	.88 (.28)	−2.07	.96 (.18)	−2.67	1.05 (.25)
Other investment assets	−3.11	1.61 (.20)	−2.95	1.58 (.08)	−3.03	1.56 (.09)

NOTE: Standard errors of the b coefficients are shown in parentheses.

TABLE 4

RELATION BETWEEN PROPORTION OF FAMILIES HOLDING SPECIFIED ASSETS AND SIZE OF NET WORTH (OR PORTFOLIO) BY AGE

Parameters of $p=a+k(1-e^{-cx})$

Asset	Age under 35			Age 35–54			Age 55 and over		
	a	*k*	*c*	*a*	*k*	*c*	*a*	*k*	*c*
Net worth components:									
Homes	0	.84	.28	0	.88	.35	0	.89	.27
Automobiles	*	*	*	.40	.58	.30	.20	.64	.09
Businesses	0	.59	.0525	0	.73	.025	0	.66	.02
Life insurance, etc.	.40	.44	.25	.30	.56	.19	.20	.48	.155
Liquid assets	.60	.40	.43	.25	.75	.30	.30	.70	.15
Investment assets	0	.79	.095	0	.96	0.35	0	1.00	.025
Portfolio components:									
Checking accounts	.70	.30	.75	.70	.30	.07	.63	.37	.027
Savings accounts	.50	.35	1.25	.50	.40	1.35	.45	.40	.43
U.S. savings bonds	.15	.52	.75	.08	.42	1.00	.28	.40	.045
Stocks	0	.66	.425	0	.79	.125	0	.76	.07
Marketable bonds	*	*	*	0	.13	.07	0	.71	.005
Other investment assets	0	.36	.35	0	.68	.131	0	.58	.08

* The basic data did not permit satisfactory estimates for these items. See Tables 2 and 8.

NOTE: The x variable, net worth or portfolio, is in thousands of dollars.

The n_1 component of the overall elasticity has been computed for various wealth levels and is presented in Table 5. Two estimates of the overall elasticity n and of the n_2 component are presented. Estimate A of the overall elasticity for each asset type is the b parameter shown in Table 3 and discussed earlier. It is invariant with level of wealth. Estimate A of the n_2 component is derived by deducting the n_1 component from the overall elasticity and is therefore a function of wealth level.

Estimate B implies that the n_2 component—the wealth elasticity for families holding a particular asset type—can be described as a constant.[6] For families who own homes, for example, investment in homes increases about 0.6 percent for every 1 percent increase in wealth, regardless of level of wealth. Estimate B of the overall elasticity is obtained by summing the n_1 and the n_2 components and is a function of wealth level.

Charting the data points suggests that Estimate A is a better description of the wealth elasticity of investment assets and possibly businesses, while Estimate B is a better description for homes. For example, in the two younger age groups the relation between the mean amount of investment assets and wealth was more clearly a linear relationship than the comparable relation for holders of investment assets only. Thus the A estimate which depicts elasticity as invariant with wealth seems a better description than the B estimate which suggests that the slope is steeper at lower wealth levels than at higher.

On the other hand, the slope of the relation between mean amount of investment in homes and wealth is clearly a function of the level of wealth, with elasticities greater than one at low wealth levels and less than one at higher wealth levels. This can be seen from the data on shares presented in Table 1 which show that the share of net worth invested in homes first rises then declines with amount of net worth. Hence the B estimate seems a better description for homes. With respect to the other asset types, the choice is less clear.

Regardless of which estimate of the overall elasticity one considers, the same general conclusions as to consumer asset preferences emerge. For example, the elasticities (Estimate B) and their ranking at the point of mean net worth are as tabulated at the top of the next page.

As before, consumer preference for investment and business assets is very strong in each age group and that for automobiles very weak. Moreover, the role of the n_1 component can be described in the same

[6] The estimation procedures followed were the same as for the equations shown in Table 3 except that the x and y values used were confined to the group of families holding the particular asset. The parameters are shown in Table 6.

	Less than 35		35–54		55 and over	
	Elasticity	Rank	Elasticity	Rank	Elasticity	Rank
Investment assets	1.89	1	1.75	1	1.71	2
Businesses	1.46	2	1.63	2	1.75	1
Homes	.86	3	.65	4	.59	4
Life insurance, etc.	.65	4	.51	5	.56	5
Liquid assets	.58	5	.70	3	.71	3
Automobiles	.30	6	.35	6	.54	6

TABLE A

DISTRIBUTION OF SAMPLE AND OF SURVEY POPULATION BY AGE AND NET WORTH, DECEMBER 31, 1962

AGE AND NET WORTH	SAMPLE CASES		ESTIMATED NUMBER OF FAMILIES	
	Number	Percentage Distribution	Number (In Thousands)	Percentage Distribution
Under 35	461	100	12,792	100
Negative	79	17	2,675	21
0–$999	122	26	3,876	30
$1,000–4,999	112	24	2,940	23
$5,000–9,999	55	12	1,590	12
$10,000–24,999	53	11	1,285	10
$25,000 and over	40	9	426	3
35–54	1,153	100	24,152	100
Negative	51	4	1,823	8
0–$999	74	6	2,723	11
$1,000–4,999	144	12	4,533	19
$5,000–9,999	119	10	3,436	14
$10,000–24,999	279	24	6,942	29
$25,000–49,999	159	14	2,823	12
$50,000–99,999	99	9	1,235	5
$100,000–499,999	145	13	541	2
$500,000 and over	83	7	96	*
55 and over	1,023	100	21,290	100
Negative	14	1	415	2
0–$999	86	8	3,316	16
$1,000–4,999	70	7	2,535	12
$5,000–9,999	89	9	3,295	15
$10,000–24,999	182	18	5,732	27
$25,000–49,999	130	13	3,366	16
$50,000–99,999	95	9	1,548	7
$100,000–499,999	169	17	949	4
$500,000 and over	188	18	135	1

* Less than ½ of 1 percent.

TABLE B

DISTRIBUTION OF SAMPLE AND OF SURVEY POPULATION BY AGE AND PORTFOLIO, DECEMBER 31, 1962

Age and Portfolio	Sample Cases		Estimated Number of Families	
	Number	Percentage Distribution	Number (In Thousands)	Percentage Distribution
Under 35	461	100	12,792	100
Zero	83	18	3,013	24
$1-199	117	25	3,789	30
$200-499	78	17	2,006	16
$500-999	55	12	1,485	12
$1,000-1,999	43	9	1,102	9
$2,000-4,999	36	8	826	6
$5,000 and over	49	11	571	4
35-54	1,153	100	24,152	100
Negative	3	*	8	*
Zero	129	11	4,644	19
$1-199	106	9	3,500	14
$200-499	87	8	2,411	10
$500-999	91	8	2,422	10
$1,000-1,999	100	9	2,423	10
$2,000-4,999	149	13	3,355	14
$5,000-9,999	117	10	2,168	9
$10,000-14,999	67	6	1,275	5
$15,000-24,999	56	5	780	3
$25,000-49,999	65	6	599	2
$50,000-99,999	52	5	342	1
$100,000-199,999	45	4	128	1
$200,000-499,999	44	4	77	*
$500,000 and over	42	4	19	*
55 and over	1,023	100	21,290	100
Negative	2	*	23	*
Zero	122	12	4,458	21
$1-199	43	4	1,486	7
$200-499	44	4	1,487	7
$500-999	44	4	1,521	7
$1,000-1,999	44	4	1,353	6
$2,000-4,999	100	10	3,293	15
$5,000-9,999	89	9	2,380	11
$10,000-14,999	51	5	1,150	5
$15,000-24,999	60	6	1,455	7
$25,000-49,999	79	8	1,423	7
$50,000-99,999	66	6	609	3
$100,000-199,999	73	7	340	2
$200,000-499,999	91	9	245	1
$500,000 and over	115	11	66	*

* Less than ½ of 1 percent.

way for both estimates. For assets which are widely held in the population, such as homes, automobiles, and liquid assets, the contribution of the n_1 component is, in general, smaller than that of the n_2 component at all wealth levels and is negligible at all but rather low wealth

levels.[7] For example, the data show that for the two older age groups the saturation level for ownership of homes, automobiles, insurance, and liquid assets has been reached at mean net worth; that is, there is no contribution to the overall elasticity from the n_1 component. On the other hand, for assets such as businesses and investment assets, the n_1 component is quite large—in some cases, larger than the n_2 component—at lower wealth levels, and continues to make a contribution at rather high wealth levels. In particular, at mean net worth there is a substantial n_1 component; that is, the proportion of families owning these asset types does not reach saturation until a much higher wealth level.

The discussion thus far has been concerned primarily with wealth elasticities and their broad similarities across age groups. The data also show differences among age groups, particularly in the frequency of ownership of different asset types.

In general, at the same wealth level, ownership rates of the wealth components are highest among the youngest families, home ownership being the exception (Table 2). The home ownership rate is highest in the middle age group. At wealth levels above $25,000 where the sample limits the comparisons to the two older age groups, the tendency for ownership rates to be greater for the younger families persists. This tendency is reflected in the "c" parameters of Table 4 which decline with age for all asset types except homes; that is, the higher the "c" parameter, the more rapidly does the ownership rate approach saturation level.

For certain types of assets such as businesses, automobiles, and life insurance, the lower ownership rates among the older families seem a reasonable result of the aging process; that is to say, one would expect ownership rates to decline as families retire from active participation in business, as their need for life insurance protection diminishes, and as automobile ownership becomes more onerous. The higher ownership rates for, say, investment assets among younger families may, however, represent a preference change compared to young families of a generation ago, and could therefore affect wealth composition if it persisted.

Consumer Portfolio Preferences

A concept of wealth limited to liquid and investment assets has been selected for study since it seems closer to the concept of consumer portfolio—that is, the investments that are not also tied to some particular consumption use such as a house or an automobile or to engaging in a business or profession. Shifts from one form of portfolio to

[7] In fact, as may be seen in Table 2, at very high wealth levels, the contribution of the n_1 component to the elasticity of homes and automobiles is probably negative.

TABLE 5

TWO ESTIMATES OF NET WORTH ELASTICITIES BY AGE

Age Group and Net Worth Level	Homes (Estimate A)			Homes (Estimate B)			Automobiles (Estimate A)			Automobiles (Estimate B)		
	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2
Under 35:												
$5,000	1.04	.46	.58	1.05	.46	.59	.36	*	.36	.30	*	.30
$10,000	1.04	.18	.86	.77	.18	.59	.36	*	.36	.30	*	.30
$15,000	1.04	.06	.98	.65	.06	.59	.36	*	.36	.30	*	.30
$20,000	1.04	.02	1.02	.61	.02	.59	.36	*	.36	.30	*	.30
$7,943 (Mean)	1.04	.27	.77	.86	.27	.59	.36	*	.36	.30	*	.30
35–54:												
$5,000	1.04	.37	.67	1.02	.37	.65	.44	.23	.21	.58	.23	.35
$10,000	1.04	.11	.93	.76	.11	.65	.44	.09	.35	.44	.09	.35
$15,000	1.04	.03	1.01	.68	.03	.65	.44	.03	.41	.38	.03	.35
$20,000	1.04	.01	1.03	.66	.01	.65	.44	.01	.43	.36	.01	.35
$30,000	1.04	*	1.04	.65	*	.65	.44	*	.44	.35	*	.35
$50,000	1.04	*	1.04	.65	*	.65	.44	*	.44	.35	*	.35
$100,000	1.04	*	1.04	.65	*	.65	.44	*	.44	.35	*	.35
$200,000	1.04	*	1.04	.65	*	.65	.44	*	.44	.35	*	.35
$24,237 (Mean)	1.04	*	1.04	.65	*	.65	.44	*	.44	.35	*	.35
55 and over:												
$5,000	1.00	.47	.53	1.06	.47	.59	.66	.43	.23	.85	.43	.42
$10,000	1.00	.19	.81	.78	.19	.59	.66	.40	.26	.82	.40	.42
$15,000	1.00	.07	.93	.66	.07	.59	.66	.33	.33	.75	.33	.42
$20,000	1.00	.02	.98	.61	.02	.59	.66	.26	.40	.68	.26	.42
$30,000	1.00	*	1.00	.59	*	.59	.66	.15	.51	.57	.15	.42
$50,000	1.00	*	1.00	.59	*	.59	.66	.03	.63	.45	.03	.42
$100,000	1.00	*	1.00	.59	*	.59	.66	*	.66	.42	*	.42
$200,000	1.00	*	1.00	.59	*	.59	.66	*	.66	.42	*	.42
$33,365 (Mean)	1.00	*	1.00	.59	*	.59	.66	.12	.54	.54	.12	.42

TABLE 5—CONTINUED

Age Group and Net Worth Level	Business (Estimate A)			Business (Estimate B)			Insurance, etc. (Estimate A)			Insurance, etc. (Estimate B)		
	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2
Under 35:												
$5,000	1.20	.88	.32	1.53	.88	.65	.62	.22	.40	.72	.22	.50
$10,000	1.20	.76	.44	1.41	.76	.65	.62	.11	.51	.61	.11	.50
$15,000	1.20	.66	.54	1.31	.66	.65	.62	.05	.57	.55	.05	.50
$20,000	1.20	.57	.63	1.22	.57	.65	.62	.02	.60	.52	.02	.50
$7,943 (Mean)	1.20	.81	.39	1.46	.81	.65	.62	.15	.47	.65	.15	.50
35–54												
$5,000	1.03	.94	.09	1.83	.94	.89	.62	.32	.30	.80	.32	.48
$10,000	1.03	.88	.15	1.77	.88	.89	.62	.21	.41	.69	.21	.48
$15,000	1.03	.82	.21	1.71	.82	.89	.62	.11	.51	.59	.11	.48
$20,000	1.03	.77	.26	1.66	.77	.89	.62	.06	.56	.54	.06	.48
$30,000	1.03	.67	.36	1.56	.67	.89	.62	.01	.61	.49	.01	.48
$50,000	1.03	.50	.53	1.39	.50	.89	.62	†	†	†	†	.48
$100,000	1.03	.22	.81	1.11	.22	.89	.62	†	†	†	†	.48
$200,000	1.03	.04	.99	.93	.04	.89	.62	†	†	†	†	.48
$24,237 (Mean)	1.03	.74	.29	1.63	.74	.89	.62	.03	.59	.51	.03	.48
55 and over:												
$5,000	1.55	.95	.60	2.01	.95	1.06	.66	.37	.29	.91	.37	.54
$10,000	1.55	.90	.65	1.96	.90	1.06	.66	.27	.39	.81	.27	.54
$15,000	1.55	.86	.69	1.92	.86	1.06	.66	.18	.48	.72	.18	.54
$20,000	1.55	.81	.74	1.87	.81	1.06	.66	.10	.56	.64	.10	.54
$30,000	1.55	.73	.82	1.79	.73	1.06	.66	.03	.63	.57	.03	.54
$50,000	1.55	.59	.96	1.65	.59	1.06	.66	*	.66	.54	*	.54
$100,000	1.55	.32	1.23	1.38	.32	1.06	.66	*	.66	.54	*	.54
$200,000	1.55	.07	1.48	1.13	.07	1.06	.66	*	.66	.54	*	.54
$33,365 (Mean)	1.55	.69	.86	1.75	.69	1.06	.66	.02	.64	.56	.02	.54

TABLE 5—CONTINUED

Age Group and Net Worth Level	Liquid Assets (Estimate A)			Liquid Assets (Estimate B)			Investment Assets (Estimate A)			Investment Assets (Estimate B)		
	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2
Under 35:												
$5,000	.65	.11	.54	.65	.11	.54	1.70	.78	.92	2.00	.78	1.22
$10,000	.65	.02	.63	.56	.02	.54	1.70	.60	1.10	1.82	.60	1.22
$15,000	.65	*	.65	.54	*	.54	1.70	.45	1.25	1.67	.45	1.22
$20,000	.65	*	.65	.54	*	.54	1.70	.33	1.37	1.55	.33	1.22
$7,943 (Mean)	.65	.04	.61	.58	.04	.54	1.70	.67	1.03	1.89	.67	1.22
35–54:												
$5,000	.84	.30	.54	.99	.30	.69	1.77	.91	.86	2.03	.91	1.12
$10,000	.84	.12	.72	.81	.12	.69	1.77	.84	.93	1.96	.84	1.12
$15,000	.84	.03	.81	.72	.03	.69	1.77	.76	1.01	1.88	.76	1.12
$20,000	.84	.01	.83	.70	.01	.69	1.77	.69	1.08	1.81	.69	1.12
$30,000	.84	*	.84	.69	*	.69	1.77	.57	1.20	1.69	.57	1.12
$50,000	.84	*	.84	.69	*	.69	1.77	.37	1.40	1.49	.37	1.12
$100,000	.84	*	.84	.69	*	.69	1.77	.11	1.66	1.23	.11	1.12
$200,000	.84	*	.84	.69	*	.69	1.77	.01	1.76	1.13	.01	1.12
$24,237 (Mean)	.84	.01	.83	.70	.01	.69	1.77	.63	1.14	1.75	.63	1.12
55 and over:												
$5,000	.81	.37	.44	1.06	.37	.69	1.18	.94	.24	2.01	.94	1.07
$10,000	.81	.28	.53	.97	.28	.69	1.18	.88	.30	1.95	.88	1.07
$15,000	.81	.18	.63	.87	.18	.69	1.18	.82	.36	1.89	.82	1.07
$20,000	.81	.11	.70	.80	.11	.69	1.18	.77	.41	1.84	.77	1.07
$30,000	.81	.03	.78	.72	.03	.69	1.18	.67	.51	1.74	.67	1.07
$50,000	.81	*	.81	.69	*	.69	1.18	.50	.68	1.57	.50	1.07
$100,000	.81	*	.81	.69	*	.69	1.18	.22	.96	1.29	.22	1.07
$200,000	.81	*	.81	.69	*	.69	1.18	.04	1.14	1.11	.04	1.07
$33,365 (Mean)	.81	.02	.79	.71	.02	.69	1.18	.64	.54	1.71	.64	1.07

* Zero or positive value less than .005.

† Not computed; ownership rates indicate negative elasticities for the n_1 component.

TABLE 6

NET WORTH AND PORTFOLIO ELASTICITIES FOR ASSET HOLDERS BY AGE

Parameters of $\log y = \log a + b \log x(H)$

Asset	Age under 35		Age 35–54		Age 55 and Over	
	Log *a*	*b*	Log *a*	*b*	Log *a*	*b*
Net worth components:						
Homes	1.37	.59 (.03)	1.19	.65 (.04)	1.46	.59 (.04)
Automobiles	1.72	.30 (.02)	1.57	.35 (.03)	1.13	.42 (.03)
Businesses	1.07	.65 (.11)	.13	.89 (.08)	−.73	1.06 (.05)
Life insurance, etc.	1.19	.50 (.06)	1.43	.48 (.03)	1.01	.54 (.04)
Liquid assets	.94	.54 (.04)	.43	.69 (.03)	.66	.69 (.04)
Investment assets	−1.58	1.22 (.08)	−1.10	1.12 (.06)	−.75	1.07 (.06)
Portfolio components:						
Checking accounts	.86	.49 (.11)	.93	.49 (.05)	1.07	.48 (.05)
Savings accounts	−.05	.92 (.03)	.23	.83 (.04)	.42	.81 (.04)
U.S. savings bonds	.90	.51 (.09)	.83	.56 (.05)	.71	.66 (.04)
Stocks	−.31	.98 (.07)	−.62	1.06 (.04)	−.77	1.11 (.05)
Marketable bonds	*	*	−.14	.88 (.12)	.26	.77 (.11)
Other investment assets	−.09	.97 (.03)	−.11	.97 (.02)	.38	.86 (.03)

* Too few cases to permit estimation.

NOTE: Standard errors of the *b* coefficients are shown in parentheses.

another can presumably be made more freely in response to investment incentives than can shifts that involve changes in living patterns or occupation.

Liquid and investment assets have been grouped into six major components as follows: checking accounts at banks; savings accounts at banks, savings and loan associations, and credit unions; U.S. savings bonds; stocks—publicly traded stocks and shares in mutual funds and other investment companies; marketable bonds—bonds, notes, bills certificates and debentures issued by the U.S. government, by state and local governments, by foreign and domestic corporations and by foreign governments; other investment assets—mortgage assets, investment real estate, closely held or family businesses with no family member active in management.

Some of these components are more homogeneous than others with respect to such characteristics as ability to produce a steady flow of current income, marketability, and risk of variation in dollar value of capital. While checking accounts at banks is undoubtedly a relatively homogeneous grouping, the other components are combinations of more diverse elements. Even the U.S. savings bonds component, which at first might appear to be a homogeneous grouping, contains some bonds which pay income currently as well as bonds of the discount variety which pay income only at time of redemption.

Among portfolio choices, publicly traded stocks and a group of investments that included mortgages, real estate and family corporations rank high in consumer preferences, while checking accounts rank low. The share invested in stocks and other investment assets rises and the share devoted to checking accounts declines with an increase in portfolio size (Table 7). The share placed in savings accounts first rises, then declines.

The elasticities (Estimate A) and their ranking from highest to lowest are as follows:

	Less than 35		35–54		55 and over	
	Elasticity	Rank	Elasticity	Rank	Elasticity	Rank
Other investment assets	1.61	1	1.58	1	1.56	2
Stocks	1.46	2	1.53	2	1.64	1
Savings accounts	.95	3	.93	4	.88	4
Marketable bonds	.88	4	.96	3	1.05	3
U.S. savings bonds	.87	5	.81	5	.87	5
Checking accounts	.57	6	.54	6	.53	6

The preference patterns across age groups are similar, with all investors showing much stronger preferences for stocks and other investment assets than for U.S. savings bonds and checking accounts.

Age and Portfolio	Mean Portfolio (In Dollars)	Total	Checking Accounts	Savings Accounts	U.S. Savings Bonds	Stocks	Market-able Bonds	Other Investment Assets
Under 35	1,716	100	9	28	6	32	2	24
$1–199	68	100	46	40	12	3	*	*
$200–499	314	100	45	33	18	3	*	1
$500–999	701	100	32	38	15	8	1	6
$1,000–1,999	1,344	100	12	49	17	10	*	12
$2,000–4,999	3,045	100	15	43	9	15	2	16
$5,000 and over	19,017	100	2	19	3	43	2	31
35–54	8,757	100	5	20	4	37	3	31
$1–199	65	100	48	38	14	*	*	*
$200–499	325	100	47	39	11	1	*	1
$500–999	709	100	33	43	12	4	*	9
$1,000–1,999	1,389	100	20	55	11	7	2	5
$2,000–4,999	3,196	100	14	49	10	11	1	16
$5,000–9,999	7,153	100	6	39	11	15	*	29
$10,000–14,999	12,301	100	5	37	3	20	1	33
$15,000–24,999	18,792	100	5	26	5	32	2	31
$25,000–49,999	34,019	100	3	17	2	35	*	43
$50,000–99,999	72,871	100	2	19	6	29	5	39
$100,000–199,999	137,790	100	5	7	2	27	2	58
$200,000–499,999	285,872	100	1	2	1	76	1	19
$500,000 and over	1,228,432	100	1	3	*	65	13	17
55 and over	22,259	100	4	17	5	45	6	24
$1–199	70	100	46	46	7	*	*	*
$200–499	302	100	42	33	20	*	5	*
$500–999	685	100	42	35	17	*	*	6
$1,000–1,999	1,306	100	27	46	12	9	1	6
$2,000–4,999	3,044	100	15	53	11	8	*	13
$5,000–9,999	6,817	100	10	46	13	13	*	18
$10,000–14,999	12,169	100	5	50	8	16	*	21
$15,000–24,999	19,044	100	4	34	10	16	6	31
$25,000–49,999	34,443	100	4	26	12	27	1	30
$50,000–99,999	68,273	100	2	20	3	39	3	33
$100,000–199,999	142,407	100	3	7	4	51	1	34
$200,000–499,999	320,935	100	2	5	1	66	5	22
$500,000 and over	1,282,457	100	3	4	1	64	15	15

Alternative estimates of the elasticities have been prepared by summing the n_1 and n_2 components and are shown in Table 9. At the point of mean portfolio the overall elasticities (Estimate B) are ranked as follows:[8]

	Less than 35		35–54		55 and over	
	Elasticity	Rank	Elasticity	Rank	Elasticity	Rank
Other investment assets	1.70	1	1.50	2	1.22	2
Stocks	1.66	2	1.61	1	1.36	1
Savings accounts	1.03	3	.83	3	.81	4
U.S. savings bonds	.86	4	.56	5	.94	3
Checking accounts	.61	5	.61	4	.66	5

While there are some shifts in the ranking for the two older age groups, the general conclusion is the same as for the A estimates, that stocks and other investment assets continue to show much higher elasticities than the liquid asset components.

With respect to the n_1 component of the elasticities, for checking accounts and savings accounts which are widely held in all age groups the contribution is relatively small at all portfolio levels. The increase in the proportion of families owning stocks, marketable bonds, and other investment assets as portfolio grows is reflected in large n_1 components at lower portfolio levels.[9]

As in the analysis of total net worth, there are differences among the age groups which are of interest, particularly with respect to the proportion of families owning various portfolio components (Table 8).

At the same portfolio level, ownership rates are generally lower among the older families. Or to put it another way, the portfolios of the younger families, at a given wealth level, appear more diversified. The c parameters shown in the lower half of Table 4 are smallest for the families in the 55 and over group, reflecting the tendency for ownership rates to move more slowly toward the saturation level.

A summary picture of the effects of age differences on the composition of portfolio is given by reweighting the data for the 35 to 54 age group in accordance with the distribution of families in the 55 and over group by size of their portfolio. The result is to remove the effect

[8] The marketable bonds component has not been included in the summary since the number of cases in the youngest age group with marketable bonds was small. For the middle and oldest groups marketable bonds ranked second and first, respectively.

[9] Ownership rates of portfolio components by portfolio level were less well behaved than were their net worth counterparts, as may be seen by a comparison of Tables 2 and 8. In particular, the appropriate asymptotic value of p was less clear. Hence it was more difficult to do a satisfactory job of curve-fitting for the portfolio components.

Age and Portfolio	Checkin Accounts	Savings Accounts	U.S. Savings Bonds	Stocks	Marketable Bonds	Other Investment Assets
Under 35	78	65	31	16	1	8
$1–199	71	53	16	3	*	*
$200–499	73	62	28	6	*	1
$500–999	87	74	38	20	2	8
$1,000–1,999	89	79	47	29	*	20
$2,000–4,999	81	85	51	39	11	24
$5,000 and over	100	76	67	66	1	36
35–54	78	78	36	24	3	21
$1–199	70	53	10	1	*	*
$200–499	79	62	16	4	1	2
$500–999	79	71	30	6	*	11
$1,000–1,999	77	86	46	16	4	6
$2,000–4,999	69	94	50	30	4	25
$5,000–9,999	81	90	56	44	2	41
$10,000–14,999	87	88	44	46	7	54
$15,000–24,999	95	89	40	74	10	52
$25,000–49,999	96	95	41	60	1	68
$50,000–99,999	100	81	48	79	9	66
$100,000–199,999	100	75	66	78	13	92
$200,000–499,999	97	98	77	95	8	35
$500,000 and over	98	85	50	100	56	64
55 and over	71	71	36	25	5	23
$1–199	63	45	10	*	*	*
$200–499	73	60	28	*	6	*
$500–999	65	49	28	6	*	6
$1,000–1,999	63	62	31	18	*	7
$2,000–4,999	64	75	30	13	*	16
$5,000–9,999	71	81	48	31	2	24
$10,000–14,999	65	85	39	36	1	35
$15,000–24,999	77	84	46	29	8	49
$25,000–49,999	86	77	56	54	11	55
$50,000–99,999	87	94	43	82	26	58
$100,000–199,999	100	75	68	76	8	47
$200,000–499,999	100	66	21	79	54	69
$500,000 and over	98	63	43	99	71	76

TABLE 9

TWO ESTIMATES OF PORTFOLIO ELASTICITIES BY AGE

Age Group and Portfolio Level	Checking Account (Estimate A)			Checking Account (Estimate B)			Savings Account (Estimate A)			Savings Account (Estimate B)		
	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2
Under 35:												
$1,000	.57	.12	.45	.61	.12	.49	.95	.17	.78	1.09	.17	.92
$5,000	.57	.03	.54	.52	.03	.49	.95	†	†	†	†	.92
$1,716 (Mean)	.57	.12	.45	.61	.12	.49	.95	.11	.84	1.03	.11	.92
35–54:												
$1,000	.54	.03	.51	.52	.03	.49	.93	.18	.75	1.01	.18	.83
$5,000	.54	.09	.45	.58	.09	.49	.93	*	.93	.83	*	.83
$10,000	.54	.12	.42	.61	.12	.49	.93	*	.93	.83	*	.83
$15,000	.54	.12	.42	.61	.12	.49	.93	*	.93	.83	*	.83
$20,000	.54	.11	.43	.60	.11	.49	.93	*	.93	.83	*	.83
$30,000	.54	.08	.46	.57	.08	.49	.93	*	.93	.83	*	.83
$50,000	.54	.03	.51	.52	.03	.49	.93	*	.93	.83	*	.83
$100,000	.54	*	.54	.49	*	.49	.93	*	.93	.83	*	.83
$200,000	.54	*	.54	.49	*	.49	.93	*	.93	.83	*	.83
$8,757 (Mean)	.54	.12	.42	.61	.12	.49	.93	*	.93	.83	*	.83
55 and over:												
$1,000	.53	.02	.51	.50	.02	.48	.88	.19	.69	1.00	.19	.81
$5,000	.53	.09	.44	.57	.09	.48	.88	.12	.76	.93	.12	.81
$10,000	.53	.14	.39	.62	.14	.48	.88	.03	.85	.84	.03	.81
$15,000	.53	.16	.37	.64	.16	.48	.88	*	.88	.81	*	.81
$20,000	.53	.17	.36	.65	.17	.48	.88	*	.88	.81	*	.81
$30,000	.53	.17	.36	.65	.17	.48	.88	*	.88	.81	*	.81
$50,000	.53	.13	.40	.61	.13	.48	.88	*	.88	.81	*	.81
$100,000	.53	.04	.49	.52	.04	.48	.88	*	.88	.81	*	.81
$200,000	.53	*	.53	.48	*	.48	.88	*	.88	.81	*	.81
$22,259 (Mean)	.53	.18	.35	.66	.18	.48	.88	*	.88	.81	*	.81

TABLE 9—Continued

Age Group and Portfolio Level	U. S. Savings Bonds (Estimate A)			U.S. Savings Bonds (Estimate B)			Stocks (Estimate A)			Stocks (Estimate B)		
	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2
Under 35:												
$1,000	.87	.43	.44	.94	.43	.51	1.46	.80	.66	1.78	.80	.98
$5,000	.87	.06	.81	.57	.06	.51	1.46	.29	1.17	1.27	.29	.98
$1,716 (Mean)	.87	.35	.52	.86	.35	.51	1.46	.68	.78	1.66	.68	.98
35–54:												
$1,000	.81	.45	.36	1.01	.45	.56	1.53	.94	.59	2.00	.94	1.06
$5,000	.81	.03	.78	.59	.03	.56	1.53	.72	.81	1.78	.72	1.06
$10,000	.81	*	.81	.56	*	.56	1.53	.50	1.03	1.56	.50	1.06
$15,000	.81	*	.81	.56	*	.56	1.53	.34	1.19	1.40	.34	1.06
$20,000	.81	*	.81	.56	*	.56	1.53	.22	1.31	1.28	.22	1.06
$30,000	.81	*	.81	.56	*	.56	1.53	.09	1.44	1.15	.09	1.06
$50,000	.81	*	.81	.56	*	.56	1.53	.01	1.52	1.07	.01	1.06
$100,000	.81	*	.81	.56	*	.56	1.53	*	1.53	1.06	*	1.06
$200,000	.81	*	.81	.56	*	.56	1.53	‡	‡	‡	‡	1.06
$8,757 (Mean)	.81	*	.81	.56	*	.56	1.53	.55	.98	1.61	.55	1.06
55 and over:												
$1,000	.87	.06	.81	.72	.06	.66	1.64	.95	.69	2.06	.95	1.11
$5,000	.87	.20	.67	.86	.20	.66	1.64	.76	.88	1.87	.76	1.11
$10,000	.87	.27	.60	.93	.27	.66	1.64	.57	1.07	1.68	.57	1.11
$15,000	.87	.29	.58	.95	.29	.66	1.64	.41	1.23	1.52	.41	1.11
$20,000	.87	.28	.59	.94	.28	.66	1.64	.29	1.35	1.40	.29	1.11
$30,000	.87	.24	.63	.90	.24	.66	1.64	.14	1.50	1.25	.14	1.11
$50,000	.87	.15	.72	.81	.15	.66	1.64	.03	1.61	1.14	.03	1.11
$100,000	.87	.03	.84	.69	.03	.66	1.64	*	1.64	1.11	*	1.11
$200,000	.87	†	†	†	†	.66	1.64	*	1.64	1.11	*	1.11
$22,259 (Mean)	.87	.28	.59	.94	.28	.66	1.64	.25	1.39	1.36	.25	1.11

Age Group and Portfolio Level	Marketable Bonds (Estimate A)			Marketable Bonds (Estimate B)			Other Investment (Estimate A)			Other Investment (Estimate B)		
	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2	n	n_1	$n_2=(n-n_1)$	$n=(n_1+n_2)$	n_1	n_2
Under 35:												
$1,000	.88	*	.88	§	*	§	1.61	.84	.77	1.81	.84	.97
$5,000	.88	*	.88	§	*	§	1.61	.37	1.24	1.34	.37	.97
$1,716 (Mean)	.88	*	.88	§	*	§	1.61	.73	.88	1.70	.73	.97
35–54:												
$1,000	.96	.97	−.01	1.85	.97	.88	1.58	.94	.64	1.91	.94	.97
$5,000	.96	.84	.12	1.72	.84	.88	1.58	.71	.87	1.68	.71	.97
$10,000	.96	.69	.27	1.57	.69	.88	1.58	.48	1.10	1.45	.48	.97
$15,000	.96	.57	.39	1.45	.57	.88	1.58	.32	1.26	1.29	.32	.97
$20,000	.96	.46	.50	1.34	.46	.88	1.58	.21	1.37	1.18	.21	.97
$30,000	.96	.29	.67	1.17	.29	.88	1.58	.08	1.50	1.05	.08	.97
$50,000	.96	.11	.85	.99	.11	.88	1.58	.01	1.57	.98	.01	.97
$100,000	.96	.01	.95	.89	.01	.88	1.58	*	1.58	.97	*	.97
$200,000	.96	*	.96	.88	*	.88	1.58	*	1.58	.97	*	.97
$8,757 (Mean)	.96	.72	.24	1.60	.72	.88	1.58	.53	1.05	1.50	.53	.97
55 and over:												
$1,000	1.05	.99	.06	1.76	.99	.77	1.56	.96	.60	1.82	.96	.86
$5,000	1.05	.99	.06	1.76	.99	.77	1.56	.81	.75	1.67	.81	.86
$10,000	1.05	.98	.07	1.75	.98	.77	1.56	.65	.91	1.51	.65	.86
$15,000	1.05	.96	.09	1.73	.96	.77	1.56	.52	1.04	1.38	.52	.86
$20,000	1.05	.95	.10	1.72	.95	.77	1.56	.40	1.16	1.26	.40	.86
$30,000	1.05	.92	.13	1.69	.92	.77	1.56	.24	1.32	1.10	.24	.86
$50,000	1.05	.88	.17	1.65	.88	.77	1.56	.08	1.48	.94	.08	.86
$100,000	1.05	.77	.28	1.54	.77	.77	1.56	*	1.56	.86	*	.86
$200,000	1.05	.58	.47	1.35	.58	.77	1.56	*	1.56	.86	*	.86
$22,259 (Mean)	1.05	.95	.10	1.72	.95	.77	1.56	.36	1.20	1.22	.36	.86

* Zero or positive value less than .005.
† Not estimated; ownership rates indicate negative elasticities for n_1 component. See Table 8.
‡ Not estimated; ownership rates indicate an increase in elasticity. See Table 8.

of differences in size of portfolio to show more clearly the effect of age. The data are as follows:

Asset Type	Age 35–54 (Reweighted with 55 and over weights)			Age 55 and over (Actual)		
	Mean Amount	Shares	Percent with Asset	Mean Amount	Shares	Percent with Asset
Total liquid and investment assets	$21,600	100	100	$22,259	100	100
Checking accounts	685	3	81	825	4	71
Savings accounts	2,926	14	83	3,699	17	71
U.S. savings bonds	600	3	41	1,045	5	36
Stocks	9,743	45	35	10,085	45	25
Marketable bonds	931	4	4	1,229	6	5
Other investment assets	6,715	31	31	5,376	24	23

This comparison shows that families in the middle age group have more diverse portfolios than do older families. They are more inclined than are older people to own stocks, such assets as investment real estate, mortgages and business ventures, and to have checking and savings accounts and U.S. savings bonds. If the investment behavior of an age group is interpreted as reflecting habits formed in the period through which a group has lived, it seems reasonable that these habits would persist. In this event, more diversity in the composition of portfolio would be expected in the future, regardless of whether the age composition of the population changes.

A SURVEY OF INVESTMENT MANAGEMENT AND WORKING BEHAVIOR AMONG HIGH-INCOME INDIVIDUALS*

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We present here some of the main findings of a study of high-income individuals recently completed by the Survey Research Center of the University of Michigan. The study focused on the attitudes and behavior of the individuals as investors and as workers. We think we have rather convincing evidence that most upper-income people, in their decisions about work and about investing, are not much concerned about escaping taxes.

In the general field of investment management, questions were asked about portfolio composition, sales and purchases of selected assets, information sources, delegation of management decisions, motives for saving, factors considered in making investments, experience with capital gains and losses, the receipt of inherited wealth, the making of gifts, and estate planning. Emphasis was placed on obtaining reasons for behavior rather than precise measures of the dollar value of the assets involved. In the field of working behavior, inquiries were made about hours of work, second jobs, retirement decisions, the employment experience of wives, and finally the presence or absence of tax disincentives.

Sampling for this nationwide study was done from unbiased lists of high-income individuals, and a total of 957 personal interviews was obtained in the spring of 1964 from individuals whose income in 1961 exceeded $10,000, hereafter referred to as the "high-income group." A disproportionately large number of interviews was deliberately secured from individuals with very high incomes. Indeed the chance of selection was roughly proportional to income.

In presenting the statistical results it was felt that each individual should be weighted in accordance with the number of income-dollars which he represented. The reason for this decision was that the economic significance of the attitudes and behavior under analysis de-

* The project was financed by the National Science Foundation and the Brookings Institution. A full report will be published by the Brookings Institution as part of its series of Studies of Government Finance. Apart from the authors of the present paper, major contributions to the study have been made by Dr. Joseph Pechman of the Brookings Institution and his Advisory Committee on Government Finance, by Professor Richard Kosobud of the Department of Economics, Wayne State University, and by Nancy Baerwaldt of the Survey Research Center, University of Michigan.

pends not so much on the number of individuals involved as on the number of dollars represented or controlled. One would perhaps be inclined to ignore the harmful effects of taxation if it were found, for example, that only 1 percent of all individuals were "locked-in" by capital gains taxes and only 1 percent worked less because of progressive income taxes. But if it was revealed that these small groups accounted for, say, 25 percent of aggregate stockholdings and received 10 percent of aggregate earnings, one might take a different view of the effects of taxation. Thus in our study each individual was weighted in accordance with his "economic importance," and income was taken to be the best available measure of such importance. Other relevant measures (such as the value of asset holdings, the value of work earnings, etc.) might provide more appropriate weights for some types of behavior, but they are highly correlated with income anyway. Consequently the percentages and proportions cited below refer not to numbers of people but to shares of the aggregate income of those with incomes over $10,000. The aggregate income of those with income above $10,000 is currently about two-fifths of total personal income in the United States and is received by about one-fifth of all families.

We present now some of the main substantive findings and turn later to a brief discussion of some of the methodological problems encountered.

Investment Behavior

Motives for Saving. In an ordinary representative sample of people, the most commonly expressed reasons for saving are to provide for retirement, for the education of children, and for security against future emergencies. But among the high-income group, the "security" motive was relatively unimportant. While the "retirement" and "education" motives were often mentioned, they declined steadily in importance as income rose. At the highest income levels (over $100,000), the motive of saving to make bequests became predominant, being mentioned by nearly half of the group.

The young (aged under 35) saved mainly for the education of their children, the middle aged for their own retirement, and the elderly for the making of bequests. Mentions of saving for the education of children were much more common among those who had themselves received extensive formal education.

Factors Considered in Making Investment Decisions. The high-income individuals were asked about the degree of importance they attached to the following investment characteristics: current yield, capital appreciation, safety, and liquidity. As would be expected from the discrimination in the tax structure against current yield (dividends, etc.) and in favor of capital gains, current yield was viewed with de-

creasing favor as income rose while appreciation was viewed with increasing favor. Of course, the need for current income is also less at the highest income levels. Perhaps surprisingly, however, there was no tendency for the investors with the highest incomes to be less concerned with safety or liquidity.[1]

Among occupation groups, those employed in the financial sector (banking, insurance, stockbrokerage, and accountancy) were the most likely to belittle the importance of current yield. Here and consistently throughout the rest of the interview this group showed the greatest sophistication in matters of investment management. This group was also the one laying the greatest stress on liquidity as a desirable investment characteristic.

Portfolio Composition. Questions about the ownership of different types of assets revealed that some types were reported with increasing frequency as income rose and ownership of other types became less extensive at the highest income levels. Among the first type ("the assets of the rich") were common stock, real estate, interests in unincorporated businesses, preferred stock, corporate bonds, municipals, U.S. bonds paying interest currently, and Treasury bills and notes. Among the second type ("the assets of the not-so-rich") were savings accounts (for which the extent of ownership reached a maximum at an income level of $40,000 and thereafter declined), U.S. savings bonds (reaching a maximum at an income of $20,000), credit union deposits ($20,000), mortgages and land contracts ($100,000), and mutual fund shares ($200,000). The assets of the not-so-rich tended to be liquid, while the assets of the rich tended to consist of business equity.

Delegation of Investment Decisions. Almost all of the high-income individuals managed their investments themselves. Only 8 percent of the group's aggregate income was received by those who had delegated some authority to others (usually to qualified professionals like stockbrokers or bank officers), and only 2 percent had delegated all authority over their investment decisions. Delegation was most frequent among those with the largest portfolios. Of those with common stock holdings worth over $500,000, about one-fourth had delegated some or all authority. Among occupation groups, female rentiers were the most likely to delegate authority.

Investment Activity and Information Sources. As would be expected, those with the highest incomes and the largest portfolios were the ones who most frequently bought and sold assets, who most frequently

[1] Similar findings were reported for a high-income sample in the Midwest by Katona and Lansing ("The Wealth of the Wealthy," *Rev. of Econ. and Stat.*, Feb., 1964, pp. 1-13). See, also, George Katona, *The Mass Consumption Society* (McGraw-Hill, 1964), pp. 212ff.

reviewed their portfolios, who most closely followed the stock market, who most often sought advice from qualified experts in making their investment decisions, and who made greatest use of other information sources such as investment advisory publications. Among occupation groups it was those in the financial sector who were the most active and the best informed. None of several other potentially relevant variables helped to explain who was best informed.

Responses to Changes in Interest Rates on Savings Accounts. The high-income individuals were asked the following questions: "Have the changes in interest rates on savings accounts had any effect on where you keep your money? In what way?" Only 22 percent replied to the first question in the affirmative. About a third of these said they reacted by keeping more of their total assets in savings accounts, and this response was most common at the incomes above $30,000 and below $300,000. Most of the other reactions consisted of transferring funds from one savings bank to another. Interestingly enough, it was those residing in the West, where interest rates on savings accounts have been at high levels and well advertised, who were the most likely to report shifting funds between savings accounts to take advantage of rate differentials.

Planned Changes in Portfolio Composition. Individuals were asked to view their portfolios as containing three types of assets—common stock, fixed-yield securities, and interests in real estate and unincorporated businesses—and were then asked whether they were satisfied with the distribution in their present portfolios between the three types. About one-fourth of the high-income group reported being dissatisfied. Dissatisfaction was especially pronounced among the young and, paradoxically, among those who had been most active in making changes in the distribution of their portfolios in the recent past. These results clearly identify a "restlessness" syndrome. Most of those who were dissatisfied expressed intentions to switch from fixed-yield securities into common stock, but at the highest incomes the most common intention was to switch in the opposite direction. Since those intending to move into fixed-yield securities usually cited tax advantages, it seems clear that they generally had municipal bonds in mind.

Experience with Capital Gains and Losses. About two-fifths of the high-income group had realized a capital gain in the fifteen months preceding the survey, and about one-fourth had realized a capital loss in that period. Asked to judge whether they had done well in their most recent sale for a gain, three-fourths expressed themselves as satisfied with their own performance. A similar proportion was satisfied with their most recent sale for a loss. Both losers and gainers usually justified their views by citing the behavior of the market after

the sale, but a substantial fraction of the satisfied losers mentioned tax considerations as a reason for having done well in the sale. These persons recognized that their capital losses could be used to reduce their income tax liability by being offset against short-term and long-term gains (and against ordinary income to a limited extent).

Two-thirds of the high-income group were in possession of large unrealized capital gains, usually in the form of common stock, and questions were asked about the forces inhibiting the sale of such assets or causing the investors to be "locked-in." One celebrated source of a locked-in effect—the tax on realized capital gains—was mentioned by only one-fifth of those with large unrealized gains. The usual explanation for not selling was simply that no better investment was available.

Gifts and Inheritances Received. A series of questions permits us to estimate that for the high-income group about one-fifth of total current wealth holdings was derived from gifts and inheritances. This estimate allows for appreciation on the original value of the amounts received. Moreover, this proportion remained remarkably constant at all income levels above $10,000.

About three-fifths of those who had at some time in the past received gifts or inheritances had sold part or all of the property received. In most cases the proceeds were largely reinvested in other earning assets.

The Making of Gifts. About one-fourth of the high-income group had made large gifts to relatives or charities within the two years preceding the survey, and this proportion rose steadily with income, reaching nine-tenths at an income level of $400,000. Gifts to relatives and to charities were about equal in frequency. Most of the gifts to relatives (usually children) were made outright, although the use of trust arrangements became increasingly frequent as income rose. When asked why the gifts to relatives had been made at that time, about half of the donors cited tax considerations. These persons were referring in part to the provisions of the federal gift tax which permit a husband and wife to make tax-free transfers of $6,000 annually to each recipient. They may also have had in mind the fact that even when gift tax rates do apply, there are still large tax advantages in disposing of estates before death.

A multivariate analysis of the factors behind the making of gifts showed that, in addition to the obvious factors of income and assets, the degree of the individual's tax consciousness was an important influence both on gifts to relatives and gifts to charities (the "tax consciousness" variable is further discussed below). This result can be explained by the numerous federal and state tax provisions which encourage the making of gifts (e.g., the federal gift tax exemptions men-

tioned above, the differential rates of the federal gift tax and the federal estate tax, the opportunity to split the wealth transfer tax base, the absence of gift taxes in most states, and the deductibility of charitable donations from income subject to federal income tax).

Estate Planning. Only one-seventh of the high-income group had made changes in their asset holdings with estate considerations in mind. As would be expected, changes were reported most frequently by those with the higher incomes, with the larger portfolios, and in the older age groups. The most common arrangement in anticipation of death was the establishment of a trust for children, followed in frequency by the distribution of property in the form of gifts, the conversion of assets into a more liquid form (presumably in anticipation of death tax liabilities, at least in part), and the purchase of extra insurance. Arrangements to increase liquidity were mentioned by only 2 percent of the high-income group, and thus the textbook discussions of this particular "economic effect" of death taxation would appear to be devoting attention to what is in practice a very minor problem.

Tax Consciousness

One of the major objectives of the survey was to investigate the effects of taxation on investment management and working behavior. Our investigation made use of three approaches. The first was to ask for the reasons behind certain selected transactions or preferences, without explicitly suggesting tax considerations to the individual as a possible motivation, and to observe how frequently tax considerations were voluntarily mentioned. The second approach was to inquire about certain types of behavior which are most likely to be tax motivated, such as the ownership of municipal bonds or the donation of appreciated assets to charity. The third approach was to ask directly about certain features or effects of the tax structure. Since the third approach involved referring to taxes in the wording of the questions, it was utilized only at the end of the questionnaire, and the first approach therefore proceeded without the interviewer ever raising the matter of tax considerations. We now briefly discuss the results of these three approaches in turn.

Each respondent was asked several questions in the areas of investment management and working behavior where tax considerations were not explicitly mentioned in the question but could conceivably have been part of the answer. (E.g., "Why is your business still unincorporated? What particular reasons did you have for making the gifts at that time?") Individuals with complex affairs were asked more than thirty questions of this nature.

The areas where statements about taxes were most frequently volun-

teered were as follows: 17 percent of the entire high-income group said that they preferred growth stocks to income stocks because of tax considerations (referring here to the favorable tax treatment of capital gains income); 13 percent said that taxes (on realized capital gains) prevented them from selling appreciated assets; 10 percent said that their most recent sale for a capital loss had been advantageous because of tax considerations; 7 percent had made estate arrangements which were motivated by tax considerations; and 7 percent had been motivated by tax considerations in their most recent sale of common stock. None of the other thirty-four questions where mentions of tax considerations could conceivably have been volunteered elicited such mentions from more than 6 percent of the high-income group.

It was found indeed that more than half of the high-income group made no voluntary mentions of taxes at all. Even among those who did reveal some awareness of tax matters, the median number of mentions was only two. The way in which the high-income respondents relegated tax considerations to a minor role in explaining their own financial behavior is rather striking.

The number of times that an individual voluntarily mentioned tax considerations was used to define his degree of "tax consciousness." A multivariate analysis of this variable revealed that it was positively correlated with income and the value of asset holdings and that male rentiers and those with large inheritances had especially high levels of tax consciousness. Perhaps the rentiers, most of whom were elderly, showed this high degree of consciousness because of their long experience in financial affairs and because of having the leisure for exploring tax matters; perhaps the heirs were highly tax conscious because of feeling some obligation to guard their patrimony with care.

The second approach involved discovering how common were certain forms of behavior which are almost invariably tax motivated. It was found, for example, that one-fifth of the high-income group owned municipal bonds. This proportion rose dramatically from 5 percent at an income level of $12,000 to 67 percent at an income level of $400,000. The absence of municipals from the portfolios of many with the highest incomes may, of course, not imply irrationality or unawareness, but rather that these people had more lucrative tax havens available to them or investments profitable enough to yield more even after taxes.

Another possible example of tax-motivated behavior is the donation of appreciated assets to charity. The law permits a taxpayer to deduct the full appreciated value from income subject to tax while paying no capital gains tax on the *de facto* realization. Only one-seventh of the high-income group had ever taken advantage of this provision, al-

though the proportion did reach three-fifths at an income level of $400,000. Here the failure to exploit the tax system more fully is rather surprising, since the great majority of high-income taxpayers routinely make charitable contributions and in high tax brackets there are large advantages to donating appreciated stock or other property instead of cash.

The third approach for the investigation of the effects of taxation utilized direct questions about the tax structure. Respondents were asked whether they had heard of the special tax advantages of investing in real estate. (The advantage might consist of being able to convert ordinary income into capital gains through legitimately claiming depreciations which in the early years may be expected to exceed the decline in market value or being able to keep a personal corporation free of penalty taxes.) Only half of the high-income group had heard of these possibilities, and only one-fifth had actually made or thought about making such investments. Respondents were then asked what other features of the income tax affected the way they invested their money. Two-thirds could think of no other features at all. One-fifth mentioned the favorable treatment of capital gains income. No other feature was mentioned by as many as 3 percent of the high-income group. A handful cited the tax-exempt status of municipal bonds and next in order came percentage depletion allowances, the tax advantages relating to investment in timber, livestock, etc., and the offsetting of capital losses against capital gains. Although these responses clearly understated the use actually made of special tax provisions, it is tempting to conclude that the vociferous defenses which are heard in support of tax loopholes often give a misleading notion of the actual numbers in the population who are vitally interested in their continuance.

An inquiry into the effects of state and local taxes on the individual's place of residence revealed that only 1 percent of the high-income group was actually thinking of moving their residence in order to save taxes. Next, it was found that one-fifth of the high-income group was unable to give even an approximate estimate of their marginal income tax rate. Finally, questions were asked about the effects of income taxation on work incentives. The results of this inquiry are reported below.

Working Behavior

Occupational Categories. In the high-income group, 9 percent were rentiers; 11 percent worked in the financial sector, most of these being owner-managers; 25 percent were professionals (doctors, lawyers, engineers, scientists, *et al.*), about half of these being self-employed; and 54 percent worked in other business enterprises, about

half of them being owner-managers and the rest employees. One-fifth of those working held more than one job. Second jobs were especially prevalent among lawyers, many of whom held company directorships.

Work Weeks and Vacations. Most in the high-income group said they worked much longer hours than the standard forty-hour week. Among those with jobs, about half said they worked more than forty-eight hours per week and about one-fourth reported working sixty hours or more. The median length of vacations was only two weeks per year.

Retirement Decisions. Seven percent of the high-income group were retired. The main reasons given for the timing of retirement were that a compulsory retirement age had been reached, that health had been deteriorating, and that others in the business were ready to take over. Virtually none said that tax considerations had affected their decision to retire. About half of the retirees had been offered work opportunities after their retirement, and about half of these had accepted the offers. The reasons given for accepting offers were usually that the work promised to be interesting or provided an escape from boredom. Financial inducements were scarcely mentioned at all. Where offers had been rejected, most explained simply that they preferred to remain men of leisure. Two persons among the twenty-four retirees rejecting work offers mentioned tax considerations.

The Employment Experience of Wives. In the high-income families about one-fourth of the wives aged under sixty-five held a job. Middle-aged wives (aged thirty-five to fifty-four) were more likely to hold a job than were those either younger or older. It was found that the likelihood of wives being employed changed markedly at a family income level of about $30,000. When the family income exceeded that level, it was found that the wives were much less likely to be currently employed; that if they were employed, they were much less likely to be employed full time; and that if they were not currently employed they were much less likely to contemplate going to work at some time in the future.[2]

When relevant, questions were asked about the reasons for the wife's plans to continue working, the reasons for her having stopped working, and the reasons for her decision about whether or not to go to work in the future. In the answers to these questions the family's financial position was rarely mentioned, and tax considerations almost never. It appeared that the wives in high-income families did not usually work for pecuniary reasons.

[2] The tendency for the wife to cease working when the husband's income gets above $10,000 or $15,000 has been reported in the *Surveys of Consumer Finances* (see *1962 S.C.F.*), p. 18, and in Morgan, David, Cohen and Brazer, *Income and Welfare in the United States* (McGraw-Hill, 1962), p. 114.

Taxation and Work Incentives. Attempts were made to investigate the delicate issue of the effects of progressive income taxation on work effort. The technique employed was to ask first some questions which did not directly raise the issue but which allowed the respondents to mention tax incentives or disincentives voluntarily; then later the questions became more specific. This procedure made it possible to check the consistency of the respondents' assertions as well as the salience of taxes to them.

The inquiry began with asking the respondents whether they had opportunities to earn additional income by working more. If they reported having such opportunities, they were asked the following question: "How do you decide how much work to do then?" Tax considerations could properly have been mentioned at this point, but only 1 percent of the high-income group did so. The most common answer implicitly referred to physical constraints (e.g., "I work as hard as I can"). Some further questions inquired into whether the respondent was working more or less than a few years previously, and the reasons for any change. Again, an opening was created for assertions about tax incentives or disincentives, but virtually no such assertions were made.

Later in the interview the following questions were put directly: "Has the income tax had any effect on how much work you do? In what way?" At this point 12 percent of the high-income group reported a tax disincentive and 1 percent reported a tax incentive.

There are good reasons, however, for caution in interpreting these results. First, many of the 12 percent claiming to suffer a disincentive had reported earlier that they had no opportunities to work more, and so the reality of the disincentive in these cases is in some doubt. Second, few in the disincentive group had voluntarily mentioned tax considerations in answer to the earlier questions about work effort. Third, it was revealed that the 12 percent claiming a disincentive worked on the average as many hours per week and as many weeks per year as those who said that their work effort was unaffected by taxation.

An attempt was made to sift through the disincentive group to find those cases where the reported disincentive was fairly plausible. From the 12 percent we eliminated those reporting that they had no opportunities for extra work and those who were already working sixty hours or more per week. This process left 6 percent of the high-income group as having "plausibly" reported a disincentive. An analysis of the characteristics of this group showed that disincentives were strongest among those in self-employed professional occupations, those with the largest asset holdings, those in the older age groups, those with a high level of tax consciousness, those with a rural or small-town background, those with Presbyterian or Methodist affiliations, and those residing in the West.

What may be said in sum about income taxation and work incentives? If it is provisionally accepted that tax disincentives are negligible among those with incomes under $10,000, where marginal rates are low; if one accepts our result that only 6 percent of the aggregate income of those with higher incomes accrues to persons who really work less because of the income tax; and if one accepts the additional finding that three-fourths of these persons are already working more than thirty-four hours per week and more than forty-seven weeks per year despite the disincentive, then it would appear that the loss of work effort in the economy due to the existence of the federal income tax instead of some feasible alternative has been small in the extreme.[3] Nonpecuniary incentives to work are clearly very powerful. The tax cut has probably had little or no effect on willingness to work, even among those with the highest incomes.

Social and Demographic Characteristics

The median age of the high-income respondents, most of whom were married males, was fifty-one years. Nearly half of the group were college graduates, and nearly half had grown up in a large city or suburb—a much higher proportion than for the remainder of the population. Two-fifths presently resided in the twelve largest metropolitan areas of the United States. Almost one-half of the group aligned themselves with the Republican party, about two-fifths with the Democrats, and the remainder with neither party. Two-fifths of the high-income group were either Presbyterians, Episcopalians, or Jews—a much higher proportion than for the rest of the population. There were comparatively few Catholics and fundamentalist Protestants. More than half of the high-income group were active in civic affairs to the extent of having been at one time an officer or committee chairman in a church or a civic group like the United Fund.

Some Methodological Problems

Interviews were obtained from 62 percent of the persons in the original sample. This "response rate," which is appreciably lower than what is normally achieved by the Survey Research Center, was depressed not so much by an unusual number of refusals as by the fact that many of the addresses in the original list were not sufficiently detailed or up to date to allow the interviewers to locate the individuals in question.

It was possible to find out some of the characteristics of the persons who could not be interviewed, and in this way ascertain whether the

[3] For similar findings for a self-employed professional group in England, see George Break, "Income Taxes and Incentives to Work: An Empirical Study," *A.E.R.*, Sept., 1957, pp. 529–49.

survey results were seriously biased. In particular it was found that single or widowed women, most of whom were presumably rentiers, were somewhat underrepresented among the interviews finally obtained. There was also strong evidence that the individuals who were interviewed generally underreported their incomes, even though it was not possible for us to check discrepancies on an individual basis. This outcome was not surprising in view of the fact that lengthy questioning is usually necessary for obtaining a full report of income, and in the survey the inquiry into incomes was quite brief. In presenting the statistical results, rough adjustments have been made for the underreporting of income.

Generally speaking, it is felt that the survey is not seriously biased for the results presented here.

Conclusions and Policy Implications

The survey of high-income individuals revealed a wide variety of motivations and behavior. One fairly prominent theme, however, emerged: the economic activities of most of the group were not dominated by precise calculations of monetary gain. Most worked long hours in the face of high marginal tax rates; the demands of their business associates and clients governed many of their activities; sheer inertia or traditionalism played a large role; indications of altruism were often present. Since concern with taxes implies a strong commitment to monetary gain, it is hardly surprising that the bulk of the respondents scarcely mentioned tax considerations at all, despite being questioned for an hour or more about the details of their financial affairs. In only a few limited segments of the high-income population did tax considerations explicitly appear to be very influential: one of the most marked influences was on the estate planning of the older people with the highest incomes and the largest wealth holdings.

Tax considerations did, however, appear to play a concealed role in some investment decisions. This is suggested by the heavy emphasis laid on capital gains as opposed to the realization of current cash yield, particularly in the case of those in the highest income brackets. People need not be keenly tax conscious to recognize and to act upon the tax aspects of the opportunity to amass fortunes through the capital appreciation route where that route is income tax free with respect to assets held until death. Our analysis suggests that it is not the level of marginal tax rates that appears to matter; nor, in the aggregate, is the array of sophisticated loopholes or tax havens of much consequence in influencing investors' decisions. Rather, it is the "pie-in-the-sky" attractiveness of tax-free capital gains that dominates the tax influences to which investors react—when they react to such influences.

Briefly stated, what are the major tax policy implications of our findings? The first, we believe, is that distributional objectives may be pursued a long way through progressive income taxation without fear of encountering appreciable costs through creating work disincentives. Second, only a very small fraction of the high-income population holds a vested interest in the continuation of the various so-called "loopholes" other than favorable treatment of capital gains and perhaps the various means of avoiding wealth-transfer taxation; the loopholes would seem, therefore, to be even less supportable than some of us had thought. And, third, the single most important tax-induced distortion of investment decisions stems from the extremely favorable treatment accorded capital gains, where the rate is zero on appreciation that remains "unrealized" through one's lifetime and rises to a maximum on realized gains of a very modest 25 percent. The findings of our survey make clear the close substitutability between capital gains and other forms of income—a substitutability which hardly seems to justify present differential tax treatment.

CONSUMER TIME AND SAVINGS BALANCES: THEIR ROLE IN FAMILY LIQUIDITY

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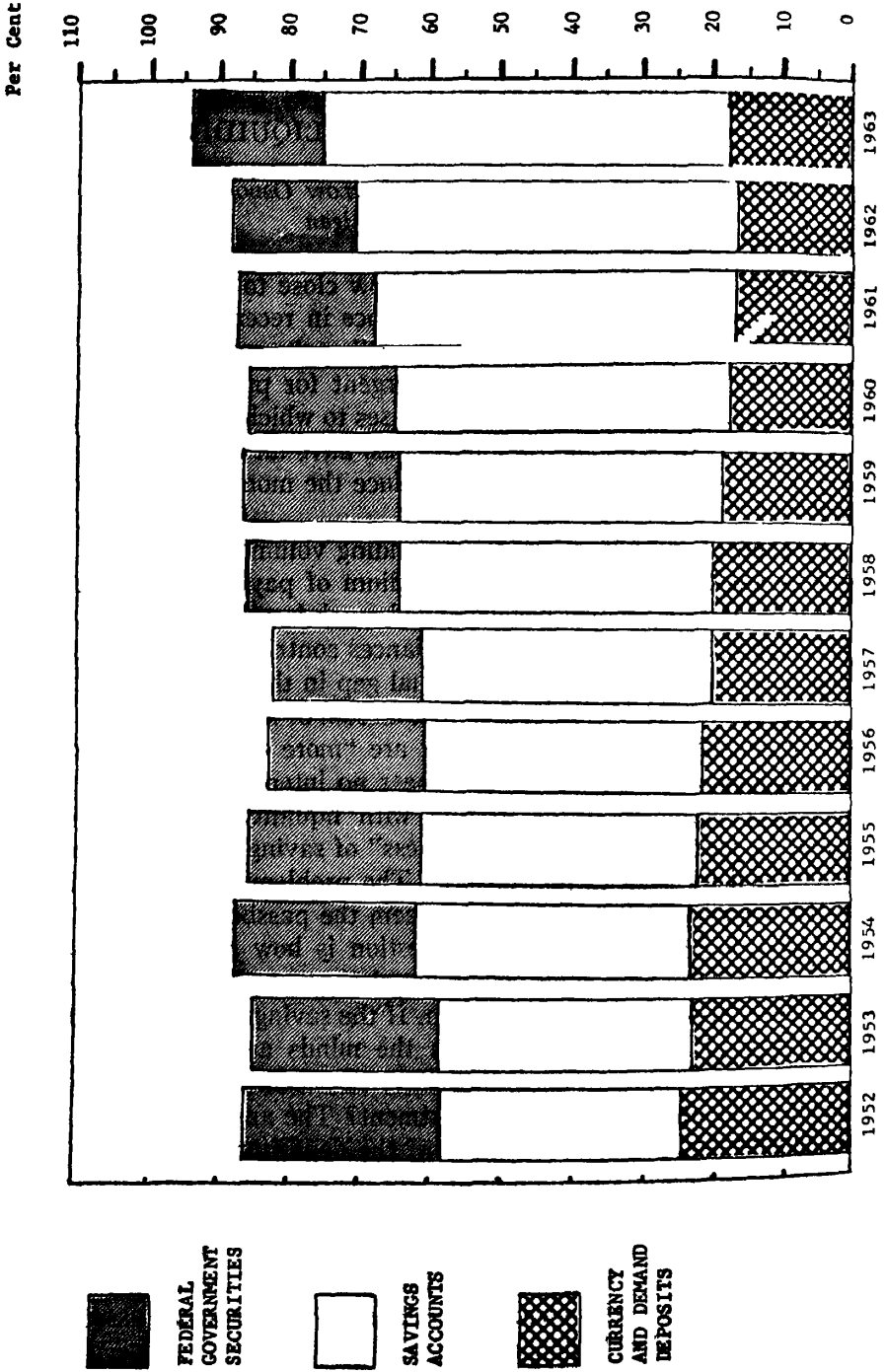
The questions, what is money and how close to money are savings accounts, have assumed added importance in recent years. The rapid growth of savings accounts and shareholdings in savings and loan associations and credit unions makes it urgent for purposes of monetary policy to obtain new insights into the uses to which these funds may be put. Builders of econometric models also have an interest in the question, what is money, when they introduce the money supply into their models.

As Chart I implies, a rapidly expanding volume of consumer transactions has been supported by a medium of payment—currency and demand deposits—which has expanded much less sharply. To what extent have rising time and savings balances contributed to this result, as they have contributed to fill the visual gap in the chart? The question cannot be answered by reference to a dictum of monetary theorists that money consists of those assets that are "more certainly realizable at short notice without loss" and/or bear no interest because interest is paid as compensation for parting with liquidity.[1] For purposes of monetary management the "moneyness" of savings balances cannot be determined by such formal criteria. The problem is not primarily one of the depositories' readiness to redeem the passbooks at short notice and in full. What is mainly in question is how savers view various kinds of assets, why they accumulated them, and for what purposes they are willing to draw them down. If the savings balances really take the place of money nowadays in the minds and operations of the holders, what tendency may they have to encourage waves of impulse spending or of stock market investment? The answers depend on the circumstances and basic attitudes of the depositors.

The present study of the liquidity of near-money assets started from the proposition that the liquidity (or closeness to money) of a financial asset depends not only on the properties of the asset itself but also, and more significantly, on the plans and purposes of its owner. Hence a survey to investigate the meaning of liquidity to financial asset owners seemed like a suitable new approach.

[1] For a discussion of various theoretical criteria of liquidity, see J. R. Hicks, "Liquidity," *Econ. J.*, Dec., 1962, pp. 787-802.

SELECTED CONSUMER LIQUID ASSETS*
AS PERCENT OF DISPOSABLE INCOME

The survey was conducted in May, 1964, with a representative national cross-section of 1,500 households by the Survey Research Center of the University of Michigan for the Board of Governors of the Federal Reserve System. The data collected relate to attitudes and behavior patterns; dollar amounts of various assets owned by each family were determined in broad brackets only. Two-thirds of the families sampled owned liquid assets of $200 or over; more than one in five had $5,000 or more.[2] Roundly a thousand of them had checking accounts, and nearly as many reported savings or share accounts. A few major findings are summarized here.

Narrow Role of Checking Balances

As consumer affluence increases, a declining proportion of income is required for the day-to-day necessities of life. The declining ratio of money holdings to consumer disposable income seen in Chart I is due partly to this fact, and to the more efficient use of transactions balances which is made possible by a larger volume of transactions per family nowadays and end by a better synchronization of income with outgo.[3] Checking accounts today, Table 1 shows, are almost wholly transactions balances. Only a very small proportion of families seems to use them to accumulate funds for long-run purposes or even emergencies. Though people were encouraged to name several uses of the funds in checking accounts, the vast majority of respondents confined themselves to explanations such as "we use our checking account to pay everyday expenses and bills," or for "nothing special." And only 8 percent of all families with $500 or more of liquid assets keep all their liquid savings in checking accounts.

Dual Role of Savings Balances

As a corollary of the lesser role of "nonlumpy," nonpostponable living expenses these days, a growing proportion of income is devoted to the acquisition of costlier goods and services which must be paid for out of several months', and sometimes several years', discretionary income. Under these conditions savings accounts serve a dual purpose. They are accumulated in most cases with such long-term goals as retirement, children's education, house buying, or emergencies in mind.

[2] Checking, savings, and share accounts; savings and war bonds.

[3] W. J. Baumol, "The Transactions Demand for Cash: An Inventory Theoretic Approach," *Q.J.E.*, Nov., 1952, pp. 545–46; John J. McCall, "Differences Between the Personal Demand for Money and the Business Demand for Money," *J.P.E.*, Aug., 1960, pp. 359–64. Apart from the widening application of payroll withholding plans, synchronization has also been facilitated by the rise of revolving credit and the freer use of "budget plans" of payment in general. A growing incentive to realize these possibilities for economizing cash balances has been provided as yields on competing financial assets have trended upward.

TABLE 1

INTENDED PURPOSES OF FOUR TYPES OF FINANCIAL ASSETS AMONG FAMILIES WHO OWN THE ASSET

Intended Purpose†	Type of Asset Owned			
	Checking Account	Savings Account	Bonds	Stock
Long-run purposes	*4%*	*52%*	*50%*	*68%*
Retirement		18	15	28
Children's education	1	22	29	20
Buy a house	1	7	2	5
Buy a business, farm	1	1		2
For interest or return		1	2	9
To bequeath money		1	2	3
Other	1	2		1
Contingency purposes	*7*	*27*	*22*	*8*
Emergencies, illness	7	22	17	4
For "security"		4	4	4
In case of unemployment		1	1	
Short-run purposes	*88*	*13*	*8*	*6*
To have funds on hand	1	4	3	2
To buy car or other durable goods	2	2	1	
Travel, vacations	1	3	1	1
Additions or repairs to house	1	1	1	
To pay taxes, debt, insurance, etc.	2	2	1	1
For usual monthly expenses	80			
Other	1	1	1	2
Other purposes	*4*	*4*	*3*	*2*
No particular purpose		*17*	*24*	*27*
Don't know, not ascertained	*4*	*2*	*3*	*4*
Total	*	*	*	*
Percent of all families who own	69%	62%	30%	25%

* Total exceeds 100 percent in that two mentions were coded.

† The questions were: "Are these funds that you have in ... being set aside for any particular use? (IF YES) What do you have in mind for these funds?"

But in addition savings accounts are viewed as revolving balances out of which large outlays for durable consumer goods, home improvements, vacation trips, and occasionally "something extra" can be financed.

In the contemporary setting, a dichotomy between current transaction balances and funds which represent long-term stores of value is inadequate. The concept of revolving savings balances which emerges from the interviews bridges this gap. The growth of these revolving funds contributed to the remarkable rise of savings and share accounts seen in Chart I.

No doubt, savings balances which are large in relation to income and

serve partly as revolving funds make consumer budget restraints less rigid and give people latitude to accelerate their spending or their investment outlays on occasion. Under what conditions might very many take advantage of this latitude at the same time? Of particular interest is the question what people would do with their savings balances if inflationary pressures grew, if installment credit terms were altered, or if interest rates changed. Would they go on a buying spree? Would they switch into marketable securities? Or would massive drafts on savings accounts require a break with observed motivations to save, and the rupture of established behavior patterns?

The answers to these questions probably vary according to the attendant circumstances. A powerful stimulus to spending, such as occurred with the outbreak of the Korean war, might lead an unusually large proportion of families to make use of their financial latitude at the same time. Under 1963-64 conditions, however, respondents' willingness to draw on the savings account showed no correlation with their price expectations, their use of installment credit, or the importance they ascribed to the rate of return. Only under relatively extreme conditions might the findings of this survey imply a threat to economic stability.

A Wave of Discretionary Spending

One part of the evidence for this conclusion comes from analysis of the circumstances in which people reported having drawn down their savings accounts over a two- or three-year period (Table 2). Around 200 families in the sample said they had done so, such declines having occurred more often in savings account balances than in checking accounts, particularly in the upper income groups. (Reductions in bond and stock holdings were much less frequent.)

Only a very small proportion of the families who reduced their savings accounts shifted the funds into other assets. About 40 percent of them spoke of making nondiscretionary outlays for various contingencies, or of carrying out such long-run purposes as buying a house or children's education. Most of another 20 percent who answered that they reduced savings balances for "no particular purpose" probably utilized the funds to meet living expenses, often during periods of interrupted income. More than one family in four reported drawing on its balance to buy a car or other durables, take a vacation, pay bills, etc. These data of course represent a distribution of purposes rather than of dollars. If the purposes mentioned were weighted by the amounts involved, contingency and investment reasons might well loom larger.

It is very important to see how much of this behavior represented established, recognized policies and customary ways of handling fami-

ly finances, such as would not be apt to change abruptly. For this reason all owners of financial assets were asked an initial general question: "Overall in your case what are the main purposes of saving?" Then they were questioned specifically regarding the purposes of funds in each financial asset (see Table 1). Later in the interview, those whose savings balances had risen were asked what they were saving up for.

In each case long-run purposes—such as children's education, purchase of a house or retirement—and contingencies were far in the lead. A significant minority of savers referred to nearer-term consumption purposes for saving such as buying a car or taking a vacation trip—projects involving payments which could not be met out of one month's paycheck or even several paychecks. And in almost all cases such consumption purposes were mentioned together with long-run or contingency uses.

Finally, savings account holders were asked if they would be willing to use their balances for certain specified purposes. An overwhelming majority said the account would be available for use in case of illness or other such emergency if one arose. If emergencies and such long-run purposes as retirement or children's education were the only frequent

TABLE 2

PURPOSES FOR WHICH FUNDS HAVE BEEN WITHDRAWN DURING THE PAST 2–3 YEARS FROM CHECKING AND SAVINGS ACCOUNT*

	Checking Account	Savings Account
To put into other form of savings	4%	5%
Long-run and contingency purposes	15	40
Emergency, illness, to supplement income	6	24
Children's education	4	5
Buy a house	5	11
Short-run purposes	32	28
Car or other durable goods, vacations	7	15
Additions or repairs to house	3	2
Regular living expenses	8	
To pay bills, taxes, and other obligations	14	11
Other purposes	4	8
Don't know, not ascertained; no particular purpose	45	19
Total	100%	100%
Number of cases who owned	1,006	931
Percent of owners of the form who withdrew from this form of savings	16%	23%

* The questions were: "People put money into . . . or take money out from time to time. Altogether did you put new money into . . . or take money out 'on balance' over the *last two or three years?* (IF TOOK OUT) Did you use the money that you took out of . . . for any particular purposes? (IF YES) What are they?"

reasons for drawing down savings balances, the monetary authorities would not need to be concerned. Emergencies (illness being the major case in point) and retirement follow a predictable pattern for the population as a whole and will not give rise to sudden large liquidations of assets among very many people. It is important therefore that only a minority of savers (23 percent of those with liquid assets of $200 or more) were prepared to say that all their liquid savings are meant for long-term purposes and contingencies, so that they would be unwilling to use them for other things. Seven families in ten expressed willingness to make occasional use of their savings account for discretionary consumption purposes: durable goods purchases, home improvements, vacations, to pay bills that pile up, or "something extra."

Expressed willingness to draw on the savings account for discretionary purposes increases somewhat with income and the size of the savings accounts, and the kinds of short-run purposes also exhibit some difference (Table 3).

What is significant about these replies is that the observed use of savings accounts to finance a variety of consumption expenditures appears as a stated and accepted policy, subject to defined limits, and not as a course of action which people resort to from time to time on impulse and somewhat against their better judgment.

The role of the savings balance as a revolving fund also emerges from the patterns of deposit turnover that were reported (Table 4). In 1963-64, about 55 percent of families with $200 or more in savings accounts reported withdrawals during the past year, usually one or two; but nearly six out of every seven families with withdrawals also reported deposits during the same year. Of those whose stated policy was not to use their savings accounts for any of the discretionary consumption purposes, only 35 percent made withdrawals during the previous year. Among those who checked three or more discretionary purposes as admissible, about 75 percent made withdrawals (Table 5). Five out of every seven of the "conservative" users also made deposits during the same year, and six out of seven of the "liberal" users did so, underlining the fact that the reduction of balances to finance such consumption outlays is usually a temporary expedient. In some cases the funds probably are accumulated in the savings account in preparation for a particular outlay; in other cases they are replaced by new deposits soon after the money is spent. Among the large group who made one or two withdrawals in the previous year, only 30 percent reported a decrease in their savings balances over the past two to three years; and if retired people and those with income declines are disregarded, the proportion is smaller still.

Any self-discipline shown by savers in making deposits is also relevant here. On this point the survey interviewers asked, "During the

TABLE 3

PURPOSES FOR WHICH FAMILY WOULD BE WILLING TO USE MONEY IN SAVINGS ACCOUNT BY AMOUNT IN SAVINGS ACCOUNT*

PURPOSES FOR WHICH WILLING TO USE MONEY IN SAVINGS ACCOUNT	ALL	AMOUNT IN SAVINGS ACCOUNT					
		$1 -199	$200 -499	$500 -999	$1,000 -1,999	$2,000 -4,999	$5,000 and Over
Not willing to use for any purpose	*4%*	*4%*	*7%*	*5%*	*3%*	*2%*	*3%*
Willing to use for contingencies and/or investment purposes	*93*	*87*	*87*	*95*	*92*	*98*	*93*
For illness or other emergencies	87	83	84	84	87	93	88
To meet monthly living expenses, when necessary	21	17	14	20	21	20	26
To buy a house	38	38	29	40	46	35	39
Willing to use for one or more short-run purpose	*70*	*59*	*68*	*71*	*72*	*69*	*76*
To make major improvements or repairs to house	36	24	32	38	37	40	42
To buy a car or large household items	35	23	34	37	32	34	43
To take a vacation trip	30	19	25	27	31	31	41
To pay bills that occasionally pile up	32	34	35	34	30	27	32
To spend on something extra	13	10	12	9	11	12	18
Total	†	†	†	†	†	†	†
Number of cases	935	144	136	128	123	163	212

* This table is based on families who have a savings account.
† Total exceeds 100 percent in that both long-run and short-run purposes could be indicated

past year or so, did you add to your savings accounts every month, in most months, only a few times during the year, once or twice, or not at all?" The majority of the respondents had made deposits only a few times a year at most. A large minority, though, reported deposits in every month or nearly every month.

The savings account holders were then asked, "Do you add about the same amount each time, or does it differ?" A majority said they made no special effort to deposit a uniform amount, but saved "whatever was left over." But again there was a substantial minority that tried to save or did save a regular sum each payday, and this group included most of the savers who made regular deposits of any size. Overall, the systematic savers[4] comprised 28 percent of all the savings de-

[4] Defined to include those who saved a fixed amount practically every month, or tried to, and those who made regular monthly deposits however variable in size.

TABLE 4

SAVINGS ACCOUNT TURNOVER DURING PAST YEAR BY INCOME*

Savings Account Turnover During Past Year	All	Family Income				
		Under $3,000	$3,000 -4,999	$5,000 -7,499	$7,500 -9,999	$10,000 and Over
No withdrawal, deposit(s)	31%	17%	33%	39%	32%	28%
No withdrawal, no deposit	7	23	9	6	5	4
Withdrew once or twice, deposit(s)	36	17	35	35	35	44
More than two withdrawals, deposit(s)	10	5	5	9	17	10
Withdrew once or twice, no deposit	7	19	12	4	5	4
More than two withdrawals, no deposit	3	13	3	2		2
Not ascertained whether withdrawal or whether deposit	6	6	3	5	6	8
Total	100%	100%	100%	100%	100%	100%
Number of cases	762	87	86	184	163	242
Percent who have $200 or more in savings account	51%	27%	35%	52%	63%	76%

* This table is based on families who have $200 or more in savings account. The majority of families with savings accounts have more than one account. The questions in the survey pertain to the liquidity and turnover of savings account balances as a whole; differentiation between the liquidity of different accounts owned by a family was not attempted. Yet for many families activity may be concentrated in one account. In this sense the data pertain to the most active account. Additional accounts such as those owned by children may be considerably less active.

positors and 36 percent of those whose accounts were growing; and apparently both the accumulated balances of these families and their current rate of saving are larger than the average.

Conversion into Securities?

It remains to ask whether savings accounts are liquid in the sense of being available for conversion into corporate stock or physical assets when conditions are opportune. Quite a number of people who now do not own stock or real estate (other than their residence) mentioned stock or real estate as the wisest place to invest money. Their opinion, significantly, accords with Mrs. Projector's observation that holdings of stock and other investment assets typically rise much faster than do savings account balances as one ascends the scale in wealth.

Surveys have shown consistently that, as people's savings increase, they tend to diversify their assets. They like to own some stock in addition to, not in place of, their liquid assets. The new survey data suggest again that it is primarily new savings that will flow into stock and other investment media. Only 9 percent of savers said that they have been thinking of making a shift in their existing asset holdings (and a

TABLE 5

SAVINGS ACCOUNT TURNOVER DURING PAST YEAR BY NUMBER OF SHORT-RUN PURPOSES FOR WHICH WILLING TO USE SAVINGS ACCOUNT*

Savings Account Turnover	All	Number of Short-run Purposes for Savings Account		
		None	One or Two	Three or More
No withdrawal, deposit(s)	31%	45%	30%	14%
No withdrawal, no deposit	7	11	7	8
Withdrew once or twice, deposit(s)	36	24	37	45
More than two withdrawals, deposit(s)	10	2	10	20
Withdrew once or twice, no deposit	7	7	8	4
More than two withdrawals, no deposit	3	3	2	4
Not ascertained whether withdrawal or whether deposit	6	6	6	5
Total	100%	100%	100%	100%
Number of cases	762	213	381	168

* See footnote to Table 4.

third of these wanted more money in their savings or checking accounts). Surveys made in the first postwar years showed many people reluctant to accept the risks involved in buying securities or real estate. The preference then reported for holdings of unquestionable safety has eroded only slowly during the past decade and a half, and any setback to stock prices such as occurred in 1962 reinforces people's caution.

One might argue that the sharp growth of savings balances, as compared to checking accounts, has reflected a lively interest by the savers in the income potential of their savings, and that therefore the present distribution of funds between fixed-value deposits and marketable securities of investment grade, at least, may be fluid.

Respondents were accordingly asked, "Would you know what happened to interest rates paid on savings accounts during the last year or two?" And, if so, "Has this influenced you in any way in what you have been doing with your savings?" No more than one family in twelve answered yes to the second question. Even among those with incomes over $10,000, only one in ten said yes. The yes-sayers still represent a volume of saving which is large in absolute terms. And the attitudes behind the negative responses obviously require further analysis.[5] Pending this, though, the survey data suggest that savers at

[5] Besides evidence from time series in the Federal Reserve Board's flow-of-funds accounts, there are several cross-section studies suggesting that time deposits in commercial banks, at least, are interest elastic. See, e.g., Edgar L. Feige, *The Demand for Liquid Assets: A Temporal Cross-Section Analysis* (Prentice-Hall, 1964), and the 1962 *Annual Report* of the Federal Reserve Bank of Boston, *Time Deposits in New England.*

large are not ready to be diverted from their present financial policies by moderate changes in yield patterns.

A Permanent Revolving Fund

The question was asked earlier: Are savings account balances a store of value which is liquid in the sense of being subjectively available for discretionary spending, or are they effectively reserved for other purposes? We can now summarize the survey evidence by saying that they are both—not in the sense that one can say *x* percent of funds are meant for discretionary current consumption and *y* percent are long-term reserves, but in the sense that the same funds simultaneously serve both functions. Long-term savings often are used temporarily for discretionary consumption but nearly always on the considered premise that they will be replaced in the account.

The liquidity of liquid assets has been explored previously by a number of analysts who introduced liquid asset holdings as one of the independent variables into multivariate cross-sectional consumption or savings functions.[6] The results have generally shown that, except for people with small or declining incomes, liquid asset holdings at the beginning of the year are not negatively correlated with saving during the subsequent twelve months. Morgan and Kosobud have found recently that the liquid asset-savings relationship is positive among home owners, but negative among the smaller group of renters (after allowing for other factors). These findings, though they seem to deny that overall the ownership of liquid balances stimulates spending, are not unambiguous. For in a cross-section those who have relatively large reserve funds have been the more active savers in the past, perhaps as a result of habit, better methods of handling family finances, personality factors, or the presence of particular incentives to save.[7] The finding that these people continue to be large savers may reflect, in part at least, our inability to control certain interpersonal differences in attitudes toward saving. In view of this qualification, the results of the present study, which approached the problem in an entirely different way, are of particular importance. They confirm the earlier impression that liquid reserves—and particularly savings balances—under ordinary conditions are not dissipated on a longer-term basis for discretionary purposes, even though they often serve as a means of financing in the short run.

[6] For example, J. N. Morgan in L. R. Klein (ed.), *Contributions of Survey Methods to Economics* (Columbia Univ. Press, 1954); L. R. Klein, "Estimating Patterns of Savings Behavior from Sample Survey Data," *Econometrica*, Oct., 1951, pp. 438-54; R. F. Kosobud and J. N. Morgan (ed.), *Consumer Behavior of Individual Families Over Two to Three Years* (Survey Research Center, Univ. of Michigan, 1964), p. 110.

[7] For a further discussion of this problem see George Katona, "On the So-called Wealth Effect," *Rev. of Econ. and Statis.*, Feb., 1961, pp. 59-60.

DISCUSSION

JOHN M. CULBERTSON: These very constructive and ably done studies provide information that will be useful in a number of connections. The particular task to which I shall devote my limited time is to consider critically the authors' interpretations of the implications of these studies for theory and policy. We like to think that the growing complexity of the apparatus of economic research, as represented in econometric studies, advanced statistical methods, elaborate formal models, and sample surveys, measures a growing power and productivity of research. However, it is not inconceivable that this apparatus can make economics less fruitful, by serving not as the means to tight inductive inference but as a substitute for it. The ceremonial impressiveness of research procedures may divert attention from their irrelevance or emptiness. The use of impressive ceremony is man's characteristic means of supporting or invoking belief. Modern research procedures may be regarded as deriving their belief-supporting powers largely from their ceremonial impressiveness, in this being the present-day counterpart of the chants and charms of the soothsayer, the rituals of priests, and the pretentious obscurity of earlier philosophy.

The scientific method supposedly provides some protection against self-deception and bias in research, but the necessary rules are neither fully developed nor widely applied. To present for testing against the body of experimental evidence a maintained hypothesis that is specifically stated, derived without bias, and compatible with the generality of prior knowledge is as uncommon as it is difficult. Accordingly, we must consider that our techniques for producing elaborate research may have outpaced our techniques for assessing its meaning, for drawing inferences from it. Thus, even with such interesting and able studies as these, it is important to be at pains to question whether—taken together with the existing body of theory and factual knowledge—the studies mean what the authors say they mean. For the two that I consider, I must conclude that the authors' interpretive frameworks and the inferences that they draw on the basis of them are not above criticism.

The Mueller-Osborne paper poses a set of ambitious but not closely defined questions regarding the place of liquid assets in macroeconomic theory. But a question such as, "Are savings balances liquid enough to serve as money?" is perhaps too vague to permit of a meaningful answer. General information argues persuasively that there is some substitutability for holders between money and liquid assets. Additional knowledge perhaps must take the form of a quantitative specification of the degree of substitutability, under various specified terms and conditions. It is not clear to me that the paper makes a substantial contribution in this direction.

The paper's principal question of reference seems to be this one: "If savings balances really take the place of money nowadays in the minds and operations of the holders, might these balances some day fuel or even provoke a wave of impulse spending, or a stock market boom?" The conclusion reached

is that enlarged holdings of liquid assets "imply a threat to economic stability . . . only under extreme conditions."

This conclusion, as an inference from the evidence adduced here, seems open to three objections: First, with reference to an essentially quantitative question, both the conclusion and the reasoning by which it was reached are so lacking in quantities—and even units in which quantities might be stated—as to be almost devoid of substantive content. Second, if liquid assets pose a threat of unwonted economic expansion, presumably this would arise in a situation of cumulative interaction of increases in spending, income, prices, and expectations regarding these. The responses given in this survey do not seem a reliable measure of how people actually would behave in such a situation.

The third point is that the consideration perhaps most importantly affecting the stability effects of liquid assets seems to be omitted from the interpretive framework of this study. This is the characteristic behavior of the amount of liquid assets in the face of changed attitudes towards the holding of them. Contrary to the argument of this paper, it is possible that an economy is more stable the more liquid assets people initially hold and the more disposed they are to finance speculative spending from this source. What is necessary is only to stipulate that the liquid assets in question be of a type extinguished by use, as savings accounts generally are. Since money characteristically is not extinguished by its use, the more heavily people rely on liquid assets to finance speculative spending, the more rapidly their aggregate liqudity position will deteriorate and the more rapidly any spending upsurge will be halted.

Perhaps these objections sufficiently indicate that the questions approached by this study require the application of a more fully developed theoretical framework, and that the conclusions reached by the authors are not supportable on the evidence given and may be misleading. Perhaps I should add a final point of some practical significance. Although the paper asserts that the rapid growth of certain liquid assets poses an urgent interpretative problem, in truth, total liquid assets when measured in relation to GNP seem to be practically unchanged over the past nine years.

The Morgan-Barlow-Brazer paper finds implications for tax policy in the results of a survey study. Again, I must express some doubt that the evidence and argument adduced adequately support the authors' conclusions. Tax policy presumably should be based upon a set of estimates of the long-run consistency of alternative tax programs with the aims and purposes of our society. To this estimate, the immediate opinions and even the immediate actions of people most directly affected by policy are not generally a decisive consideration. If a poll disclosed that corn farmers agreed that corn ought to be supported at $8.00 a bushel, this would not likely persuade us to adopt such a policy. If Negroes in a region practicing discrimination did not volunteer opposition to it, this would not likely persuade us that discrimination is an acceptable social institution in a liberal society. Similarly, I question that the responses of high-income persons to this survey are a decisive consideration in forming a judgment as to what tax system in the long run will best serve the purposes of our kind of society.

So far as the choice among tax programs depends upon an estimate of their economic effects in some narrower sense, what is relevant is quantitative estimates of responses, taking account of indirect consequences and of revisions of social values and behavioral norms in response to an altered opportunity field. This study has some relevance to the making of such an estimate, but to my mind its implications are more uncertain and limited than the authors take them to be.

Specifically, I find it disturbing that the policy conclusions are so indefinite and the process by which they are arrived at seems similarly subjective. The authors say they have found "rather convincing evidence that most upper-income people in their decisions about work and about investing are not much concerned about escaping taxes." Just what does this mean? What is rather convincing? How many are most? How much is much concern? Or, again, to take the first of the three "major tax policy implications of our findings": "that distributional objectives may be pursued a long way through progressive income taxation without fear of encountering appreciable costs through creating work disincentives." How far is a long way? Have we yet gone a long way, or more than a long way? What incentive effects should properly be considered appreciable?

I find it disturbing also to become aware that if I had been the one to go through the study results to characterize them and draw conclusions at a similar level of vagueness, the words that I used probably would have struck listeners as involving rather different policy implications. On these grounds, my judgment is that the stated conclusions of the paper do not follow in any tight way from the research, and the effect that the paper will have upon the prevailing state of belief regarding tax policy may or may not be a constructive one.

To return to a general theme, both of these very able pieces of survey research, done by eminent students of this method, seem to share the defect of lacking a framework of applied theory that was explicit, well developed, and supportable in view of the existing body of knowledge. Such inadequacy of specification of terms of reference and theoretical framework is not uncommon in modern empirical work. I wonder whether at its present stage of development survey research should not join statistics and econometrics in their basic rethinking of the validity of their procedures of inductive inference.

George F. Break: The three papers just presented all relate to empirical studies of major significance, and as good preliminary reports should, they both present findings of immediate usefulness and stimulate the reader's desire to see the final products. Only then will it be possible to appraise their results realistically and fully, but there seems little doubt that fiscal and monetary specialists will find much to interest them. In the meantime, I should like to discuss some of the implications for tax policy that emerge, particularly from the Morgan-Barlow-Brazer paper.

First, with regard to income tax reform, the authors note that surprisingly few of the high-income individuals interviewed appeared to have a strong monetary investment in the continuance of most of the well-known, and much

discussed, loopholes in the tax structure. This information comes at a time when tax reform may well be facing a major dividing point. On the one hand, concerted attention to its manifold intricacies in the recent past has so far produced little action. Many ardent reformers may consequently be weary of the task. On the other hand, Senator Long proposed last fall an improved and expanded version of his optional simplified tax system which has a number of attractive features. In congressional discussions of this plan, therefore, tax reform may well return to the limelight once again. It could hardly happen at a more opportune moment. If it is true, as many believe, that federal tax receipts will, at high levels of employment and output, continue to outrun federal expenditures, policy-makers will face a recurring choice between income tax reduction, on the one hand, and the mobilization of federal taxing powers in support of state and local governments, on the other. To those who feel, as I do, that education, health, and welfare programs merit high social priorities, the second alternative has obvious advantages, and these would be materially enhanced by a rationalization of the federal income tax base. With these reforms in hand, states could more realistically be urged, in the interests of efficiency, to tie their own income taxes more closely to the federal base and to expand, perhaps under the stimulus of a federal tax credit, the role of income taxation in their revenue structures. In addition, federal grants-in-aid could be expanded, either on a categorical or on some less restricted basis, with fewer fears that gross interpersonal tax inequities would be perpetuated in the process. Should structural reforms not be made, however, federal income tax reduction might well receive majority support, and state and local governments, being deterred by interstate competition from taking up much of the slack, might then find themselves even more financially hard pressed than they now are.

Advocates of federal tax reduction lay much stress on the dangers to work and investment incentives posed by existing high tax rates. The Morgan-Barlow-Brazer study, however, will clearly show these fears to be largely illusory. From reading their paper, one gets the happy impression of their respondents as a hard-working group of high-income people for whom material rewards are far from being everything and who have adjusted to high taxation by living above its burdens. Few of their actions have been strongly tax motivated, and such perennial tax victims as the locked-in investor and the illiquid decedent are apparently much less in evidence than one would have guessed. An important result of these findings should be to free the hands of the government whenever strong inflationary pressures are the major fiscal problem. No longer can it be argued that higher income tax rates must be rejected as a solution because of their disincentive effects. Instead, both tax and monetary policy can be brought to bear on the problem in accordance with their relative merits.

A third important tax problem concerns the treatment of capital gains and losses. I agree with the authors that the law should provide for constructive realizations at death. Along with interest on state and local government securities, capital gains accrued until the end of the taxpayer's life provide a wide avenue for tax avoidance, especially at the highest income levels where the

ownership of such assets is widespread. Both of these loopholes distort investment decisions, and it is worth noting that the second apparently robs the first of some of its few redeeming features. Having numerous opportunities for tax-free capital gains, high-income investors are likely to be less interested in tax-exempt bonds, and state and local governments must therefore seek a wider and less receptive audience for their securities. As a result, interest rate differentials between taxable and nontaxable bonds are less than they otherwise would be, and the federal tax subsidy benefits private investors more and state and local governments less. Officials of these governments, therefore, would be well advised to support increased taxation of long-term capital gains. I would only urge that tax reforms of this kind be based on an analysis of the effects of both the corporate and the personal income taxes. Under the latter, capital gains are clearly undertaxed, but a separate tax on corporate income, unless it is completely shifted, will partially offset many of these tax differentials and actually reverse others.

Finally, let me return to a problem touched on at the beginning of my remarks; namely, the social value of further investment in education. Not only was saving to finance the education of their children an important characteristic of the Survey Research Center's high-income group, but this motive was mentioned much more frequently by those who had themselves received the most extensive formal education. One implication of this finding is clear. By increasing its educational services a society sets in motion a chain reaction which not only raises the productive capacities of the current generation (the gains from which have been well documented in recent empirical studies) but through the carryover effect just noted will increase the educational attainments of future generations. If for these and other reasons more investment in education is needed in this country, future expenditure and tax policies of the federal government will have to be increasingly oriented to the financial needs of state and local governments.

LAWRENCE E. THOMPSON: The FRB survey, which Mrs. Projector described, rounds out the postwar line of small-sample interview surveys which have sought to determine the pattern of family ownership of investment assets. By concentrating on the top-wealth group this survey clarifies the picture where it was most blurred, and undoubtedly it will stand as the reference on that group for a long time. I believe that Mrs. Projector's use of a wealth elasticity concept to measure investor preferences is also new in this field. The estimates which she computed led her to the conclusion that common stock and a class of "other investments" are strongly preferred to other types of assets, and it is that interpretation which seems to me to require some qualification.

Ordinarily we are inclined to think of wealth as the cumulant of savings flows. In that context the elasticity results for common stock imply that the share of savings invested in stock increases with wealth and is larger at each successive wealth level than the stock-wealth ratio which prevailed at the beginning of the savings period. The implausibility of that interpretation of Mrs. Projector's estimates is suggested, however, by the fact that funds flows

accounted for only $10 billion of the $267 billion increase in the value of stockholdings of all individuals which took place in the ten years immediately preceding the Fed's survey. The remainder, of course, was accounted for by the 9 percent growth rate of stock prices during that period.

Many of the respondents in this survey undoubtedly experienced a similar gain in wealth from the appreciation of their stock investments. And since they were asked to value their holdings in current market prices, it follows that the observed positive correlation between wealth and share of wealth held in common stock must be attributable in some unknown (but probably large) part to capital gains rather than the investment of new funds. To put it differently, the proper inference may run, on balance, from prior common stock investment decisions to the level of wealth rather than from wealth level to decisions to commit new funds to common stock.

The same can probably be said for the greater-than-unity elasticity estimates of the "other investments" category, which includes income-producing real estate and closely-held businesses.

Mrs. Projector has expertly broken down the elasticity estimates into additive components representing frequency of ownership and amount of ownership, but unfortunately the latter component cannot be taken apart on the basis of survey cross-sectional data to show the relative contributions of capital gains and the investment of new funds. It may be argued that this is not important for a preference interpretation of elasticity estimates, since the decision to continue to hold an appreciating asset is prima facie evidence of a continuing, strong preference for that asset. On the other hand, it should be recognized that these specific elasticity estimates have little explanatory or predictive power where interest is centered on investment flows.

The Morgan-Barlow-Brazer study, by reason of purpose and method and curiosity concerning possible changes in the responsiveness of investors to income and estate taxation, invites some comparison of findings for today's high-income families with those which we reported for investors in 1950. Tax changes, economic growth, some inflation, and methodological differences might have been expected to produce some striking contrasts, but a comparison of the two reports led me to the conclusion that investor behavior is not much more or much differently responsive to taxes now than then.

The 1950 study was undertaken at a time when tax rates were being held at record peacetime levels and many observers were seriously concerned with a probable "drying-up" of the supply to business of outside equity financing. Experience since then has confirmed the study's main conclusion that those fears for investors' behavior were badly exaggerated, and at least two important reasons for that conclusion, as might have been hoped, stand out again in the 1964 survey.

We were impressed as the S.R.C. interviewers evidently were, by the variety and number of motivations which affect investment decisions. Even though high tax rates have continued to be an important determinant of take-home gains, individuals generally are "not dominated by precise calculations of monetary gain" in their economic activities.

The second reason is evident in the heavy emphasis which respondents

gave in 1964 to capital gains. Those who are particularly sensitive to their tax situation may regard the capital gains-ordinary income rate differential as a stimulant rather than deterrent to risk investments, including business equities. The tax structure has some inherent balance in that sense, and the nature of that balance is one crucial factor to be taken into account in weighing a key policy recommendation which emerges from the paper—to eliminate the present capital gains-ordinary income rate differential.

In 1950 the balance between stimulating and deterring effects on the willingness of investors to hold and commit new funds to risky investments seemed quite precarious. The new evidence suggests, however, that today's investors are basically more appreciation-minded than investors were fifteen years ago, and, consequently, this preliminary report shows little concern for that old issue. But when one moves on to the question of specific proposals for eliminating the rate differential—for example, to bring the two rates together at current levels of ordinary income taxation or at lower compromise levels—it is clear that attention will have to be given again to the balance of opposing tax influences. Evidence from other sources indicates that there are still conservative high-income investors around who are primarily attentive to the safety and after-tax yield of their investments. And on the other side, it is difficult to believe that the capital gains investment objective would remain as strong and widespread as it appears to be in the absence of the rate differential. These groups would be affected differently by such proposals, and their reactions should be expected on balance to produce quite different effects in markets for risk and fixed-value assets.

The Mueller-Osborne paper addresses one of the hottest controversies in the monetary policy field, and while one may balk at their subjective definition of liquidity, it is difficult to quarrel with their assertion that the regulatory questions which are at issue should be resolved with full knowledge of the deposit and withdrawal behavior of savings account owners. Their study will be welcomed because it does contribute new information on the attitudes and, to a lesser extent, the actions of that group.

On the other hand, one may hope that this survey will be followed by others which will concentrate on the owners of large and active accounts, not only because the issues which are at stake demand information on account owner behavior at different points of the cycle, but also because the need for a sharpshooting approach is as strong here as it was in the wealth and tax fields.

To back up that point, let me cite a few figures from another study which started with the account ledgers of all banks and savings and loan associations in a major metropolitan area and then moved to the owners of sampled accounts for income and wealth data and attitudinal information. We found that more than 75 percent of the residents of the area owned one or more regular savings accounts. On the other hand, an ownership concentration breakdown of those holdings showed that four-fifths of the total was held by less than one-fifth of the account-owning families. The same point can also be made with reference to the growth of the area's total accounts during the 1957-61 period covered by the study. Families who held one or more accounts

throughout that period owned 88 percent of total account balances outstanding at the end of 1961—which establishes the importance of that particular group. The surprising fact is that only one-eight of the members of the group were responsible for more than 80 percent of the net growth of the group's total accounts.

I believe that findings of this nature will prove to be reasonably representative of other major metropolitan areas, and therefore I am inclined to reserve judgment on the authors' conclusions which are most exposed to the divergent behavior of a small fraction of all savings account owners. In that respect I would be much less surprised to see savings accounts fuel a "securities market boom" than a "wave of discretionary spending."

RECENT CAPITAL AND PRODUCTION THEORY

ON SOME RECENT DEVELOPMENTS IN CAPITAL THEORY*

By ABBA P. LERNER
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I

In 1936 Keynes introduced the term "marginal efficiency of capital," substituting the word "efficiency" for the word "productivity." His declared purpose was to obtain a measure that could be compared with the rate of interest, i. He did this by considering values rather than physical quantities [4].

At least as important was the undeclared shift from considering the stock of capital to considering the flow of investment and its influence on the level of employment. This led me to substitute "marginal efficiency of investment" for his marginal efficiency of capital, leaving the term "marginal productivity of capital" to represent the effects on output of having a greater stock of capital [5]. I would now like to suggest a slight change in terminology which would clearly show both distinctions, using four terms instead of two: the marginal productivity of capital (mpK), the marginal productivity of investing (mpI), the marginal efficiency of capital (meK) and the marginal efficiency of investing (meI).

MpK is the extra flow of output resulting from a unit increase in the stock of capital. But this is not comparable with i because the units of output may not be the same as the units of capital. It is possible to make mpK commensurable with i by measuring both the capital and the output by their value either in dollars or in output, which we can call apples, but this would obscure the distinction between the effects of a change in the size of the capital stock (K) and a change in the rate of investing (I).

We can isolate these different influences by distinguishing between apples and apple trees, so that mpK stands for the extra potential apples per annum from one extra tree. This diminishes with K, the number of trees already in existence, because the more trees there are the less favorable is the best site remaining available for planting yet another tree.

* Comments and questions by Jack Hirshleifer, Dale Jorgenson, Arthur Lerner, and Robert Solow are responsible for some significant clarifications.

MpI is the extra capital produced by diverting resources from making one unit of consumption goods to making capital goods. It is also not comparable with *i*. The greater is *I*, investment, the rate at which trees are being planted (or *S*, saving, the rate at which apples are being sacrificed) the smaller will be *MpI* because more use will have to be made of resources that are relatively less efficient at planting new trees than at coaxing apples out of the already existing trees. *MpK*, too, will diminish as *I* increases because fewer resources are left to help trees produce more apples.

We get a rate of return to compare with *i* by multiplying together the two marginal productivities. If a tree yields 100 apples per annum (net), *mpK* is $\frac{\text{100 apples per annum}}{\text{1 tree}}$. If it takes the sacrifice of 1,000 potential apples to plant an additional tree, *mpI* is $\frac{\text{1 tsee}}{\text{1,000 apples}}$. The product is $\frac{\text{100 per annum}}{\text{1,000}}$ or 10 percent per annum.

MeI is this rate of return which in (perfectly competitive) equilibrium will be equal to *i*. A positive *I* will then indicate that *K* is less than that appropriate to *i*; namely, that *meK* is greater than *i*. In long-period equilibrium *K* would be adjusted, *meK* would be equal to *i*, and *I* would be zero. *MeK* may, therefore, be defined as *meI* when $I = 0$.

These relationships are illustrated in Figure 1 and Table 1, in which position (1) represents an initial long-period equilibrium, position (2) a new short-period equilibrium following a reduction in *i*, (3) a long-period equilibrium corresponding to that lower *i*, and (4) a new short-period equilibrium following a further reduction of *i*. The *meI* curves are downward sloping because $meI = mpI \times mpK$ and both of these diminish as *I* increases.

The downward slope of the *meK* curve may seem to be more questionable. While *mpK* may be expected to fall as the ratio of capital to other factors increases, *mpI* is as likely to rise as to fall in response to an increase in *K*. If *mpK* falls less in making capital goods than in making consumption goods, giving up a unit of consumption sets free enough resources to make more extra capital goods than it did before the increase in *K*. *MpI* will have increased!

But the new capital goods will suffer exactly the same decline in their *mpK* in making consumption goods. The greater number of trees that can be planted when 1,000 potential apples are sacrificed is exactly offset by the greater decline in yield of potential apples per annum

per extra tree. As long as there is any decline in *mpK* in making capital goods, as *K* increases, *meK* will be downward sloping.

II

The peculiar nature of capital in relation to time in the productive process and how this applies just as much to the services of capital as to land or labor services is shown in Figure 2. Time is measured along *AB*. The increasing distance from *AB* of a point moving along the line *AC* represents the cumulative application of "original" or "noncapital" factors of production—land-labor services (hereafter called "labor"), beginning at *A* and concluding at *B*. The line *BC* represents the emergence of the product at time *B*. The total length of the process is *AB* and the average time between the application of the factor and the
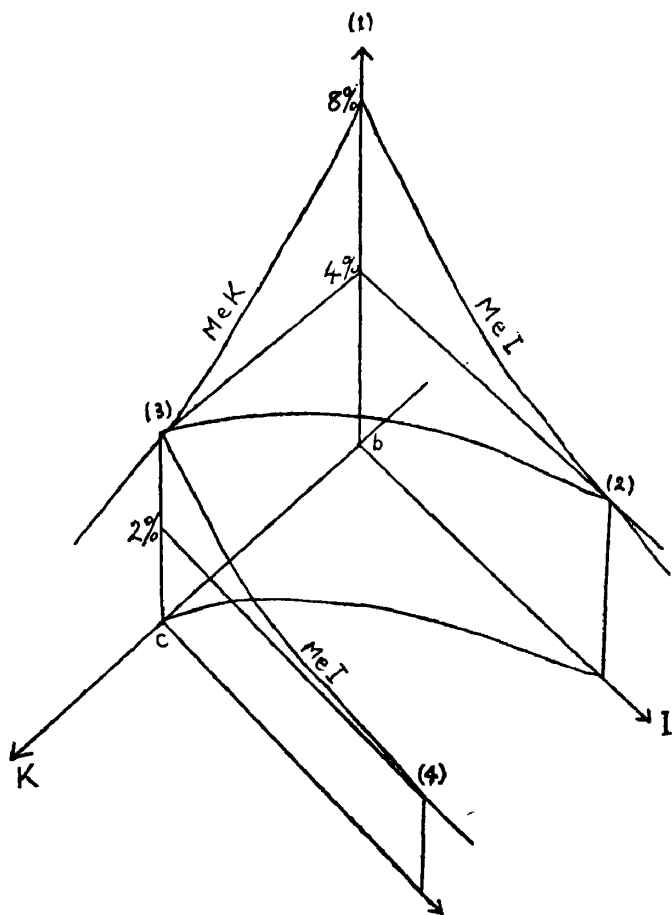
FIGURE 1

TABLE 1

		(1)	(2)	(3)	(4)
i	Rate of interest	8%	4%	4%	2%
K	Capital stock (number of trees)	2,000	2,000	6,000	6,000
O	Output (potential apples per annum)	1,000,000	1,000,000	2,100,000	2,100,000
C	Consumption (apples produced and consumed per annum)	1,000,000	700,000	2,100,000	1,500,000
S	Saving (O–C) (potential apples sacrificed per annum to produce trees)	0	300,000	0	600,000
I	Investing (trees produced per annum, net)	0	40	0	48
O/K	Average productivity of capital (potential apples per annum per tree)	500	500	350	350
mpK	Marginal productivity of capital (extra potential apples per annum per extra tree)	400	400	240	240
I/S	Average productivity of investing (trees planted [per annum] per apple sacrificed [per annum])	—	1/7,500	—	1/12,500
mpI	Marginal productivity of investing (extra trees planted [per annum] per extra apple sacrificed [per annum])	1/5,000	1/10,000	1/6,000	1/12,000
p_k	Price of capital goods ($=1/mpI$ in short period equilibrium) (price of a tree in apples)	5,000	10,000	6,000	12,000
meI	Marginal efficiency of investing or rate of return ($\equiv mpK \times mpI$) ($=i$ in short period equilibrium) (extra apples per annum per extra apple sacrificed from consumption)	8%	4%	4%	2%
meK	Marginal efficiency of capital or rate of return in long period equilibrium ($\equiv meI$ when $I=0$)	8%	8%	4%	4%
K'	Value of the capital stock (value of the trees in apples)	10,000,000	20,000,000	36,000,000	72,000,000
K'/O	"Capital-Output Ratio"	10	20	$17\frac{1}{7}$	$34\frac{2}{7}$
$\Delta K'/\Delta O$	"Marginal Capital-Output Ratio"		∞	$14\frac{6}{11}$	∞

emergence of the product is LB. If AC is a straight line, LB will be half of AB. The total quantity of capital, measured by its labor content, will be represented by the area of the triangle ABC or the rectangle $LMCB$.

If $i > 0$, the values of the flow of consumption goods and of the stock of capital goods will exceed the values of the labor services incorporated in them. If we represent the value of a unit of land-labor by the vertical distance AA' we can show its growth as, with the passage of time, it turns into a unit of output. In equilibrium this growth in value must correspond to the rate of interest or discount. The unit of labor applied at time A has an initial value AA' which grows at a rate equal to i, reaching B'' at the end of the process. The other units of labor, since they are applied later, do not reach so high a value. The values they do reach are traced out by the curve $B''C'$. The growth in value may be conceived of as due to inputs of capital services; i.e., as resulting from the reinvesting from moment to moment of the continuous increase in the value of the goods in process during the production period. The causal influence is, of course, the other way around. All inputs are put in only because of the expectation of a greater value of future outputs.)

The total value of output is represented by the area $BB''C'C$ at the further end of the figure, of which the rectangle $BB'CC'$ represents the payment for land-labor, the remainder $B'B''C'$ being the return to capital. The value of the capital stock is represented by the complete solid figure $ABCB''$. $AA'BB'CC'$, the lower slice of this figure, represents the land-labor incorporated in the stock of capital. It may be considered as corresponding to the "wages fund," or "rent and wages fund," and we may call this "land-labor capital." The rest of the figure, the upper part $A'B'C'B''$, represents capital that must be invested in addition to the initial payment for the land-labor because it is impossible for the capital providers to start consuming all the interest on these (wages and rent) payments from the moment they make them. We may call this part of the capital the "interest fund" or "capital capital."
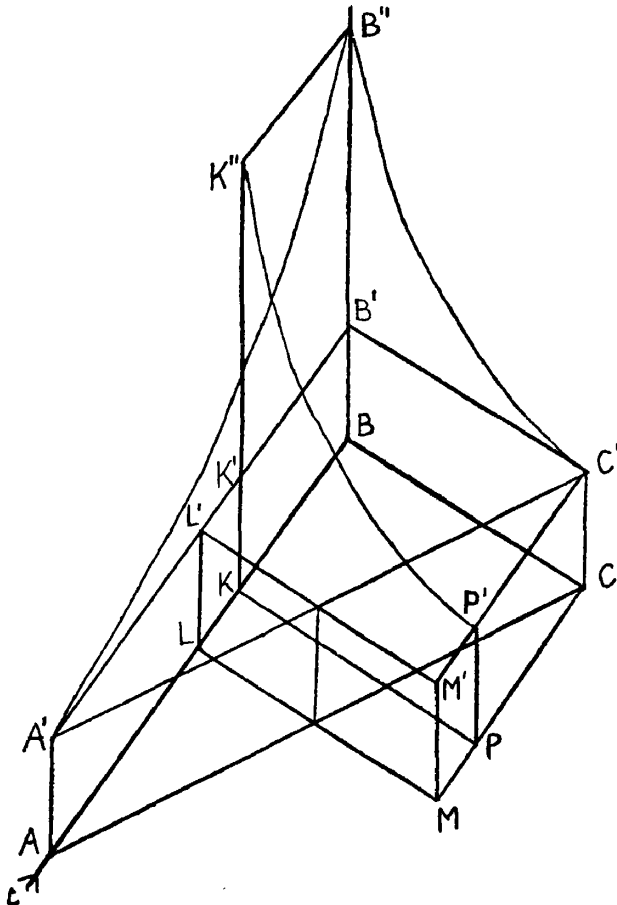
FIGURE 2

The average period of production is now represented by *KB*. The point *K* is obtained by moving the plane figure at the end of the diagram back along *BA* towards *A* up to *K*, where the volume it will have moved through, the slice between the congruent parallel planes *BB″C* and *KK″P*, will be just equal to *ABCB″*. KB will be less than *LB* because the greater weight of capital services comes closer to the end of the process when there are more capital earnings that must be reinvested as capital services.

It should be noted that the average period of production, which is also the capital-output ratio and the measure of capital intensity, is not independent of the rate of interest or the price of waiting, just as it is not independent of the relative prices of land and of labor. Without the relative prices it would be impossible to have a single measure of input.

III

I now turn to "post-Keynesian" capital theory—the development, mostly in Cambridge, England, of models of economic growth centering largely, in its present presumably introductory efforts, on steady states, often called "golden ages," in which everything grows at the same rate so that all the current ratios are steady. The "Keynesian" element is the direction of influence from investing to saving rather than the other way around, as in Keynes's insistence on this direction of influence via employment and the multiplier. The difference which makes it post-Keynesian is that full employment is usually assumed. *I* influences *S* by increasing not employment but profits. The additional spending that constitutes the investing is received and saved by the capitalists. If they spend some of this, no less is saved, because that only increases their income enough to make the *S* seem appropriate.

As one who certainly has spilled not too little ink in the "saving equals investment" spree of the 1930's and 1940's, I should have had no difficulty with this kind of argument. But I experienced severe discomfort which I traced to an unreadiness to accept the implied reduction in real wages. The rather startling response to my expression of these difficulties in Cambridge was "of course, if you bring in the inflation barrier the whole process is stopped and we just have inflation." The trouble was clearly that my own post-Keynesianism rests essentially on the belief that most economies today are at or beyond the inflation barrier. Nevertheless, it seemed incumbent on me to try to overcome my provincialism and make the assumptions necessary to follow the analysis.

This was not easy. One difficulty was the reversal of the roles of capital and investment. I tend to consider as natural a short-period

model in which the quantity of capital is given by history. Together with the current techniques it governs the current investment opportunities. The problem is to achieve the desired level of employment, perhaps with some consideration, among other social objectives, of a desired rate of growth.

Post-Keynesian capital theory does all this the other way around. The rate of growth of everything is set by the rate of growth of population and the technical progress, since it is only when everything else has been completely adjusted to these that a steady state can be achieved. A greater degree of thrift—meaning that a larger fraction of the output is saved and invested—thus does not show itself in a more rapid rate of accumulation of capital or of economic growth. It shows itself only in a higher level of capital stock in relation to output which just absorbs the higher rate of saving and investing in growing at the predetermined steady rate of growth of everything.

But after many painful exercises have got one into the swing of thinking in terms of states of steady growth, some interesting things begin to emerge. It now makes sense to isolate the effects of an increase in K from the effects of an increase in I by holding I, or rather I/K, constant not at zero but at g the rate of growth of a steady state. In particular, what may justify the pains is that technical progress can be incorporated in the main structure of the model instead of being dragged in as an afterthought.

IV

If technical progress is neutral as between the use of capital and the use of labor, it fits into g, the steady state rate of growth, in exactly the same way as an increase in labor. A 1 percent per annum rate of such neutral technical progress means that each succeeding year the same number of men working with 1 percent more machinery can produce 1 percent more output including the 1 percent greater output of machines required by the 1 percent increase in the number of machines operated per working man. It is just as if the working population grew at an additional 1 percent per annum, except that per capita wages and incomes now rise.

However, some interesting questions arise as to how it is appropriate to define neutrality of technical progress—the line between capital-using and capital-saving technical progress. Here the field has been narrowed to competition between J. R. Hicks and R. F. Harrod.

Hicks's definition is enticingly neat, "equiproportional increases in the marginal productivities of capital and labor"[1] and works ideally for an individual producer who can freely change the number of men he hires or the number of machines he rents or buys and will not

be induced by Hicks-neutral technical progress to change the ratio in which he combines them.

But society as a whole cannot vary the ratio between the quantities of capital and labor. It can only vary the uses to which they are put. If the marginal productivities of labor and of machines both increase by 10 percent, an additional unit of labor used in the production of machines produces 10 percent more of these, and each machine in turn produces 10 percent more of its product. The marginal product of labor used indirectly is then increased twice by 10 percent; i.e., by 21 percent. Labor will, therefore, be shifted from direct use, where its marginal product increases by only 10 percent, to indirect use, where the technical progress—the increase in marginal productivity—is "multiplied" by the number of stages in the economic process. Similarly, labor will be shifted from indirect uses to still more indirect uses. This is what constitutes the substitution of capital for labor.[1]

Harrod's definition of neutral technical progress—"technical progress that results in no change in the capital-output ratio for the same rate of interest"[2]—gives us neutrality with respect to the substitution between direct and indirect labor. By avoiding any reference either to the marginal productivity of capital or to physical quantities of capital, it avoids the traps that surround these concepts. If the marginal productivity of direct labor increases by 10 percent, Harrod-neutrality would be satisfied by a 10 percent increase in the productivity of men making machines with no increase in the marginal productivity of machines; by an unchanged marginal productivity of men or machines in making machines with a 10 percent increase in the marginal productivity of machines making final goods, or by some intermediate changes that add up to a 10 percent increase of productivity in the indirect manufacture of final goods just as in the first two cases.

In all three cases, K' the value of the capital stock K (measured in terms of output) will increase in the same proportion as output. In the first case, each batch of 100 retired machines is replaced by 110 of the new kind, each new machine having the same capacity and the same marginal cost as one of the old machines. In the second case, the number of machines does not change, but each new machine has a 10 percent greater capacity and a 10 percent greater marginal cost. The third case is intermediate between these two. The only difference between the three cases is in the degree to which the 10 percent increase in the value of the stock of capital goods is due to a change in the number of machines and the degree to which it is due to a change in the value of

[1] Hicks may not have intended to identify the marginal productivity of capital with the marginal productivity of capital goods, but only on this interpretation is Hick's neutrality different in content from Harrod-neutrality.

each machine. The increase, decrease, or stationariness of the capital output ratio—the Harrod criterion—will correctly indicate capital-using, capital-saving, or neutral technical progress.

V

Harrod, feeling that a measure of technical progress should hold "the quantity of capital in some sense" constant if it is not to "ascribe to technical progress some element of the increase of output which is better ascribed to capital accumulation," suggests that the proper measure of the quantity of capital for this purpose is not its value in terms of consumption goods but "the length of the productive process" defined as "the average time of waiting multiplied by the number of man hours (or other non-capital factors of production as currently valued in terms of man hours) in respect of which there is waiting"[3]. This means the average period of production of our "labor."

But technical progress can also affect the productivity of capital services, changing the structure of production and altering the value of the capital stock, the average period of production and the capital output ratio, even though the average period of production of labor has not changed.

This is shown in Figure 3 (which reproduces part of Figure 2). The broken lines show a shift in the input stream from the middle to both ends of the production process, the added area Q being just equal to the subtracted area R, and $BB'CC'$, the labor content of the output, and $AA'BB'CC'$, that of the capital stock, are unchanged and so is LB labor's average period of production. But the value of the output is increased because the addition S (the accumulated value of Q) must exceed the subtraction T (the accumulated value of R), while the value of the whole capital stock, including the "capital capital," must increase still more since it must grow in the same proportion as $B'B''C'$, the interest on it. Since that increases by the same absolute amount as total output $BB''C'C$, it will increase in a greater proportion. The overall average period of production and the capital output ratio will therefore increase from KB to say K^*B. Harrod's restriction of "the average time of waiting" to "noncapital" factors of production is unjustified. We must recognize that at any moment the capital goods and their services are just as "given" as the labor force and the land and their services.

VI

I have so far followed the convention of treating capital as a stock and labor as a flow. But the legitimization of capital services on a par with labor opens the way to a more pleasingly general approach which treats every flow of a productive service as yielded by a stock of the

source of that flow, and also reduces the differences between the Hicks and Harrod neutrality to a difference between the private and the social points of view that lie behind them.

In microeconomics we consider a producer who buys or hires his factors of production on the market. If he uses only flows of services to produce a flow of output, he simply gets the difference between the two flows of payments for these as the payment for the flow of his personal services. He requires no capital to run such a business.

Capital comes into the picture only if the producer wants to own some of the sources of the flows of services. Even if he lets others own all the buildings and machines, etc., and buys their services (or rents the sources) he will normally find it necessary to own some goods in process which incorporate past services which will contribute to future output. They constitute working capital and must also be considered

FIGURE 3

as sources of flows of productive services. We may then define private capital as sources owned.

The more of the sources he is to own, the greater the amount of capital he must have or borrow. A capital-using invention from his point of view is one that induces him to shift from the use of services that he buys to the use of services from sources that he owns, for which he needs capital. This will happen whenever the marginal productivities of the services of owned sources rise relatively to those of bought services. This fits Hicks's formula that a capital-using invention raises the marginal productivity of capital relatively to the marginal productivity of labor, etc., remembering that by capital we mean any sources that the producer owns and that by labor we mean any productive services that he buys.

But the economy as a whole cannot change the proportions between sources owned and sources rented. It can only change the uses to which the given flows of services from the given stock of sources are directed. In particular, it can and will shift them away from the manufacture of producible sources whose marginal productivity relatively to their marginal cost has fallen and toward the manufacture of producible sources where this has risen.

From the macroeconomic point of view, the significant distinction is not between services flowing from sources owned and services bought from others but between sources that are producible and sources that are not. Producible sources may then be called "social capital." From the social point of view a technical improvement is capital using if it diverts current services from producing current consumption goods to increasing the stock of producible sources, capital saving if it diverts current services in the opposite direction, and neutral if it does neither. Harrod's device—his use of the capital-output ratio (for a given i) as an indicator of the bias in technical progress—accurately reflects the initial direction of movement of the marginal productivity of indirect relative to that of direct use of resources.

VII

There is, however, a third and more natural because more direct indicator of the bias in technical progress. This is none other than a change in our *meI*, the marginal efficiency of investing.

MeI has been defined in terms of potential consumption goods: the future flow of consumption goods made possible per unit reduction of current consumption via the shifting of current services from making current consumption goods to making (producible) sources. A change in *meI* is, therefore, a change in the relative marginal productivity of indirect as compared to direct use of services flowing from all kinds of

sources: labor, land or capital, producible or nonproducible. It may even be read as the marginal efficiency of indirectness in the use of productive services of all kinds.

An increase in *meI* (which = *mpK* × *mpI*) may be the result of an increase in *mpK,* the marginal productivity of (the services of) producible sources in making consumption goods. But it may also come about from an increase in *mpI* by virtue of an increase in the *mp* of services in general (including nonproducible sources) in making producible sources relatively to their productivity in making consumption goods. In the first case, *meI* takes the form of an increase in the prices of producible sources: the tree is worth more because it produces more apples. In the second case it takes the form of a reduction in their marginal cost: it takes a smaller sacrifice in apples to get an extra tree. Combinations of these are, of course, also possible. In all cases an increase, decrease, or absence of change in *meI* (for a given ratio of *I* to *C*) is a proper indicator of whether an improvement is capital using, capital saving, or neutral in the fundamental sense of how it affects the marginal efficiency of indirectness in the use of the currently available flow of services yielded by the currently existing stock of sources of all kinds.

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SUBSTITUTION, TECHNICAL PROGRESS, AND RETURNS TO SCALE*

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I. *Introduction*

In 1957 Professor Solow published an essay that has had a significant impact on the analysis of economic growth [7]. In this study Solow separated the growth effects of increased labor input, capital accumulation, and shifts in the aggregate production function. Stated differently, he determined the relative importance of capital deepening and of technical progress in augmenting output per man. His analysis was based upon an aggregate production function of the form

$$Y = A(t)F(K, L), \tag{1}$$

where Y, K, and L represent national income, capital stock, and labor respectively. $F(K, L)$ is assumed to be homogeneous of degree one; and the term $A(t)$ represents Hicks-neutral technological progress.

From (1) it follows that

$$_1G_A = G_y - S_K G_k, \tag{2}$$

where G_x denotes the rate of growth of x, S_K is the property share, and y and k represent the output-labor and capital-labor ratios, respectively. The left subscript is used to distinguish among growth equations. Assuming (1) takes the Cobb-Douglas form, Solow found that technical progress or shifts of the aggregate production function accounted for about 90 percent of the increase in output per man. Employing approximately the same techniques, Massell [5], Niitamo [6], and others confirm this result.

Many economists interpreted these studies to mean that capital accumulation or capital deepening has only negligible importance in economic growth.[1] If this conclusion is accepted, another follows inevitably: capital theory is unimportant relative to an understanding of the forces causing shifts in the production function or technical progress. Solow's conclusions and the implications derived from them, however, are based upon certain assumptions that have recently been called into question. First, equation (1) assumes all technical progress is "dis-

* The calculations reported in this paper were financed by an Auxiliary Research Award from the Social Science Research Council. Correspondence with Robert Solow has been of great help, but he is not to be held responsible for the end product.

[1] Solow himself is certainly not in this category; see [8].

embodied" in the sense that it can occur in the absence of gross investment. Solow was among the first to suggest that technical progress is actually embodied in new capital equipment and, therefore, that capital accumulation is essential to economic growth [8]. This is indeed an important qualification, if not of Solow's original result at least of the interpretation placed upon it; however, it is an aspect of the problem not discussed here.

Other potentially important qualifications are the subject of this paper. In particular, the foregoing analysis of growth in output per man is based upon the explicit assumptions of constant returns to scale and of Hicks-neutral technological progress. Brown [3] and Walters [9] have recently presented evidence indicating the American economy is actually subject to "increasing returns to scale" (or, the aggregate production function is homogeneous of degree $m>1$).[2] Next, Brown [3] and Ferguson [4] found that American manufacturing industry has been characterized by Hicks-biased technological progress during most of the present century. Either of these forces separately, or both jointly, tend to diminish the importance of technical progress relative to the increase in labor input and capital accumulation.

II. *Biased Progress and Returns to Scale*

First assume the aggregate production function is homogeneous of degree m, not necessarily equal to one, and is characterized by Hicks-neutral technological progress. This function may be written

$$Y = A(t)H(K, L). \tag{3}$$

The counterpart of equation (2) thus becomes

$$_3G_A = G_y - (m-1)G_L - \left(\frac{kh'(k)}{h(k)}\right)G_k. \tag{4}$$

Under certain assumptions concerning distribution, (4) is the same as (2) except for the term $-(m-1)G_L$. For $m>1$ and $G_L>0$, G_A is correspondingly smaller and the importance of technical progress is less. On the other hand, if there were "diminishing returns to scale" or a decrease in employment (but not both), the importance of technical progress would be augmented.

Next, suppose the production function is homogeneous of degree one but is subject to Harrod-neutral technological progress. In one fairly general form the function may be written

$$Y = F^*[K, A(t)L], \tag{5}$$

[2] The meaning of this phrase, as applied to the economy as a whole, is questioned in Section IV.B. It is used throughout, however, as a shorthand for the phrase "the aggregate production function is homogeneous of degree $m>1$."

and the counterpart of equation (2) is thus

$$\frac{Af_A^*}{f^*}\,{}_2G_A = G_y - S_K G_k, \tag{6}$$

where a subscript to functional notations represents the first partial derivative of the function with respect to the variable indicated.

Finally, if the aggregate production function is homogeneous of degree m and technological progress is Harrod-neutral, the function may be written

$$Y = H^*[K, A(t)L]. \tag{7}$$

The growth equation thus becomes

$$\frac{Ah_A^*}{h^*}\,{}_4G_A = G_y - (m-1)G_L - \frac{kh_k^*}{h^*}G_k. \tag{8}$$

Equations (6) and (8) may yield estimates that differ from the corresponding Hicks-neutral equations. One reason for difference is the multipliers Af_A^*/f^* and Ah_A^*/h^*; another reason lies in the difference between the F and F^*, H and H^* functions. In fact, however, the corresponding estimates turn out to be almost exactly the same (see Table 3).

III. *Empirical Results*

A. The Statistical Models. As previously noted, the original studies of technical progress were based on the Cobb-Douglas function. However, Solow and others [2] have developed a more general function that is just as susceptible of statistical testing, while not being subject to the statistical bias inherent in Cobb-Douglas estimates. The new function, a constant elasticity of substitution (CES) function, is used throughout, although some estimates based upon a restricted Cobb-Douglas equation are given.[3]

The general form of the CES function for Hicks-neutral technological progress is[4]

$$Y = Ae^{\lambda t}[\delta K^{-\rho} + (1-\delta)L^{-\rho}]^{-m/\rho}, \tag{9}$$

[3] Let γ and β denote the labor and capital coefficients or their relative shares. Let μ be the geometric mean of the ratio of relative shares. The estimates are restricted in that we set $\beta=\gamma\mu$. The estimates are denoted RCD.

[4] The mathematical form is about the same whether the function is homogeneous of degree one or m. Let the function be $y=L^{m-1}f(x)$. Setting the elasticity of substitution equal to the constant $1/(1+\rho)$ gives the following second-order, nonlinear differential equation:

$$\frac{f'(mf - xf')}{(m-1)xf'^2 - mxff''} = \frac{1}{1+\rho}.$$

where ρ, δ, λ, and m may be called the substitution, distribution, technology, and homogeneity parameters, respectively. If m is constrained to equal unity, (9) is homogeneous of degree one and is denoted OCES. When m is not restricted, (9) is homogeneous of degree m and is designated MCES. If Harrod-neutral technological progress is assumed, the CES function changes slightly, becoming

$$Y = A[\delta K^{-\rho} + (1 - \delta)(e^{\alpha t}L)^{-\rho}]^{-m/\rho}. \tag{10}$$

As one may readily see, in the CES form Harrod-neutral technological progress is (Hicks-biased) labor-using or labor-saving according as $-\rho\alpha \gtrless 0$.

After a little manipulation, (9) and (10) yield the statistical models used in this study. An important characteristic of the models is that (9) and (10) result in identical regression equations. Hence there are no grounds in statistical theory for choosing between the alternative hypotheses of Hicks-neutral or Harrod-neutral technological progress.

The regression model for the OCES function is

$$\log y = {}_1b_0 + {}_1b_1 t + {}_1b_2 \log w, \tag{11}$$

where w is the wage rate and left-hand subscripts distinguish between regressions. The b's are the statistical parameters to be estimated by least squares, and the economic parameters are calculated from the b's according to the following relations:

$$\sigma = {}_1b_2; \qquad \lambda = \frac{{}_1b_1}{1 - {}_1b_2}; \qquad \alpha = \frac{{}_1b_1}{1 - {}_1b_2}, \tag{12}$$

where σ denotes the elasticity of substitution.

While the mathematical forms of OCES and MCES functions are very similar, the statistical models are quite different. For m-homogeneity, the regression model corresponding to (9) and (10) is

$$\log Y = {}_2b_0 + {}_2b_1 t + {}_2b_2 \log w + {}_2b_3 \log L. \tag{13}$$

The economic parameters are estimated as follows:

$$\sigma = \frac{{}_2b_2}{{}_2b_3}; \qquad m = \frac{{}_2b_3 - {}_2b_2}{1 - {}_2b_2}; \qquad \lambda = \frac{{}_2b_1}{1 - {}_2b_3}; \qquad \alpha = \frac{{}_2b_1}{{}_2b_3 - {}_2b_2}. \tag{14}$$

Equations (11) and (13) comprise the statistical models to be tested.

B. Equations for Technical Progress. The general growth equations based upon the general production functions (1), (3), (5), and (7), were derived in Sections I and II above. When the specific CES func-

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Two quadratures yield

$$f(x) = L^{-m}[\delta K^{-\rho} + (1 - \delta)L^{-\rho}]^{-m/\rho}.$$

Since $y = L^{m-1}f(x)$ and $Y = yL$, these two substitutions yield equation (9).

tions are used, the growth equations usually also assume a more specific form. However, in the OCES case with Hicks-neutral technological progress, the growth equation is precisely the same as (2):[5]

$$(15) \qquad {}_1G_A = G_y - S_K G_k.$$

When technological progress is Hicks-neutral and the MCES function is used, the growth equation is

$$(16) \qquad {}_3G_A = G_y - (m-1)G_L - \left[\frac{m\delta k^{-\rho}}{\delta k^{-\rho} + (1-\delta)}\right] G_k.$$

Finally, if technological progress is Harrod-neutral, the growth equations are as follows for the OCES and MCES functions, respectively:

$$(17) \qquad \left| - \frac{(1-\delta)e^{-\rho\alpha t}}{\rho[\delta k^{-\rho} + (1-\delta)e^{-\rho\alpha t}]} \right| {}_2G_A = G_y - S_K G_k,$$

$$(18) \qquad \left| - \frac{m(1-\delta)e^{-\rho\alpha t}}{\rho[\delta k^{-\rho} + (1-\delta)e^{-\rho\alpha t}]} \right| {}_4G_A = G_y - (m-1)G_L - \frac{m\delta k^{-\rho}}{(\delta k^{-\rho} + (1-\delta)e^{-\rho\alpha t})} G_k.$$

Since S_K is a time variable when $\sigma \neq 1$, evaluation of equations (15)–(18) involves the choice of an appropriate date at which to measure the time variables. One could measure these variables, for example, at the beginning or end of the period, or he could use an average for the entire period. The difference, in fact, is not particularly material.

C. Statistical Results. Equations (11) and (13) were fitted to OBE data covering the periods 1929–63 and 1948–63.[6] The results are summarized in Table 1, where the figures in parentheses beneath the regression coefficients are their standard errors. The computed magnitudes of the economic parameters are shown in Table 2.

It is clear from Table 1 that the MCES function gives a better fit for the entire period 1929–63; but it is equally clear from Table 2 that the economic parameters for the MCES function are more or less absurd. Both λ and α are negative, implying technological retrogression. This is to be expected in light of the homogeneity parameter (2.53), because a doubling of inputs alone would account for more than a sixfold increase in output. At this rate of expansion, no "room" is left for techno-

[5] This does not mean, however, that calculations based upon different forms of this general class of functions yield identical statistical results.

[6] The data used are as follows: (*a*) national income in manufacturing industry, and (*b*) compensation of employees in manufacturing, (*c*) both deflated by the implicit price deflator for goods output; (*d*) number of full-time equivalent employees in manufacturing; and (*e*) privately owned structures and equipment in manufacturing establishments in constant dollars deflated by an index of unemployment.

TABLE 1

SUMMARY OF REGRESSION RESULTS

	Function	$_tb_1$	$_tb_2$	$_tb_3$	$\bar{R}^2$
1929–63	OCES	.005 (.003)	.67 (.27)		.89 (.002)
1929–63	MCES	−.0004 (.0002)	.71 (.15)	1.44 (.06)	.99 (.0006)
1948–63	OCES	−.003 (.002)	1.16 (.32)		.95 (.0001)
1948–63	MCES	.0003 (.006)	.72 (.45)	1.13 (.21)	.98 (.0001)

logical progress. This explains, of course, why the OCES model yields consistently higher values of λ and α.

Other marked regression differences concern σ and m. OCES yields higher estimates of the elasticity of substitution, while MCES shows materially higher values for the homogeneity parameter. The two parameters are, in this sense, statistical substitutes.

As a rough check of homogeneity, an RCD function was fitted to the data for both periods. For the years 1929–63, $\gamma+\beta=1.54$ and $\gamma+\beta \geqq 1.25$ at the 0.05 level. Over the shorter period of time, $\gamma+\beta=1.20$, but the sum is not statistically different from unity.

If "economies of scale" exist, the phenomenon should be observed most clearly when the economy passes from one relative peak to another. Accordingly, MCES and RCD functions were fitted to nine peak observations over the 1929–63 period. The "expected" results were obtained. For the MCES function, $b_3=1.13(\pm 0.07)$, $b_2=0.95(\pm 0.27)$, and $\bar{R}^2=0.999(0.0001)$. The calculated value of m is 3.61; and it is presumably highly significant. Similarly, the RCD regression resulted in $\gamma=1.34(\pm 0.06)$, $\beta=0.22(\pm 0.07)$, and $\bar{R}^2=0.99(0.001)$. Thus $\gamma+\beta=1.56$ and the sum is statistically significant.

Despite the statistical fit, one conclusion seems inevitable. On economic grounds one must reject the MCES function for the period 1929–63; i.e., technological progress must be equal to or greater than zero. A similar conclusion applies to the 1948–63 period. Taking only years of

TABLE 2

ESTIMATED MAGNITUDE OF ECONOMIC PARAMETERS

Period	Function	σ	m	λ	α
1929–63	OCES	.67	1	.015	+.015
1948–63	OCES	1.16	1	.019	+.019
1929–63	MCES	.49	2.53	−.001	−.0006
1948–63	MCES	.64	1.45	.001	+.0007

increasing Y from 1948 to 1963, the MCES function yields a computed m of 0.47 and $\gamma+\beta$ from the RCD function is only 1.08. These results can be explained largely in terms of the time movement of the regression variables. Nonetheless, homogeneity of degree one certainly appears to be the relevant hypothesis when time series data cover a relatively short period.

D. The Importance of Technical Progress. The relative importance of technical progress in explaining the growth of output per man depends upon the assumptions concerning homogeneity and technological progress. However, the calculation is somewhat more sensitive to the former than to the latter. The computational method is the same for Hicks-neutral and Harrod-neutral progress, except the latter introduces a "multiplier." Table 3 shows the percentage of the change in output per man attributable to technical progress under the alternative assumptions discussed.

TABLE 3

PERCENTAGE CHANGE IN OUTPUT PER MAN ATTRIBUTABLE TO TECHNICAL PROGRESS

Period	Function	Type of Neutral Progress	Percentage
1929–63	OCES	Hicks	.99
1929–63	MCES	Hicks	.12
1948–63	OCES	Hicks	.93
1948–63	MCES	Hicks	.76
1929–63	OCES	Harrod	.98
1929–63	MCES	Harrod	.11
1948–63	OCES	Harrod	.95
1948–63	MCES	Harrod	.76

These estimates offer some range of choice, depending upon the period selected and the assumptions involved. In a sense, therefore, the importance of capital theory is also open to a range of choice. For reasons given quite sketchily below, I personally prefer the OCES Hicks-neutral function for the 1929–63 period and the OCES Harrod-neutral function for 1948–63. Using these two functions, one reaches the following conclusions: technological change has, on balance, been positive during both periods; and technical progress has accounted for more than 90% of the increase in output per man. The remaining growth is attributable to other elements. As would be expected, this is about the same as Solow's original results.

IV. *Returns to Scale and the Nature of Technological Progress*

A. Neutral or Biased Technological Progress? Since the CES regression model is the same for Hicks-neutral and Harrod-neutral technological progress, one cannot choose between these two hypotheses on

grounds of statistical theory. Nonetheless, there are considerations which, in certain instances, enable one to make a logical selection. The relevant relationships are summarized in Table 4. They apply both to the OCES function and to the MCES function for $m>1$.

A priori, "technological retrogression" has not characterized the American economy. Hence Hicks- or Harrod-neutral technological progress is not plausible when λ and $\alpha<0$. Accordingly, cases (*b*) and (α) must be excluded. In the other two situations, cases (*a*) and (β), this criterion cannot be used; λ and α are positive and provide no basis

TABLE 4

HICKS-NEUTRAL AND HARROD-NEUTRAL TECHNOLOGICAL PROGRESS IN A CES FUNCTION

$\sigma < 1$	$\sigma > 1$
(*a*) if $b_1 > 0$: $\lambda > 0$, $-\rho\alpha < 0$	(α) if $b_1 > 0$: $\lambda < 0$, $-\rho\alpha < 0$
(*b*) if $b_1 < 0$: $\lambda < 0$, $-\rho\alpha > 0$	(β) if $b_1 < 0$: $\lambda > 0$, $-\rho\alpha > 0$

for choice. Yet in certain instances empirical evidence pertaining to the movement of relative input shares can serve as a guide.

Let us agree that the average wage rate in manufacturing industry has increased relative to the rate of return on capital. First, consider case (*a*) in which the elasticity of substitution is less than unity. *Ceteris paribus*, one would expect an increase in the relative share of labor over time. If a decrease in labor's share were actually observed, it would seem reasonable to attribute the decline to labor-saving (Harrod-neutral) technological progress.[7] On the other hand, if the elasticity of substitution is greater than unity [case (β)], one expects the share of property to increase. If the labor share is augmented instead, one might plausibly impute the movement to labor-using (Harrod-neutral) technological progress. It is this consideration that dictated my choice of Harrod-neutrality for the OCES function describing the 1948–63 period.

Finally, if the expected and observed movements of relative shares are consistent, there seems to be no basis for choosing between the Hicks-neutral and Harrod-neutral hypotheses in cases (*a*) and (β).

B. Increasing Returns to Scale? The statistical results presented here, as well as those of Brown [3] and Walters [9], clearly show the statistical superiority of m-homogeneity in studies covering long periods of time. Thus two questions arise: (*a*) why does a production function

[7] Strictly speaking, Harrod-neutral progress requires constant relative shares since the definition stipulates a constant rate of return and a constant capital-output ratio. Needless to say, these precise requirements are violated by time series data.

homogeneous of degree m provide a better statistical fit than a corresponding function homogeneous of degree one and (b) can one interpret the statistical result $m>1$ to imply "increasing returns to scale"?

Statistically, it is easy to understand why m-homogeneity yields superior results. When the "disembodied progress" form of the OCES function is used, all qualitative changes are necessarily represented by an expression such as $e^{\lambda t}$ or $(1+\lambda)^t$. Either term moves smoothly and monotonically, while output per man (the regressand) is neither monotonic nor smooth. It sometimes declines; and when it advances, it does so by fits and starts, perhaps reflecting the uneven flow of inventions. Since the wage rate tends to move almost monotonically, there is no regressor to reflect downward movements in output per man. As a consequence, the time series fit is not particularly good.

When the MCES function is used, value added (Y) becomes the regressand and employment (L) enters the calculation as a regressor. The dips in Y are reflected in dips in L and dramatic, sudden increases in Y are usually accompanied by corresponding changes in L. The latter becomes a significant regressor, and its regression coefficient is crucially important in determining the computed value of m. The L-series, roughly speaking, becomes proxy for qualitative changes that do not follow a smooth, regular time pattern.

For widely fluctuating statistical series, it is easy to see why m-homogeneity provides a better statistical fit. Furthermore, the reason for its statistical superiority when widely separated "peaks" are used should be clear. But the homogeneity parameter can be statistically too "efficient," swamping out technological progress entirely. In these cases one must reject m-homogeneity on economic grounds. But even if accepted, one would be seriously mistaken to regard "economies of scale" as an explanatory term in this context. I do not think it is possible to identify the statistical result $m>1$ with "increasing returns to scale" in the technical sense. To do so simply distorts the meaning of another term well defined and well established in our jargon. Increasing returns to scale means that a proportional expansion of all inputs leads to an expansion of output by a greater proportion. The relationship holds at an instant of time and applies only to a situation in which the character of inputs does not change. This is a far cry from the conditions that prevail when one analyzes the aggregate economy or the manufacturing sector by means of time series data.

In aggregate studies we heroically lump all inputs into two broad categories, capital and labor. If one uses cross-section data or time series data covering a relatively short span, there may not be particularly significant changes within or between these two input classes. But over substantial periods of time, important qualitative changes occur in the

composition of these aggregates. First, the specific inputs within each class change. Even more important, perhaps, the quality of what might seem to be the "same" input changes. Replacement of equipment, even without positive net investment, allows technological advances to be embodied in new capital goods. Similarly, there are a wide variety of improvements in intermediate goods and in the labor force. All these changes tend to augment output or value added without a corresponding quantity or value increase in inputs; and statistically, such changes are not adequately represented by a smooth exponential term in the regression.

Next, the product mix comprising manufacturing output shifts over time, as do relative output prices. These changes may also contribute to the statistical result $m>1$, but they are certainly not "economies of scale." In summary, then, the following conclusions seem to emerge: (*a*) in aggregate studies covering long periods of time, homogeneity of degree one is likely to provide economically more meaningful results, though these results may be statistically less significant than in the case of m-homogeneity; and (*b*) homogeneity of degree $m>1$ should not be interpreted to mean that the aggregate economy is subject to increasing returns to scale; the statistical result doubtless has a meaning, but it is not one that can accurately be described by a short, simple phrase.

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DISCUSSION

RALPH W. PFOUTS: For a reason other than the obvious ones, I am a poor discussant of Professor Lerner's paper because I am largely in sympathy with the points that he makes. Consequently I do not feel a motivation to oppose his views—at least not in the fiery and unrestrained manner that seems to be considered appropriate for discussants.

Indeed, with regard to the golden-age models, I am inclined to feel that Professor Lerner erred in being too easy in his criticism of them. As Lerner explains, the economic structure in the golden-age models adjusts to the rate of growth of population and of technological progress. These two are usually represented as time series; i.e., as functions of time. Hence so long as the economic mechanism equates the rate of profit to the rate of growth, everything is fine and the economy follows a path predestined by two time series, which are not explained.

If we view the construction of such models as either an oversimplified pioneering effort or as a purely theoretical inquiry, I see no reason to raise any objections. After all, theoretical inquiry even under assumptions that have never been valid and that never will be valid is a perfectly legitimate type of investigation, and so long as we keep the golden-age models in this perspective. I fail to see any way to object to them.

But I think it must be recognized that the golden-age models are at a different level of realism from the earlier Keynesian models. The Keynesian models pointed the way toward study of problems of stagnation, under full employment equilibrium, the liquidity trap and so on, while examination of the golden-age models might lead one to believe that problems of attaining full employment have little relation to economic growth. This is ironic in the face of concern in Washington about the backlog of unemployment and its adverse effects on the rate of economic growth. Indeed it would seem that the Keynesian model would come closer to describing and analyzing the present difficulties of economic growth than would many of the post-Keynesian models.

Actually the golden-age models strike me as an eccentric development that would have been difficult to anticipate. Who would have expected that a neoclassical resurgence would have manifested itself in a type of growth model which contains a coincidental resemblance to marginal productivity theory? In the face of the then prevailing Keynesian climate, I would not have expected such a development.

Instead, I would have expected that extensions of the Keynesian models would have been made. I would have anticipated that efforts would be made to broaden the scope of Keynesian theory. The effects of various price levels might, for example, have been brought into efforts to dynamize and expand the Keynesian analysis. Indeed the place of money in the Keynesian framework might well have been rethought. I would have expected such efforts to take precedence over the development of the golden-age models, but they have not.

Professor Lerner's remarks on the problem of defining neutral technological

ogress seems to me to raise questions rather than settle them. His rejection the Hicksian definition of neutral technical progress seems to be ısed on an artificial distinction between the uses of labor and capital. If bor can be used in producing both consumer goods and capital goods, then, Lerner points out, if the productivity of both labor and capital increase, e productivity of labor used in making capital is increased by the fact of e increase in capital's productivity. Since it is not reasonable to think of pital as being employed in the production of labor, an asymmetry exists beeen the two.

It is by no means clear that there is any operational asymmetry. If we lmit the possibility that capital may be used in producing both consumer ods and capital goods, then surely the two factors of production can be eated in a symmetric way. Capital used in producing capital goods has its oductivity magnified in the same way as does labor. Thus the possibilities of ing the Hicks definition seems to be entirely within reason. For Lerner's iticism to stick, it is necessary to assume that none of the existing stock of pital can be diverted from manufacturing consumer goods to manufacturing pital goods.

Indeed, the appropriate definition is the one which gives the most accurate rmal representation of our intuitive notions on the subject. It is really on is grounds that one should rally to one definition or the other. It seems to e that "equiproportional increases in the marginal productivity of capital ıd labor" come closer to describing the notion of neutral technical progress ıan does a constant capital-output ratio with a fixed rate of interest.

Actually it may be reasonable to suggest that a defect of the Harrod finition is that it treats capital and labor in an asymmetric fashion. Thus e would be reversing the criticism made by Lerner.

Even if we oppose Lerner's choice on conceptual grounds, it must be admitd that in a practical sense there is something to be said for his preference. he Hicks definition is couched in concepts that are more appropriate to the alm of microeconomics, while the Harrod definition is stated directly in agegated terms. This in turn probably means that we can come closer to using e Harrod definition in actual measurements. But even here actual measureent is extremely difficult, the requirement of a fixed rate of interest being ıfficient by itself to cause considerable difficulty.

Finally, the definitions of the marginal efficiencies and the marginal proıctivities which Lerner offers should be mentioned. When one reads these finitions simply as verbal statements, they seem reasonable and, very likely, ıeful. Yet they are a bit disturbing, because it is not at once obvious how ıey fit into the entire scheme of things. This problem could be solved if Ler-r would formulate these definitions as mathematical functions, because then ıe independent variables would be identified in each case and the relations of ıch to general economic models would be clarified.

MARVIN FRANKEL: We have come a long way since Professor Dougs asked, in the title of a well-known article, "Are There Laws of Proıction?" New ways of viewing output-input relations have been discovered, ore sophisticated measurement methods have been devised, and experimen-

tation with production functions, especially in recent years, has taken place at a high rate. But these developments have not brought any generally agreed answer to Douglas' question, and it is debatable whether they have increased much our knowledge of the actual economic forces at work.

Professor Ferguson would extend the menu of choices already before us. His point of departure is a CES function with neutral technical change, and he seeks to allow first for (Hicks) nonneutral technical change and second for increasing returns. I sympathize with the direction of this effort but am skeptical of his treatment at certain points and uncertain of the meaning that should be given certain findings.

In each of the two regression equations in the paper, the wage rate appears as a variable. Use of the variable rests on the assumption of profit maximization under competition along a constant returns to scale production function. Under these conditions, labor gets its marginal product. This rationalization of the wage rate variable is lost when the scale paremter, $m > 1$, is introduced. If labor gets its marginal product, the return to capital must be less than its marginal product if total product is to suffice. Given only modestly increasing returns and plausible values for the other parameters, I should think it possible that labor's share could more than exhaust total product. In the context of regression analysis, under increasing returns, this means simply the lack of a defensible series for representing the marginal product of labor.

This difficulty aside, if one is prepared to forego, as increasing returns requires, the theoretic virtues of a linear homogeneous production function, then the door is open to wide-ranging experimentation. There is no need, and there may be no particular advantage, in preserving a constant elasticity of substitution. The CES function becomes but one of many candidates, with the possibilities limited only by the experimenter's imagination and problem orientation. Such flexibility is perhaps preferred if one's object is close fits and reliable parameter estimates for forecasting purposes.

Table 3 of the paper offers figures on the fraction of change in output per man attributable to technical progress. The figures are based on the growth equations which, strictly speaking, hold only for a point in time. Where, as in the present case, the interval covered is an extended one, there is a joint particle and hence a partitioning problem. The procedure underlying Table 3 appears to be one that implicitly causes the whole of the joint particle to be absorbed into the technical change variable which, in the growth equations, is derived as a residual. Thus the figures in the table cannot be validly compared without first knowing the relative importance of the joint particle in each case.

In explanation of the relatively inferior fit to the 1929-63 interval of an OCES function, Ferguson refers to the more or less inflexible, monotonic behavior of the wage rate (the regressor) in face of the comparatively volatile movements in output per man (the regressand). I think it should be emphasized that this is a comment on the adequacy of the data and on the fitting procedures we are obliged to use rather than on the inherent worth or "truth" of the constant returns hypothesis. If, that is, the wage rate did not tend to be institutionally rigid, or if we were able to discover and use in our regression work a statistical series that accurately mirrored marginal product move-

ments, then we might well find that the constant returns function gave a much better fit.

For production functions of the type used in the present paper, the interpretation of statistically derived parameter estimates for technical progress and increasing returns poses, it seem to me, formidable problems. Some of these have been stressed by Professor Ferguson, and I should like to comment further on them. Many things happen over time more or less concurrently, though not necessarily coincidentally, with the changes in output and in capital and labor inputs. The resource base changes, the organization of productive activities changes, and the qualities of the factor inputs change and tend to improve. Outside the sector of one's concern, which in the present case is manufacturing, changes occur which may tend to augment or inhibit the productivity of that sector. Components of each of these several types of changes may, for particular intervals, occur monotonically, or irregularly, or coincidentally with changes in capital or labor inputs. To the extent that they occur monotonically, they will tend to be reflected in the parameter for time. To the extent that they occur coincidentally with increases in the inputs of labor and capital, they will tend to be picked up by the increasing returns parameter. If their impact is irregular, but nonrandom, in character, their influence cannot, without more information, be guessed. Presumably either the time or increasing returns parameter, or both, could be affected.

Consider a few possibilities. All technological change might be embodied and its effects manifested coordinately with factor input increases. Or it might be embodied but with its consequences for productivity realized quite gradually over time as enterprises gained experience in the use of new methods. Increasing returns might emerge coordinately with changes in factor inputs, or they might be realized slowly and with considerable lag as industries sought to adapt and adjust to higher levels of activity. Changes outside the manufacturing sector that affect the latter's productivity might follow closely manufacturing and aggregate output or they might resemble more an autonomous long-term force.

Such possibilities as these are not, I think, implausible ones. Since we know little of the specific content or relative importance of the various changes or of the extent to which they may reenforce, complement, or offset one another, we cannot give distinctive economic meaning to the two parameters in question. For all we really know, the familiar labels might be reversed without doing violence to the truth. The argument for this view is yet stronger when one remembers how aggregative a production function for the manufacturing sector is. Must one, then, as Professor Ferguson contends, and other considerations aside, reject the increasing returns function on economic grounds? Are there in fact any economic grounds? One need not and probably should not interpret the negative trend parameter in this function as technical retrogression. All we know is that the neutral component, which is the resultant of a composite of ill-defined forces, takes on small negative values in both the Hicks and Harrod versions. This outcome may not be an easy one to explain, but neither may it be impossible.

In any event, considering the nature of the problems and the state of the art in this area, we need not drive ourselves to making seemingly hard choices.

TECHNOLOGICAL CHANGES: STIMULI, CONSTRAINTS, RETURNS

RATES OF RETURN FROM INDUSTRIAL RESEARCH AND DEVELOPMENT*

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I. *Introduction*

Although recent years have seen a notable increase in the amount of attention devoted to the economics of research and development, no attempt has been made to use econometric techniques to estimate the marginal rate of return from R and D expenditures in individual firms and manufacturing industries.[1] The enormous difficulties, both conceptual and practical, in making estimates of this sort are all too obvious. Nonetheless, it is important that the task be begun, since business firms and government agencies badly need better measures of the return from R and D.

This paper presents some results which are both preliminary and tentative. Having cursed the dark loudly and publicly,[2] my purpose here is to stimulate discussion of the problem and to light a few candles, limited though their power may be. Section II shows how the marginal rate of return from R and D expenditures can be estimated, assuming that a simple model of production holds and that all technical change is organizational. Section III presents numerical results for a small number of manufacturing firms and industries. Section IV shows how such estimates can be obtained, assuming that all technical change is capital-embodied, and Section V presents numerical results based on this assumption. Section VI discusses the findings and concludes the paper.

II. *Organizational Technical Change: Model*

To begin with, I assume that all technical change is organizational

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1 For agricultural research, Griliches [2] [3] has made some estimates of the rate of return. Ewell [1] has made some rough estimates for the entire economy. However, no estimates have been made for particular manufacturing industries or firms. For some estimates of payout periods, see [9].

2 See Mansfield [6].

and that the production function for a particular firm is

$$Q(t) = Ae^{a_1 t}\left[\int_{-\infty}^{t} e^{-\lambda(t-g)}R(g)dg\right]^{a_2} L^{\alpha}(t)K^{1-\alpha}(t), \tag{1}$$

where $Q(t)$ is the output rate (in 1960 prices) at time t, $L(t)$ is the labor input at time t, $K(t)$ is the stock of capital (1929 prices) employed at time t, $R(g)$ is the rate of expenditure (1960 prices) on R and D by this firm at time g, λ is the annual rate of "depreciation" of an investment in R and D, a_1 is the rate of technical change that would occur even if R and D expenditures (net of "depreciation") by this firm were to cease, a_2 is the elasticity of output with respect to cumulated past net R and D expenditures, A and α are parameters, and time is measured in years from 1960.

Five points should be noted regarding equation (1). First, it recognizes that the firm's efficiency depends on its previous R and D expenditures, as well as on the inventive activity of other firms and nonprofit institutions (the effect of the latter being incorporated in a_1).[3] Second, it allows for the possibility that an investment in R and D, like that in plant and equipment, depreciates over time, because of the obsolescence of research findings and designs.[4] Third, it makes the usual assumption that holding constant the level of previous R and D expenditures (which determines the level of technology at a given point in time), there are constant returns to scale with respect to labor and capital. Fourth, A and α can vary from firm to firm, and a_1 and a_2 can vary from industry to industry.[5] Fifth, the little evidence that is available seems to support equation (1). A study [11] of seventeen chemical firms indicates that this type of production function fits well and that the coefficients are generally statistically significant and of expected size. This evidence is hardly conclusive, but it is all we have.[6]

Next, assume that, for the period up to 1960,

$$R(g) = R_o e^{(\rho-\sigma)g}, \tag{2}$$

[3] For other papers that assume that current output is a function of cumulated past R and D expenditures, see Minasian [10] [11] and Kurz [4]. Apparently, this is the first model that uses a depreciation factor and includes "imported" technical change.

[4] One implication of this model, as it stands, is that the firm must do some R and D just to maintain its efficiency at a constant level. Since it takes some R and D simply to move along a given production function in response to changing factor prices, etc., this may not be too unrealistic. If one believes that this is not the case, he can set λ equal to zero. This makes little difference in the subsequent analysis.

[5] We assume that a_1 and a_2 are the same for firms within the same industry, but this assumption can be relaxed if one part of an industry seemed to differ significantly from another part. In Sections III and V, we assume that a_2 is the same in chemicals as in petroleum. Because the industries are similar in many respects, this seems reasonable. Of course, if more data were available, it would not be necessary to make this assumption.

[6] Note that Minasian's results [11] are based on the assumption that λ and σ equal zero and that his data on R and D expenditures do not go back very far. Moreover, because of the correlation between time and the sum of previous R and D expenditures, his estimates are not very precise when both of these variables are included in the production function. Nonetheless, his study, which is the only one of this sort that has been done to date, seems to point toward the use of equation (1) as a first approximation. For further comments see note 9.

where R_o is the firm's R and D expenditures in 1960, ρ is the rate of increase of R and D expenditures in current dollars, and σ is the rate of price increase of R and D, as well as an allowance for noncomparability of R and D figures over time. According to the available data, R and D expenditures in current dollars rose (approximately) exponentially in most firms during 1927–60. It seems reasonable to assume as a first approximation that R and D expenditures in constant dollars did so, too.

Given these assumptions, what would have been the marginal rate of return from an additional amount spent in 1960 on R and D, assuming that R and D expenditures after 1960 would continue to conform to equation (2)? If an additional expenditure of ϵ had been made on R and D in 1960, the output at a subsequent time t would have been

$$Q(t) = Ae^{a_1 t}\left(\frac{R_o e^{(\rho-\sigma)t}}{\rho - \sigma + \lambda} + \epsilon e^{-\lambda t}\right)^{a_2} L^{\alpha}(t) K^{1-\alpha}(t), \tag{3}$$

and

$$\left.\frac{\partial Q(t)}{\partial \epsilon}\right|_{\epsilon=0} = a_2 A e^{(a_1-\lambda)t}\left(\frac{R_o e^{(\rho-\sigma)t}}{\rho - \sigma + \lambda}\right)^{a_2-1} L^{\alpha}(t) K^{1-\alpha}(t). \tag{4}$$

Thus, if $L(t) = L_o e^{lt}$ and $K(t) = K_o e^{kt}$, it follows that

$$\int_0^{\infty} \left.\frac{\partial Q(t)}{\partial \epsilon}\right|_{\epsilon=0} e^{-rt} dt \tag{5}$$

$$= a_2 A \left(\frac{R_o}{\rho - \sigma + \lambda}\right)^{a_2-1} L_o^{\alpha} K_o^{1-\alpha}$$

$$\times \int_0^{\infty} e^{t[a_1-\lambda-r+(a_2-1)(\rho+\sigma)-\alpha l+(1-\alpha)k]} dt$$

$$= a_2(\rho - \sigma + \lambda)\frac{Q}{R_o}$$

$$\cdot [r + \lambda - a_1 - (a_2 - 1)(\rho - \sigma) - \alpha l - (1 - \alpha)k]^{-1},$$

where Q is the 1960 value of $Q(t)$. We assume that the price of the product will remain constant from 1960 on, and that the elasticity of supply of the R and D inputs to the firm is infinitely elastic. Consequently, since both $Q(t)$ and $R(g)$ are expressed in 1960 prices, we set the right-hand side of equation (5) equal to one, solve for r, and find that

$$r^1 = a_2(\rho - \sigma + \lambda)\frac{Q}{R_o} + a_1 - \lambda + (a_2 - 1)(\rho - \sigma) + \alpha l + (1 - \alpha)k, \tag{6}$$

where r^1 is the marginal rate of return from the extra expenditure in 1960 on R and D.

III. *Organizational Technical Change: Empirical Results*

To illustrate how equation (6) can be used to estimate the marginal rate of return from R and D, I did a pilot study of ten chemical and petroleum firms and ten manufacturing industries. First, let us consider the case of the ten firms. From correspondence with the firms, annual reports, and *Moody's*, I obtain estimates for each firm of α, R_o, ρ, and Q. Assuming that the value of labor's marginal product is set equal to its wage, I use labor's average share of value added in the firm (during 1946–62) as an estimate of α. To estimate Q and R_o, I use data regarding the firm's value added and R and D expenditures in 1960. To estimate ρ, I use the slope of the regression of the natural logarithm of the firm's R and D expenditures (current dollars) on time during 1945–58.

The next step is to estimate $b' = a_1 + a_2(\rho - \sigma)$. Letting

$$V/V = Q/Q - \alpha L/L - (1 - \alpha)\dot{K}/K, \tag{7}$$

it follows from equations (1) and (2) that

$$ln\ V(t) = \text{constant} + b't. \tag{8}$$

For each firm, I obtained data regarding $Q(t)$, $L(t)$, and $K(t)$ for 1946–62.[7] Letting $V = 1$ in 1946, I calculated $V(t)$ for each subsequent year, regressed $ln\ V(t)$ on t, and used the resulting regression coefficient as an estimate of b'. Since the model applies only to periods when the firm is operating at full capacity, I omitted years when output was less than some previous year.

Next, given an independent estimate of a_2, I estimate a_1, assuming that a_2 is the same for all of these firms but that a_1 may differ from industry to industry. Since

$$b' = [a_1 - a_2\sigma] + a_2\rho + z, \tag{9}$$

[7] Note that the data on value added contain an error, which was judged to be too small to be worth correcting. By mistake, they are net of R and D expenditures. However, since R and D expenditures are a very small and relatively constant percentage of value added in these firms (about 5 percent), their omission is very unlikely to have an important effect on the findings. Note too that our estimate of α is very rough. We assume that labor's marginal product is set equal to its wage. (Of course, we could assume that physical capital's marginal product is set equal to its price. The resulting estimate of α would be the same.) However, we cannot assume that the marginal products of both labor and capital are set equal to their prices, because this would leave nothing as compensation for the investment in R and D. Of course, if this assumption seems too unrealistic, there are other ways that α could be estimated.

where $\hat{b}'$ is our estimate of b' and z is the sampling error it contains, it follows that

$$a_1 = [\hat{b}' - a_2\rho] + a_2\sigma + z.$$

An independent estimate [15] of a_2, based on data for seventeen chemical firms, is .11, the standard error being .01. To check this estimate,[8] I regressed b on ρ, and found that the regression coefficient, .12, was very close to .11, the difference not being statistically significant. Setting a_2 equal to .11,[9] the average value of $(\hat{b}' - a_2\rho)$ for our ten firms is .013, and there is no evidence that it differs significantly between industries. Thus, omitting z,

$$a_1 = .013 + .11\sigma. \tag{10}$$

Finally, inserting these estimates of a_1 and a_2, together with the estimates of α, ρ, R_o, and Q, into equation (6) and assuming that $\alpha l + (1-\alpha)k = .02$ for all firms, we obtain the estimates of r', given assumed values of σ and λ. The results are shown in Table 1.

Let's turn to the ten two-digit manufacturing industries in Table 2. The model in Section II can be reinterpreted on an industry-wide basis, $Q(t)$, $L(t)$, etc., being regarded as industry aggregates rather than figures for individual firms. Based on this interpretation, estimates of α, ρ, Q, R_o, and b' were obtained for each industry by methods similar to those used for the firms. However, since a_2 would be expected to vary considerably from one industry to another, I cannot estimate a_2 and a_1 in the way I did for the firms, unless data are available for a number of relatively homogeneous industries. Having data for only a small number of industries, I rewrite equation (6) as

$$r' = \frac{Q}{R_o} b'\left(1 - x + \frac{a_2}{b'}\lambda\right) + b' - \lambda - \rho + \sigma + \alpha l + (1 - \alpha)k, \tag{11}$$

where $x = (a_1/b)$ is the proportion of the industry's technical change not due to its own R and D. Then I set a_2 equal to zero, insert the estimates of α, ρ, Q, R_o, and b' into equation (11), assume that $\alpha l + (1-\alpha)k = .02$, and obtain the results shown in Table 2. It can easily be seen that these results are lower bounds on the marginal rates of return.

[8] We can obtain our own estimate of a_2 by regressing b' on ρ, since z should be uncorrelated with ρ. To allow for interindustry differences in a_1, one can allow the intercept to differ between industries. However, it turns out that these differences are not statistically significant.

[9] Note two things. First, since the two estimates are largely independent, it would be better to average Minasian's estimate of a_2 and ours rather than to use his alone. However, the two estimates are so close that this would make no difference. Second, it may be objected that Minasian's estimate of .11 is inappropriate here because it is based on the assumption that a_1 is zero. However, if we use the estimate he obtains when he relaxes this assumption, together with our results, we get almost exactly the same answer. Specifically, if we average his estimate (.08) and ours (.12), we get .10. Moreover, although each of these estimates has a substantial standard error, the average is statistically significant.

TABLE 1

ESTIMATES OF b AND b' AND MARGINAL RATES OF RETURN FROM R AND D EXPENDITURES, TEN CHEMICAL AND PETROLEUM FIRMS

FIRM	ESTIMATES		RATES OF RETURN							
			Capital-Embodied				Organizational			
	b	b'	$\sigma=.04$ $\lambda=.04$	$\sigma=.04$ $\lambda=.07$	$\sigma=.07$ $\lambda=.04$	$\sigma=.07$ $\lambda=.07$	$\sigma=.04$ $\lambda=.04$	$\sigma=.04$ $\lambda=.07$	$\sigma=.07$ $\lambda=.04$	$\sigma=.07$ $\lambda=.07$
C1	.0051	.0035	.12	.18	.03	.12	.04	.04	.04	.04
C2	.0624	.0239	.25	.26	.25	.25	.02	.01	.02	.02
C3	.0200	.0260	.42	.46	.38	.42	.10	.11	.09	.10
C4	.0354	.0141	.33	.36	.29	.33	.14	.17	.12	.14
C5	.0534	*	.53	.58	.47	.53	.06	.06	.05	.06
P1	.0212	.0033	.24	.25	.20	.24	.13	.16	.11	.13
P2	.0594	.0191	.57	.72	.49	.57	.25	.33	.17	.25
P3	.0656	.0317	.64	.70	.56	.64	.51	.63	.40	.51
P4	.0947	.0107	.92	.99	.82	.92	.79	.96	.63	.79
P5	.0877	.0182	.73	.78	.67	.73	.31	.37	.26	.31

* Less than zero.
SOURCE: See Sections III and V.

IV. *Capital-Embodied Technical Change: Model*

Before discussing the results of Section III, let us consider an alternative model based on the supposition that technical change is capital-embodied, not organizational, and see the extent to which the results differ from those in Section III. For capital installed at time v which is still in existence at time t, the production function for a particular firm is assumed to be

$$Q_v(t) = Ae^{a_1 v}\left[\int_{-\infty}^{v} e^{-\lambda(v-g)}R(g)dg\right]^{a_2} L_v^{\alpha}(t)K_v^{1-\alpha}(t), \tag{12}$$

where $Q_v(t)$ is the output rate (in 1960 prices) at time t from such capital and $L_v(t)$ is the rate of labor input being combined with this capital at time t. Of course, A, a_1, and a_2 are not the same as in Sections II–III.

Next, assume that all capital in a particular firm, regardless of vintage, depreciates at an annual rate of δ and that the firm's labor force is allocated efficiently among various vintages of capital. Given this assumption, one can show that

$$Q(t) = AL^{\alpha}(t)\left\{e^{-\delta t}\int_{-\infty}^{t} e^{(a_1/(1-\alpha)+\delta)v} \cdot\left(\int_{-\infty}^{v} e^{-\lambda(v-g)}R(g)dg\right)^{a_2/(1-\alpha)} I(v)dv\right\}^{1-\alpha} \tag{13}$$

TABLE 2

ESTIMATES OF b AND b' AND LOWER BOUNDS ON THE MARGINAL RATES OF RETURN FROM R AND D EXPENDITURES, TEN MANUFACTURING INDUSTRIES

INDUSTRY	ESTIMATES		RATES OF RETURN							
			Capital-Embodied				Organizational			
	b	b'	$\sigma=.04$ $\lambda=.04$	$\sigma=.04$ $\lambda=.07$	$\sigma=.07$ $\lambda=.04$	$\sigma=.07$ $\lambda=.07$	$\sigma=.04$ $\lambda=.04$	$\sigma=.04$ $\lambda=.07$	$\sigma=.07$ $\lambda=.04$	$\sigma=.07$ $\lambda=.07$
						$x=.25$				
Chemicals.........	.037	.035	.03	.03	.04	.03	.24	.21	.27	.24
Machinery.........	*	.020	†	†	†	†	.01	−.02	.04	.01
Food..............	.047	.014	.58	.57	.58	.58	1.77	1.74	1.80	1.77
Paper.............	.034	.023	.26	.26	.27	.26	1.51	1.48	1.53	1.50
Instruments........	.083	.010	.07	.07	.07	.07	−.07	−.10	−.04	−.07
Electrical equipment	.036	.037	.04	.04	.04	.04	.07	.04	.10	.07
Stone, clay, and glass	.015	.025	.08	.08	.08	.08	.81	.78	.84	.81
Furniture..........	.019	.010	.37	.37	.38	.37	2.49	2.46	2.52	2.49
Apparel...........	.030	.009	.98	.97	.99	.98	3.38	3.35	3.41	3.38
Motor vehicles.....	.086	.024	.04	.04	.05	.04	.01	−.02	.04	.01
						$x=.50$				
Chemicals.........	.037	.035	.02	.02	.02	.02	.14	.11	.17	.14
Machinery.........	*	.020	†	†	†	†	−.04	−.07	−.01	−.04
Food..............	.047	.014	.43	.42	.43	.43	1.15	1.12	1.18	1.15
Paper.............	.034	.023	.19	.18	.19	.19	.98	.95	1.01	.98
Instruments	.083	.010	.05	.05	.05	.05	−.08	−.11	−.05	−.08
Electrical equipment	.036	.037	.03	.02	.03	.03	.01	−.02	.04	.01
Stone, clay, and glass	.015	.025	.11	.05	.06	.11	.52	.49	.55	.52
Furniture..........	.019	.010	.27	.26	.27	.27	1.64	1.61	1.67	1.64
Apparel...........	.030	.009	.75	.74	.74	.75	2.23	2.20	2.26	2.23
Motor vehicles.....	.086	.024	.03	.03	.03	.03	−.04	−.07	−.01	−.04
						$x=.90$				
Chemicals.........	.037	.035	.00	.00	.00	.00	−.03	−.06	.00	−.03
Machinery.........	*	.020	†	†	†	†	−.10	−.13	−.07	−.10
Food..............	.047	.014	.11	.11	.11	.11	.17	.14	.20	.17
Paper.............	.034	.023	.04	.04	.04	.04	.13	.10	.16	.13
Instruments........	.083	.010	.01	.01	.01	.01	−.10	−.13	−.07	−.10
Electrical equipment	.036	.037	.01	.01	.01	.01	−.08	−.11	−.05	−.08
Stone, clay, and glass	.015	.025	.01	.01	.01	.01	.05	.02	.08	.05
Furniture..........	.019	.010	.06	.07	.06	.06	.26	.23	.29	.26
Apparel...........	.030	.009	.22	.23	.22	.22	.38	.35	.41	.38
Motor vehicles.....	.086	.024	.01	.01	.01	.01	−.12	−.15	−.09	−.12

* Due to sampling errors, this estimate violates the a priori constraint that $b>0$.
† No estimate can be made because the estimate of b is negative.
SOURCE: See Sections III and V.

where $I(v)$ is the gross investment in plant and equipment (1929 prices) by the firm at time v. Of course, δ varies from industry to industry, but I assume that it is the same for all firms in the same industry. Finally, I assume once again that equation (2) holds up to 1960.

Given this alternative model, what would have been the marginal rate of return from an additional amount spent in 1960 on R and D, assuming that R and D expenditures after 1960 would continue to con-

form to equation (2)? If an additional expenditure of ϵ had been made on R and D in 1960, the output at a subsequent time t would have been

$$Q(t) = AL^{\alpha}(t)\left\{e^{-\delta t}\left(\int_{-\infty}^{0} e^{[a_1+a_2(\rho-\sigma)]/(1-\alpha)v} I(v)dv\right) \cdot \left(\frac{R_0}{\lambda+\rho-\sigma}\right)^{a_2/(1-\alpha)} + e^{-\delta t}\int_0^t e^{[\delta+a_1/(1-\alpha)]v}\left[\frac{R_0 e^{(\rho-\sigma)v}}{\lambda+\rho-\sigma} + \epsilon e^{-\lambda v}\right]^{a_2/(1-\alpha)} dv\right\}^{1-\alpha}. \tag{14}$$

Let $Q_0(t)$ be the output rate at time t if $\epsilon=0$, and let

$$J_0(t) = e^{-\delta t}\int_{-\infty}^{t} e^{[\delta+(a_1+a_2[\rho-\sigma])/(1-\alpha)]v} I(v)dv. \tag{15}$$

That is, $J_0(t)$ is the "effective" stock of capital [13] [15] at time t if $\epsilon=0$. Assuming that $I(v)=Ie^{iv}$ from 1960 on, it follows that

$$\left.\frac{\partial Q(t)}{\partial \epsilon}\right|_{\epsilon=0} = a_2(\lambda+\rho-\sigma)\frac{Q_0(t)}{J_0(t)}\frac{I}{R_0} \cdot \left[\frac{e^{[i-\lambda+a_1/(1-\alpha)+(a_2/(1-\alpha)-1)(\rho-\sigma)]t} - e^{-\delta t}}{\delta-\lambda+i+\frac{a_1}{1-\alpha}+\left(\frac{a_2}{1-\alpha}-1\right)(\rho-\sigma)}\right] \tag{16}$$

where I is gross investment in plant and equipment in 1960. Assuming for simplicity that $Q_0(t)/J_0(t)=Q/J$, where Q and J pertain to 1960, we have

$$\int_0^{\infty}\left.\frac{\partial Q(t)}{\partial \epsilon}\right|_{\epsilon=0} e^{-rt}dt = \frac{a_2(\lambda+\rho-\sigma)QI}{\left[\delta-\lambda+i+\frac{a_1}{1-\alpha}+\left(\frac{a_2}{1-\alpha}-1\right)(\rho-\sigma)\right]JR_0} \times \left\{\int_0^{\infty} e^{[-r+i-\lambda+a_1/(1-\alpha)+(a_2/(1-\alpha)-1)(\rho-\sigma)]t}dt - \int_0^{\infty} e^{-(\delta+r)t}dt\right\}. \tag{17}$$

$$= a_2\frac{(\lambda+\rho-\sigma)}{(\delta+r)}\frac{Q}{J}\frac{I}{R_0} \cdot \left[r+\lambda-i-\frac{a_1}{1-\alpha}-\left(\frac{a_2}{1-\alpha}-1\right)(\rho-\sigma)\right]^{-1} \tag{18}$$

where r is assumed to exceed

$$i - \lambda + \frac{a_1}{1-\alpha} + \left(\frac{a_2}{1-\alpha} - 1\right)(\rho - \sigma).$$

Setting the right-hand side of equation (18) equal to one and solving for r, we have

$$(19) \quad (r^* + \delta)\left[r^* + \lambda - i - \frac{a_1}{1-\alpha} - \left(\frac{a_2}{1-\alpha} - 1\right)(\rho - \sigma)\right] = a_2(\lambda + \rho - \sigma)\frac{Q}{J}\frac{I}{R_o},$$

where r^* is the rate of return from the extra amount spent in 1960 on R and D.[10]

V. *Capital-Embodied Technical Change: Empirical Results*

To illustrate how equation (19) can be used to estimate r^*, I again consider the ten chemical and petroleum firms and the ten manufacturing industries. To estimate I for each firm, I obtained data regarding its investment in plant and equipment (1929 dollars) in 1960. The slope of the regression of $ln\ I(v)$ on v during 1946–62 is used as a rough estimate of i. As an estimate of δ, I use the reciprocal of the length of life of plant and equipment in the industry, assuming that plant has a 45 year life and that the average life of equipment is given by the Treasury's 1962 *Depreciation Guidelines and Rules*. To estimate J, the deflated book value of the firm's 1960 fixed assets was multiplied by Phelps's estimate [13] of the ratio of "effective" to "old-style" capital in the business sector of the economy.

The next step is to estimate $b = a_1 + a_2(\rho - \sigma)$ for each firm. If the assumptions in the previous section hold,

$$(20) \quad (1-\alpha)\, ln\left\{[W(t) - W(t-1) + \delta W(t)]/I(t)\right\} = ln\left[A\left(\frac{R_o}{\lambda + \rho - \sigma}\right)^{a_2}\right] + bt,$$

where $W(t) = Q^{1/(1-\alpha)}(t)/L^{\alpha/(1-\alpha)}(t)$. Thus, in each firm, I calculate the value of the left-hand side of equation (20) for each year during 1946–62, using the estimates of α and δ described above and the data provided by the firms regarding $Q(t)$, $L(t)$, and $I(t)$. Then I regress this value on t, and use the regression coefficient, $\hat{b}$, as an estimate of b. As in Section III, I omitted years when output was less than some previous year.

[10] In most cases, a good approximation for r^* is $\sqrt{a_2(\lambda+\rho-\sigma)IQ/R_oJ}$.

Next, I estimate a_1 and a_2, assuming that a_2 is the same for all firms but that a_1 may differ between industries. The regression of $\hat{b}$ on ρ is

$$\hat{b} = -.024 + \underset{(.242)}{.673}\,\rho, \tag{21}$$

the correlation coefficient (adjusted for degrees of freedom) being .61, and there is no evidence that a_1 differs between the industries. Thus, our estimate of a_2 is .673 and our estimate of a_1 is $-.024+.673\sigma$. Finally, inserting these estimates, together with the estimates of δ, α, ρ, i, I, R_0, Q, and J, into equation (19), we obtain estimates of r^*, given assumed values of σ and λ. The results are shown in Table 1.

Turning to the two-digit manufacturing industries, we again reinterpret the model in terms of industry-wide aggregates. Estimates of δ, I, i, and J were obtained by methods similar to those used for the firms. Rewriting equation (19) as

$$(r^* + \delta)\left(r^* - \frac{b}{1-\alpha} + \lambda - i + \rho - \sigma\right) = b\left(1 - x + \frac{a_2\lambda}{b}\right)\frac{I}{R_0}\frac{Q}{J}, \tag{22}$$

I set a_2 equal to zero, insert the estimates of δ, I, R_0, Q, J, i, α, and b into equation (22), and obtain the results shown in Table 2. Like the comparable results in Section III, they are lower bounds on the rate of return rather than estimates of the rate of return. Without data for more industries, we cannot obtain the latter.

VI. *Discussion and Conclusion*

In view of their roughness it would be unwise to read too much into the results in Tables 1 and 2. However, several points are worth noting. First, like the results of previous studies the estimates in Table 1 tend to be very high. Among the petroleum firms, regardless of whether technical change was capital-embodied or organizational, the marginal rates of return average about 40–60 percent. Among the chemical firms, they average about 30 percent if technical change is capital-embodied, but only about 7 percent if it is organizational. Even if the elasticity of supply to the firm of R and D inputs is less than infinite, as Machlup asserts, the rates of return remain high, so long as the elasticity remains within seemingly reasonable bounds.[11]

[11] If true, these high marginal rates of return may persist because of the riskiness of R and D activities or because firms are ignorant of the true returns. For further discussion, see below. However, if there is a lag in the effect of R and D expenditures on the production function our estimates in Tables 1 and 2 overestimate the true rate of returns. If one knows the length of the lag, the necessary adjustment can be made quite easily. For example, in the case of organizational technical change, it turns out that our estimates in Table 1 (with σ and λ equal to .04) are about 20 percent too high if the lag is one year. Obviously, this may be important

Second, comparing these results with the few estimates of this sort that have been published by other economists, it appears that although our estimates seem very high they are generally much lower than those obtained by others. However, comparing our results with data I obtained from a number of large chemical and petroleum firms regarding the expected profitability of their current R and D projects, it appears that our estimates are considerably higher than the firms'. To some extent, the latter difference is due to differences in concept and to sampling errors and other inadequacies in our estimates. But to the extent that this difference is real and our estimates are closer to the truth, it suggests an underinvestment in R and D, particularly among the petroleum firms.[12]

Third, the estimates in Table 1 suggest that the marginal rate of return was directly related to a firm's size in chemicals but inversely related to it in petroleum. To the extent that these differences, which seem to persist for all values of λ and σ in Table 1 and for capital-embodied and organizational technical progress, are real, they suggest that a transfer of R and D inputs from the smaller chemical firms to the largest ones and from the largest petroleum firms to the somewhat smaller ones might be desirable.

Fourth, the lower bounds in Table 2 suggest that, even if x is as large as .90, the marginal rate of return exceeded 15 percent in the apparel industry, regardless of whether technical change was organizational or capital-embodied. If it was organizational, the same was true for food and furniture. Although they are by no means unambiguous, these results suggest that there may have been an underinvestment in R and D in some of these industries.[13]

Fifth, whether technical change is capital-embodied or organizational, there is evidence of diminishing return to scale from cumulated net R and D expenditures in chemicals and petroleum. If technical change is organizational, our results, like Minasian's [11], suggest that the average effect of a 1 percent increase in cumulated net R and D expenditures is a .1 percent increase in current output. If technical change is capital-embodied, its effect is a .7 percent increase in current output.

Sixth, since the rate of technical change is measured by b if technical change is capital-embodied (and by b' if it is organizational), Table 1 provides the first published estimates of the rates of technical change during the postwar period in particular firms. These estimates indicate that a firm's rate of technical progress is directly related to the rate of

[12] For results based on other studies, see [1] [2] [3] and [7]. The data regarding the firms' expectations pertain to eight firms and were obtained by interviews and correspondence. These data, together with similar information collected during the next few months, will be used in a paper I am writing with Michael Hamburger on industrial R and D expenditures.

[13] Of course, this presumes that x does in fact lie between .25 and .90 in all industries. This is reasonable, but by no means indisputable.

growth of its R and D expenditures, as would be expected on the basis of our model.[14] However, there is no evidence that such frequently-used variables as the firm's ratio of R and D expenditures to sales or its growth rate exert an important influence on its rate of technical change.[15]

Seventh, Table 2 provides new estimates of the rate of capital-embodied technical progress in various manufacturing industries. According to these estimates, technology advanced most rapidly in motor vehicles and instruments and least rapidly in machinery, glass, and furniture. As would be expected, the estimated rate of technical change generally exceeds the estimated rate of organizational technical change. However, there is relatively little correlation between them.

In conclusion, these results should be viewed with considerable caution, for at least three reasons. First, they are based on a number of highly simplified assumptions: that uncertainty can be ignored, that technical change is cost-reducing,[16] that all technical change is neutral, that the production function is Cobb-Douglas, and that, to use Phelps's phrase [12], capital is putty, not hard-baked clay. Second, the estimates in Tables 1 and 2 contain substantial sampling errors.[17] Third, they are incomplete estimates of the social rate of return, since they do not take account of the effects of increased R and D expenditures in one industry or firm on productivity in another industry or firm. The social rates of return may be higher than ours.

Nonetheless, I believe that the results represent a useful first step toward the formulation of operational techniques to measure the returns from R and D, and I hope that others will be encouraged to join in the work toward this end.

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[14] Note three things: (1) When industry-wide data are used rather than firm data, there is also a positive relationship between b (or b') and ρ. (2) In practically all cases, regardless of whether the data pertain to firms or industries and whether b or b' is used, this positive relationship is statistically significant, the exception being the case where firm data are used and technical change is assumed to be organizational. (3) It is possible that an identification problem exists in all of these relationships and that b or b' influences ρ. However, the tests that have been made to determine whether some other variable, like a firm's (or industry's) rate of growth, is responsible for the variation in both b or b' and ρ, have not indicated that this is the case. See note 15.

[15] Both the ratio of R and D expenditures to sales in 1960 and the rate of growth of sales during 1945–58 were used as additional independent variables in equation (21) and the corresponding regression in the case of organizational technical change. Neither was statistically significant. The rate of growth of profits during 1945–58 was used, too, with similar results. When industry-wide data are used instead of firm data, the results are the same. For some relevant discussion, see [16]. Note that, according to the model, the rate of technical change does not depend on λ.

[16] Of course, this helps to explain our choice of the petroleum and chemical industries, both of which direct a great deal of their R and D at new processes.

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MARKET STRUCTURE, BUSINESS CONDUCT, AND INNOVATION*

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I

We rely heavily on the a priori relationships between market structure and business conduct and performance for providing meaningful interpretations of the activities of the approximately eleven million business enterprises that make up the private sector of the American economy. If firms are numerous and no one of them controls a significant share of the appropriately defined market, we predict reasonably competitive pricing; if firms are very few (concentrated oligopoly), we predict the contrary. Indeed, it is the assumed validity of these relationships that makes price theory something more than random speculations and public policy toward monopoly something more than the caprice of politicians and bureaucrats. There seems to be a general presumption, *ceteris paribus* and all that, in favor of low concentration as a structural goal on the grounds that this is more likely to assure the attainment of certain performance goals such as price-cost relationships compatible with efficient resource allocation.

It is generally agreed on a priori grounds also that market structure affects the level of innovational effort, but there is far less agreement on the magnitude and direction of the effect. Many economists adhere to the traditional view that competition begets technical progress [9, p. 317]. But the Schumpeterian hypothesis [13, Chap. 7], probably the oldest and most respectable on this subject, asserts that the possession of accumulated monopoly rewards, the prospects of additional such rewards in the future, and the security attending market power are prerequisites to undertaking the risks and uncertainties of innovational activities. Contemporary students of the problem have recently interpreted the hypothesis to state that the greater the profits and the degree of market power (the potential capability to earn monopoly rewards) or firm size, the greater should be the effort to innovate. The original and restated versions, while akin to each other in terms of their policy implications, are obviously not the same. The original Schumpeterian hypothesis simply stated that in the absence of uncom-

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mitted accumulated surpluses the undertaking of risky and uncertain innovational activities would not occur; and unless the prospective rewards to such innovational activities were high (relative to competitive returns), the incentive to undertake them did not exist. But for several important reasons this does not imply that the measurable innovational effort of a firm as expressed, say, in terms of its research and development expenditures should be a continuous and increasing function of market power, business size, or, for that matter, retained monopoly earnings.

First, it has not yet been demonstrated that industry concentration ratios are good surrogates for monopoly power or past or future monopoly profits. The lines of demarcation between census classifications of industries are drawn often faintly, often heavily, and sometimes arbitrarily. And the concentration ratio—the measure of market power most frequently used in empirical tests of the restated Schumpeterian hypothesis—is after all only one of the many possible points on the cumulative concentration curve and not a summary index of the entire curve. Accordingly, even if industries identically monopolistic by a true measure of monopoly always earned identical monopoly profits, one would surely still expect to find what we in fact find: considerable variation in reported profits among industries having concentration ratios of roughly the same magnitude.

Second, and this is much more important, there is no presumption in the Schumpeterian hypothesis as originally stated that innovational activity as measured in terms of research and development expenditures is the only use to which accumulated uncommitted profits will be put, or that any of such profits, simply because they exist, will be put to this use in the absence of appropriate opportunities. Schumpeter defined innovations broadly enough to include, among others, mergers, new organization, the new advertising campaign, the new product, and the new process. Only the last two are logical consequences of technical research and development activities conducted inside the business firm. What Schumpeter really said was that uncommitted balances were a prerequisite to engaging in highly uncertain commercial activities. Hence, to run regression analyses of research and development expenditures as reported, say, to the National Science Foundation (the most comprehensive source of research and development expenditures) and firm size, concentration ratios, or any other index of market structure, at best only determines the statistical relationship between a fraction of the Schumpeterian variable and some rather imprecise measures of monopoly.

Third, there is the fundamental question of whether, even if our in-

exes of monopoly power or business size were statistically unbiased
nd our quantitative measures of innovative effort conceptually complete and unambiguous, one would on logical grounds expect them to ary systematically and continuously with each other; and there are ery persuasive practical and theoretical reasons why we would not exect them to vary proportional to each other. If we can predict cricket hirps from the temperature, then we can also predict the temperature rom cricket chirps; so let us take a look at the policy implications of he presumed collinearity between monopoly and research and development outlays by interchanging the dependent and independent variables. Between 1930 and 1963 the private outlays of firms on research nd development increased from $116 million to $5 billion—an increase of 4,310 percent. No one to my knowledge, even the most lugubrious observer of our antitrust policy, has estimated anything like a 43-fold increase in monopoly over this period. It could—and hould—hastily be pointed out that these data must be deflated by some general economic indicator such as the gross national product as xpressed in current dollars. In 1930 the outlays of private firms on esearch and development amounted to 0.1 percent of GNP, and in 1963 to 1.0 percent of GNP, a relative increase of 1,000 percent. To suggest the possibility that monopoly may have increased even tenfold over this period would surely horrify every Assistant Attorney General rom Thurman Arnold to William Orrick. I believe we can rescue ourselves from this situation and restore the philosophical calm of our antitrust crusaders as well by interpreting Schumpeter's theory as a "threshold theory"; some departure from a state of perfect competition (or the presence of some monopoly) is a necessary concomitant of innovation, but it does not follow that twice this volume of departures, somehow measured, should lead to twice the volume of innovations.

Moreover, the number of scientists must surely be an important determinant of the volume of inventions and, in turn, volume of innovative activity. As Derek J. Price has pointed out [16, p. 517], for three hundred years now the number of scientists has doubled every ten years or so; which means that at any point in time in this three-hundred-year period it could be said that approximately 80 percent of all the scientists that ever existed, currently exist. Again it is difficult to equate, or even systematically relate, this apparent law of growth of the scientific population with monopoly power in the private sector of the economy.

There is also a good theoretical reason why we should expect at best a rather weak relationship between either the measurable input or output of a given innovative effort and firm size in any given industry, or

between innovative effort and levels of concentration among industries. As will be demonstrated later in simple quantitative terms, innovative effort is heavily concentrated in oligopolistic industries where at least several strategies are available to each firm and the patterns of reaction among rivals are characterized by high orders of uncertainty. It is very likely that, as it is frequently contended, a significant portion of private research and development is imitative and competitive and not innovative in the Schumpeterian sense. This portion may possibly be distributed among firms selling in the same market in rough proportion to their size. But oligopolists have other Schumpeterian innovative strategies open to them, and at any given time one firm may easily reason that the best counter strategy to another's increase in research and development outlays is not greater research and development outlays of his own, but the development of a new product line through acquisition or an increase in advertising outlays on product lines he currently produces. The Cournot reaction paths among oligopolists with respect to research and development outlays could therefore conceivably be multidimensional and could tend toward some rough equilibrium bearing no systematic relationship to firm size.

The important question, however, is not so much whether the recent voluminous and, on the whole, excellent empirical work constitutes effective tests of the Schumpeterian hypothesis. Schumpeter, more than any other economist of his generation, assigned great importance to the innovational activities of the business firm and suggested certain of their implications for economic welfare and public policy. The hypothesis itself was the product of empirical observation and, by the crude and limited test employed, was of course delivered to us already pretested by Schumpeter himself. As he put it, speaking of the sources of economic progress in a capitalistic society: "As soon as we go into the details and inquire into the individual items in which progress was most conspicuous, the trail leads not to the doors of those firms that work under conditions of comparatively free competition but precisely to the doors of the large concerns" [13, p. 82]. In the context of Schumpeter's basic thesis he left no doubt that he viewed the "large firm" as synonomous with "large firm with market power."

By the same crude tests, the hypothesis is as consistent with observable facts now as it was then. We need only consult any recent *Survey* of the National Science Foundation [6, p. 20] to remove any serious doubt that company-financed research and development effort is highly concentrated in large firms making up, for the most part, the economy's more highly concentrated industries. In 1961 an estimated 11,800 manufacturing and other companies performed research and development; a smaller number financed research and development

out of their own funds. Three hundred and ninety-one firms having 5,000 or more employees amounted to only 3 percent of the total number of firms performing research and development, and for an insignificant one-hundredth of 1 percent of all industrial firms, but they accounted for slightly over 80 percent of the total company-financed research and development. In 1958, the 478 firms making up the 5,000 and over employee group accounted for 29 percent of total industrial employment and 26 percent of total sales. Firms having less than 1,000 employees accounted for 90 percent of the total number of firms and for 60 percent of total industrial employment, but for only 5 percent of total company-financed research.

Five industry groups containing most of the more narrowly defined industries making up the "oligopolistic core" of American industry accounted for nearly 75 percent of all company-financed R and D: chemical and allied products (19 percent), electrical equipment and communications (19 percent), motor vehicles and other transportation equipment (14 percent), machinery (13 percent), and aircraft and missiles (8 percent). At the opposite end of the spectrum are such industry groups as textiles, lumber, wood products and furniture, and other manufacturing and nonmanufacturing industries, which together account for about 5 percent of total company-financed research and development expenditures but for a considerable number of both the low concentration industries and the firms with 1,000 or less employees. To be sure, there are several spectacular examples of highly concentrated industries such as tobacco products and steel that rank low in research and development. Similarly, some of the firms included on *Fortune's* list of the largest 500 corporations spend a relatively small percentage of their sales on innovative activities. However, the data, now far more complete than those Schumpeter had before him, clearly support his hypothesis: the trail of innovative effort leads to the doors of the large concerns operating in markets distinctly not characterized by all the conditions of free competition. But it need not be inferred from this that all big firms and all oligopolists innovate. Schumpeter recognized the existence of both somnolent giants and oligopolists more disposed to preserving the calm than to participating in the "perennial gale."

The quantitative work of the past decade, while acknowledging Schumpeter as its inspiration and intellectual parent, has in fact been testing a different hypothesis and one having different policy implications. The restated hypothesis, since it has envisaged tests cast in terms of regression analysis, must also be stated in these terms. If one can sum up Schumpeter in the statement, give me factual evidence of innovational activity in the market economy and I can safely predict

the presence of large firms having temporary monopoly power, the restated hypothesis is, give me x innovational activity and I will give you $b_1, b_2, \ldots b_n$ values showing how much of x can be "explained" in terms of firm size, the level of concentration, and similar a priori relevant variables. The policy implications of the original and restated versions are also different. Schumpeter simply argued that any indiscriminate assault on bigness and market power, taken out of their evolutionary context, would deprive the capitalistic process of its source of progress. The newer version proposes to tell us in quantitative terms, to the extent available data permit, how much, where, and up to what point innovative effort (and results) are attributable to such factors as firm size and market structure. Accordingly, it holds out the possibility of providing guidelines that can significantly enhance the efficiency of public policy toward the private sector. The historic dilemma of remedial action under our antitrust policy has been the problem of weighing the social benefit of more competition in the classical sense against the possible loss of scale economies and innovative effort that attend firm size and market power. Obviously, if something approaching probability values can be defined linking certain structural and behavioral attributes of markets with the intensity of innovative effort, we can reduce the risk, to use a favorite expression of Schumpeter's, of throwing the baby out with the bathwater. Instead, to extend the metaphor, we can at least distinguish the baby from the water and may even be able to salvage the clean before disposing of the dirty water.

II

A summary analysis of the neo-Schumpeterian literature cannot do full justice to its statistical subtleties and refinements. I shall deal here with the more essential findings, including their significant qualifications, and their public policy implications. The statistical results obtained by D. Hamberg [1], Frederic Scherer [10] [11], Edwin Mansfield [4] [5], Jacob Schmookler [12], James Worley [15] and others support the generalization that, at least beyond a certain size level, the ratio of research and development expenditures to some index of firm size does not increase significantly with size, and may not increase at all. But this, like most generalizations, must be qualified. Scherer [10] found that within the 500 largest corporations on *Fortune's* list, the largest 100 accounted for slightly smaller percentages of total R and D expenditures and patents than of total sales. This suggests that size, at least after a firm has made the list of 500, has no favorable effect on innovative or inventive effort. Mansfield found that the largest firms measured in terms of sales in

petroleum, drugs, and glass spent a smaller percentage of their sales on research and development than did "somewhat smaller firms" [5, p. 334], that in chemicals the largest firms spent relatively more, and that in steel they spent relatively less but the computed difference was not statistically significant [5, p. 334]. My own unpublished data on the ethical drug industry (as distinguished from the drug industry) are consistent with Mansfield's findings on the drug industry generally, but they suggest that the ratio of research and development expenditures to sales increased markedly up to the firm size of $100 million annual sales and beyond this point declined slightly. However, between 1946 and 1958 the differences in the ratios among size classes declined substantially as firms in the smaller size groups rapidly stepped up their research activities. In the "research revolution" in drugs, the larger firms were the innovators and the smaller firms the imitators. Worley found that in six out of eight two-digit industries, research and development personnel per 1,000 employees increased with total company employment. But in only two industries were the results statistically significant enough to warrant rejection of his null hypothesis that research and development effort increased no more than proportionately with size. In a more recent study of seventeen two-digit and three-digit industries, Hamberg reached results consistent with Worley's in all of the seven industries common to both studies. In twelve industries R and D intensity increased with firm size.

Before the consistency of these general findings be given too much emphasis, it should be pointed out that Worley's and Hamberg's results, because of differences in data and method, conflict with Mansfield's in the case of every individual industry common to the three studies: glass, petroleum, and chemicals. These conflicting results, however, can be easily reconciled, and this reconciliation points up the central finding of the recent statistical studies of market structure and innovational effort: up to a certain size, innovational effort increases more than proportional to size; at that size, which varies from industry to industry, the fitted curve has an inflection point and among the largest few firms innovational effort generally does not increase and may decline with size. For the three industry groups cited, Worley and Hamberg fitted curves to all the available observations and found R and D intensity to be an increasing function of size. Mansfield, on the other hand, confined his analysis to the "very" largest firms: ten large chemical firms, nine large petroleum firms, and only four large glass firms. It is almost certain that Mansfield's petroleum and glass firms fell to the right of the inflection points of curves fitted to a wider range of firm sizes.

In a study soon to appear, Scherer [11] has demonstrated that in

the regression equations used by Worley and Hamberg of the form:

$$\log Y_i = \log a + b \log X_i + E_i,$$

where Y_i is R and D employment, R and D outlays, or patents of the i^{th} firm and X_i is some size variable of the i^{th} firm, such as total employment, sales, or assets, the regression coefficients b are greatly affected by (1) whether zero observations are included or excluded and (2) the size variable used. In his own regressions of patents (more appropriately regarded as an index of R and D output rather than inventive and innovative input) on the three different size variables—assets, sales and employment—for all usable observations for the 500 largest American corporations (ranging from 365 to 448), he obtained values of b ranging from .795 to 1.158, depending upon whether the zeros were deleted and upon which size variable was used. In all cases with zeros deleted, intensity of inventive activity as measured by patents tended to decline with firm size, and in all cases tended to increase with firm size when he adopted the convention of counting each zero patent as 0.5 patent to permit its inclusion in the logarithmic analysis. Also, in all cases, the values of b were higher (showing greater increases in patenting intensity with size) when firm size was measured in terms of employment. By using alternative regression equations Scherer found, as did Mansfield, that R and D employment inputs increased with firm size in chemicals, and that in five other two-digit industries R and D effort increased with size up to about the $500 million sales size group and then tended to decline, except in primary metals, which U.S. Steel pulled back into a stage of increasing returns.

Clearly, any answer to how inventive and innovative efforts are affected by firm size hangs on an extraordinarily slender reed that may alternatively bend upward or droop downward, depending on the species of statistical zephyrs blowing at the time.

Similarly, we are equally unsure about the precise relationship between market power and inventive and innovative effort. Mansfield has found that in petroleum and coal over the periods 1919-38 and 1939-58 the ratio of important innovations to market share for the four largest firms was high: 1.42 and 1.26 for petroleum and 2.46 and 2.31 for coal [4, p. 561]; on the other hand, the corresponding ratio for steel was very low: 0.48 and 0.68. It would seem from the poor showing of steel and the previously indicated relationship in primary metals between size and research and development effort that the principal conclusion on the steel industry is that U.S. Steel tries hard but does not meet with spectacular success. Hamberg [1, pp. 74-75] found 1958 R and D expenditures and R and D to sales ratios in seventeen two-

digit and three-digit industry groups positively correlated with their weighted concentration ratios, but concluded that only 30 percent of the variance in research and development intensity could be explained by industrial concentration. Horowitz, running similar correlations [2, pp. 300-01], reached a similar conclusion: a positive but weak correlation exists between R and D intensity and the level of industrial concentration.

The difficulty with such regression analyses as these is not so much their statistical as their conceptual inconclusiveness. It is clear that oligopoly theory in its present state is highly inadequate for purposes of generalizing on such traditional variables as price and output. In a Cournot model, price and output move monotonically from pure monopoly to pure competition equilibrium positions as the number of firms increases. In a Chamberlinian model, at some threshold point on the spectrum of firm numbers between one and many, the monopoly solution collapses and is replaced by a competitive one. And in game theory models almost anything can happen. Moreover, the facts on oligopoly lead us to look with considerable pessimism for neat and continuous relationships between variables characterized by far less uncertainty than inventive and innovative effort. For example, the synthetic fibers and cigarette industries both have concentration ratios of approximately .80, but over the period 1953-60 synthetic fibers had a price flexibility index of 0.87, cigarettes an index of only 0.02; and primary aluminum with a concentration ratio of 1.00 had more flexible prices than paperboard boxes with a concentration ratio of only 0.16. Until these diverse patterns of the commonplace variables of oligopoly have themselves been explained more satisfactorily, it is difficult to say much more about the statistical analysis of R and D than that it reveals far more stable and systematic relationships than those found for variables about which we are supposed to know much more. I, for example, would consider a computed correlation coefficient that explained one-third of the rate of return variance in American industry a computation of some significance.

The recent statistical analysis of inventive activity obviously is not without policy implications. It is true that it provides no basis for either condemning or beating the drums for bigness or for concentration on the grounds that they either stifle or promote technical progress. But this may be a much more significant contribution than it first appears. Only a short decade ago one of the nation's most respected public officials, not having the essential facts available, could state without fear of committing serious error that "Big Business" was the nation's greatest spur to technical progress [3]. A prominent spokesman from industry, equally unaware of the facts, could state with equal assur-

ance in the same year that the essential feature of our large corporations was their failure to engender progress, except in the single case of the household garbage grinder [8]. Such statements should diminish in the light of the recently developed stubborn facts. The large corporation and some degree of market power appear to be concomitants of organized innovative effort, but corporate size and market power in excess of Schumpeterian threshold levels appear to be with us, and for this and other reasons are still legitimate concerns of public policy.

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TECHNOLOGICAL CHANGE AND ECONOMIC THEORY

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During the last dozen years or so economists have shown that the production, diffusion, and use of new knowledge are more important for the growth of output per head than is the accumulation of physical capital. It seems safe to say that this discovery occasioned more surprise among economists than among educated men generally. The differential surprise is an instructive example of how damaging to the understanding professional knowledge can sometimes be.

Surely, what distinguishes the twentieth from the eighteenth century is less its physical plant than the knowledge embodied or used in that plant. It seems, for example, utterly obvious that the technological knowledge possessed by its people played a bigger role in Germany's post-World War II recovery than did the devastated and rundown plant which survived the war.

If it is true that most educated men knew a vital truth about economics that many economists did not, the reasons for this would seem to be of interest. In this paper I propose to explore them and their implications.

We can bring the problem into focus if we recognize at the outset that the recent evidence in itself would have caused little stir among our intellectual forebears, even though it was they who, for the sake of analysis, put the accumulation of capital at the center of attention, and even though they knew at most only the agricultural and industrial revolutions rather than the seemingly permanent technological revolution of our time. Adam Smith, Karl Marx, and John Stuart Mill, to cite just a few outstanding examples, discussed at length the contribution of the knowledge and skills of a nation's labor force to its aggregate output, and perforce assigned to new inventions and discoveries and to the more widespread dissemination of old knowledge a vital role in development. Likewise, much of List's infant-industry argument on behalf of protection was based on the time needed for investment in labor to take place.

Among more modern economists Alfred Marshall, Frank W. Taussig, and Irving Fisher come to mind as having been likewise appreciative of these matters.[1] The point, of course, is not that the great men

[1] Obviously, the writers listed are only those in the orthodox tradition. Heterodox economists like Veblen, Schumpeter, and Kuznets usually put technological or technical change at the heart of their analysis.

of the profession understood, but that the troops in the line did, too, partly because of their professional education but also simply because they were generally well educated.

And of course, I am not trying to say that a decade ago the leading members of the profession did not equally understand. The basic point is that the generality of contemporary economists were surprised by the evidence, and the question is, why? The answer, I think lies essentially in an inadequate strategy for the advancement of knowledge in our field. It lies not in what we have done but in what we have not done. We declined in understanding because we grew without balance.

Teaching aside, the activities of the overwhelming majority of the profession have been directed toward two entirely proper objectives: (*a*) the further refinement and, more recently, the empirical testing of received principles, and (*b*) the solution of pressing social problems. The pursuit of the first of these goals led inevitably to a considerable growth of the formal apparatus of our science. This, of course, was precisely what was desired and indeed needed. However, since we needed more time to learn and teach it, we had less time than before to learn and teach other knowledge, however vital such other knowledge might be to an understanding of those economic phenomena that the formal theory failed to deal with.

And so we were forced to rediscover with a worm's-eye view what the birds of earlier generations knew as a familiar feature of the landscape. Lacking time to learn the history of economic thought, we were doomed to repeat it.

Obviously, however, there is more to it than that. For the evolution of the formal apparatus might have brought technological change to the fore instead of pushing it into the background. That the theoretical apparatus grew as it did, however, was predestined by the very nature of the underlying principles on which that growth fed. On the one hand, the law of variable proportions presupposes a given state of the technical arts. On the other, the law of diminishing marginal utility (or its more modern form, the law of diminishing marginal rate of substitution) presupposes a given set of preferences. To a large extent, the theoretical apparatus which has evolved consists of the precise formulation of these principles and their application under various assumed conditions. Constructing this theory has preoccupied most of the best minds of the profession for generations. The most convenient setting for this purpose was, of course, a static framework. While the results derived were readily transferable to comparative statics, and, then, not quite so readily, to dynamics, in both the latter changes in technology, tastes, etc., are assumed, not explained. They are, in short, exogenous variables of the theory.

These developments in theory and the mathematical and economet- techniques which evolved with them were, and will prove in the ure to be, enormously valuable. On the other hand, a perhaps inevi- le by-product was a distinct tendency to forget that the knowledge ned in the process was merely a knowledge of the assumptions and ir consequences, and that the precise relation of such knowledge to nomena remained to be discovered. Earlier economists studied the rld and knew it was round. Only their maps were flat. Too many er economists studied the maps and mistook them for the world.

Probably the outstanding example of this misplaced "concreteness" echnological change itself, by which I mean in this context the pro- tion of new knowledge useful in production. With few exceptions erations of economists regarded technological change analytically an exogenous variable. Not knowing its precise linkages, they could make it a dependent variable in their analysis. With the passage of e, however, economists came actually to believe that it really was marily a noneconomic phenomenon; i.e., that its causes lay in other nains of human behavior and therefore should in principle be treat- in the same way as earthquakes. Now, there undoubtedly is some genous component in technological change, but there is also an en- genous one (a fact which was recognized, to cite just a few exam- s, by Smith, Marx, Mill, Marshall, and Hicks). What is more, re- t evidence indicates that the endogenous component is usually dom- nt, at least in modern economies.[2]

When this fact also becomes more widely known, we will again be prised. Yet, if we had really thought about it, we would not be. nomic activity is concerned with the satisfaction of human wants. chnological progress permits those wants to be satisfied better than existing knowledge did. While some inventions are made by acci- it, most of them are made on purpose. Since making them is neither y nor costless, that inventive resources tend to be allocated among ernative projects in accordance with anticipated profits is only to be ected. In short, in the main the production of new technology is it- f an economic activity. It represents in essence the mobilization of iety's creative energies to relieve the scarcities which existing re- irces and products cannot. Far from being an exogenous variable, it ne of the most interesting endogenous variables of them all.

Be that as it may, the necessarily heavy emphasis in recent decades the increasingly complex analytical framework conditioned our nds to the view that technological change was exogenous, not only

Cf. my forthcoming book on invention to be published by Harvard Univ. Press; my with Zvi Griliches, "Inventing and Maximizing," *A.E.R.*, Sept., 1963, and references e cited.

from the standpoint of our theoretical models, but also from that of the economic system. And of course, out of sight, out of mind. When once again our attention turned to economic growth, too many of us found it natural to suggest as its chief cause capital formation—the main variable internal to our models which could make output per head rise.

That it was our renewed interest in economic growth that led to the rediscovery of the importance of technological progress is significant, because that renewal of interest primarily reflected developments not internal but external to our science. On the whole, neither our interest in growth nor our interest in technological change derives from a steady scientific evolution and a clear scientific vision. Rather our current interest in both phenomena exemplifies the other principal reason for our diminished awareness, until recently, of the significance of technological progress. The second reason is our concern, in itself admirable, with pressing current social problems. More specifically, our attention has been turned to technological progress and investment in human capital, primarily because governments and people generally today place economic growth and military power high on the agenda and toward that end make large public and private outlays on research and education. The profession's interest in these matters has followed, not led, informed opinion.

One can confidently assert that, if the pressing economic problems of the interwar years had been not monetary policy, tariffs, labor, and business cycles but the problems of today, then most of what we are just beginning to learn about technological progress would have been known long ago. Thus, once again we find ourselves, as we have so many times in the past, running from behind to catch up to the events of the day. In the meantime, the world pays, as it did in the big depression, a heavy price for our ignorance.

In brief, most of us have been preoccupied either with problems of immediate public concern, or with the elucidation of received principles which do not illuminate economic phenomena of major significance. These endeavors were noble and necessary, but our inability to cope first with the big depression and now with the problems of economic growth suggests that they were not enough.

What has been lacking is that respect for the facts which characterizes the natural sciences.[8] We are presumably interested in explaining

[8] To an extent, econometricians are obviously exempt from the charge, since their avowed mission is the empirical testing of theory. But only to an extent, for the number of untested econometric models still exceeds the number of tested ones. In many respects, the rise of econometrics is the most heartening recent development in economics, since it represents a clear professional recognition of the necessity for empirically testing hypotheses. Nonetheless, for the reasons given below, this will not be enough, if our hypotheses continue to be confined only to a narrow range of phenomena.

events which we call economic—events having to do with the social aspects of the satisfaction of wants. We develop a few powerful principles and then allocate virtually our entire fundamental research effort to evolving, refining, and, more recently, testing them. Other than that, with a few notable exceptions, we spend our time putting out fires—fires which too often reach inferno dimensions because our fire-fighting apparatus is so inadequate.

Such a research strategy would be entirely appropriate if the theory thus developed dealt substantially with the whole range of phenomena to be explained. That it does not is perfectly plain. To get something empirical out of a theory, you have to put something empirical in. While more, theoretically-oriented empirical work is being done by the profession now than ever before, not nearly enough is being done in those areas where such knowledge is needed if we are to enlarge the range of economic phenomena that our science explains. A theory which can tell us what will happen if technology, preferences, resources, and institutions change can be at best but a stage in the growth of our science, not its terminus. We also need theories about how these change.[4]

Even if these variables were in fact exogenous to the economic system as is commonly assumed, we would still need adequate theories of them. Without such theories we will be unable either to explain or control major economic phenomena. What transpires when the state of the arts, etc., is unchanged or change in an assumed way is hardly coterminous with the range of phenomena to be explained. In particular, since economic growth is caused to a large extent by changes in variables with respect to which our existing theory is silent, that theory cannot be expected to yield an adequate theory of growth without substantial emendation. What we shall probably need in the end, to repeat, is a theory which explains what our existing theory takes as given.

There are two further points which need to be made in this connec-

[4] Cf. Paul A. Samuelson, *Foundations of Economics* (Harvard Press, 1947): "However, there is nothing sacred about the conventional boundaries of economics if the cycle were meteorological in origin, economists would branch out in that direction, just as in our day of political theory of fiscal policy is necessary if one is to understand empirical economic phenomena." P. 316. Cf. also James Tobin, "What is wrong with economics is not so much the putting together of the pieces. . . . What is wrong is the poor quality of the pieces that we put together in such models." Quoted in T. C. Koopmans, *Three Essays on the State of Economic Science* (New York, 1957), p. 209. And Koopmans himself, "The test of suitability of a tool of reasoning is whether it gives the most logical and economical expression to the basic assumptions *appropriate to the field in question*, and to the reasoning that establishes their implications. *The difficulty in economic dynamics has been that the tools have suggested the assumptions rather than the other way around. Until we succeed in specifying fruitful assumptions . . ., we shall continue to be groping for the proper tools of reasoning.*" (*Ibid.*, pp. 182-83. Italics supplied.) Tobin's and Koopmans' complaints relate entirely to the inadequacy of our information about behavior, especially with respect to decision making. However, as the quotation from Samuelson suggests, we may also need better information about the state of nature; e.g., in the present context, the production function for new technology.

tion. The first is that to assert with confidence that a given variable exogenous to the economic system may require that we know wh does influence it. Thus, the grandmothers' tales sometimes found i economic treatises to justify treating technological change as exog nous reflect not our knowledge but our ignorance. That this assum tion should have gone unchallenged for so long reflects that inadequa interest in the facts to which I referred above.

The second point is that to the degree that such variables as tec nological change, tastes, resources, etc., prove on examination to be er dogenous, existing economic theory is inaccurate even with respect t the phenomena with which it purports to deal. For example, if recer evidence is to be credited, the rate of technological progress tends t be higher in industries with higher rates of investment. If this is co rect, then an industry in long-run equilibrium in the neoclassical sens will not be in equilibrium at all—not because of external but becaus of internal conditions. The so-called "long-run" supply curve of th industry will shift, and, other things being equal, the proportional shi will be greater, the greater the initial size of the industry. Whether thi is indeed so will require further empirical research to determine. N amount of postulating alternative possibilities will tell us which possi bility nature elects.

There are other even more interesting problems awaiting investiga tion. The old adage, "Necessity is the mother of invention," empha sizes the induced element of invention and suggests that demand tend to create its own supply; i.e., a reversal of Say's Law. The general pat tern of development of new consumer goods suggests indeed that to large extent new goods are invented in response to changes in incom and preferences. Similarly, the evidence alluded to previously suggest an allocation of inventive effort among existing production processes i accord with the prevailing rate of investment in them. If anything lik such forces is operative, then our present dynamic models are seriousl incomplete. To improve them we have no recourse but to develop soli knowledge about relations of the sort suggested and incorporate then into our theory.

A common objection, not to be taken lightly, to undertakings of th sort proposed is that we lack the tools to perform them. The answer t this is twofold. First, whether we shall need concepts and researc techniques beyond those already at our disposal will not become ap parent until we begin to work on the site. So far work in the field o technological change has progressed appreciably with only modes modification of preexisting tools. The main problems have been to ask the right questions and get the right data. Second, if we need new tools, we can get them in the same way that we got the old tools: by

making them, or borrowing them from some neighboring discipline. No doubt there will be failures, and in any case the work will not be easy. In instances ingenuity rivaling that displayed on occasion by natural scientists will be required. But if we do not do it, nobody will, and our science will remain but a fragment, dealing with only some and by no means the most important economic phenomena.

A second but related objection is the judgment that we shall make more progress if we advance outward from our present, well-established base rather than, so to speak, dropping troops by air deep in enemy territory. The more conservative strategy has the advantage of preserving lines of communication more effectively, and the disadvantage that what is communicated is less interesting.

Finally, the more conservative strategy seems rather shortsighted in any case. Sooner or later, if the past is any guide, we shall have to advance in the directions indicated anyway. If we do not proceed now with deliberate speed, we shall find ourselves doing so with desperate haste as members of an emergency government task force of one sort or other, seeking instant wisdom to palliate some pressing social problem, just as we now find our advice sought on the economics of growth, automation, the patent system, the space program, military research, education, manpower retraining, etc. One cannot say with certainty that our advice would now be much better on these matters if our predecessors had possessed a more scientific attitude toward our subject. To make such a statement would require that we guess what, if anything, that we now know that is also relevant would have gone undiscovered. It is obvious, however, from what has been said above what my guess on this question is.

Like the other policy sciences, e.g., medicine and engineering, economics tends to attract into it men with a strong interest in immediately useful results. Even without a strong predisposition in this direction, we would be less than human not to feel an obligation to assist in the solution of those social problems which may be supposed to lie within our professional competence. Moreover, the bracing effect of reality to which economists are subjected when they try to solve practical problems is especially valuable when the conceptual framework of the science is narrow and the scientific interest in widening it is weak.

Nonetheless, a strong orientation toward immediate social problems presents two obstacles to the development of any science. In the first place, the problems tend to abate before their scientific aspects are adequately explored. The patient gets well, or well enough, before the cure is discovered. When the disease recurs, as it usually does, the profession has to start over again, often almost from scratch. Scientific activity thus tends to be repetitive instead of cumulative, because we

do not stay long enough with a single topic to break through to new ground.

Aspects of this seem evident, for example, in the case of the Keynesian revolution. Two key ingredients for the multiplier—the distinction between savers and investors and the association of saving with income—appear often in the "business cycle" literature of the nineteenth century, as well as in Keynes's own *Treatise on Money*. Yet it was more the depth and duration of the big depression than the work of earlier economists which led Keynes to the doctrines of the *General Theory*. It is perhaps not too much to suggest that had the profession exhibited a greater interest in economic phenomena for their own sake rather than in terms of their obvious relevance to pressing social problems or their compatibility with received theory, then the multiplier would probably have emerged before it did.

Be that as it may, it is obvious that only chance could assure that the period of public concern with a problem would be at least as long as the gestation period for scientific discovery. Hence, when public concern governs the duration of scientific work, the scientific yield tends to be smaller than would otherwise be the case. In addition, of course, the pressure for immediate results often necessitates the use of "quick and dirty" methods, which further deteriorates the scientific value of the work. Granting that many economists must be deployed in applied work, the scientific residue left by their efforts would be increased if, after the emergency has passed, they undertook as a matter of routine to develop the more general aspects of their work.

The second impediment to scientific progress posed by a preoccupation with applied problems is the lack of any necessary identity between socially-urgent problems and scientifically-promising ones. Any given phenomenon which gives society trouble is usually a member of a larger class the other members of which do not. A study of one of the latter may be scientifically more illuminating, and in the end may permit a more effective assault on the troublesome member than would a frontal assault upon it.

Unfortunately, when time seems of the essence, a frontal assault which yields a rough but useful answer is always preferred by policymakers. In some cases, such preferences are legitimate; i.e., when the quick solutions we give are indeed useful, and the problems they ameliorate are indeed serious.

Even in such cases, however, let alone when these criteria are not satisfied, there is a cost, both to our science and to society at large. The cost to our science is obviously slower progress than would have prevailed had the same resources been allocated to scientifically more promising work. The social cost, which follows from the scientific, is

our reduced ability to solve future social problems; for to the extent that our future understanding is diminished by past concentration on social problems, our ability to cope with future social problems is likely to be impaired.

In recent decades medical researchers and their sponsors have begun to awaken to this truth, partly no doubt because such major problem areas as cancer have proved too refractory thus far to yield to the half-blind empiricism which characterized so much earlier research. The earlier approach yielded, tardily to be sure, thousands of specifics for individual diseases and eased and prolonged life. Yet perhaps more often than not the reasons why they work or fail are not known. Because the necessary fundamental knowledge was not developed, each disease entity was *sui generis*. The more recent strategy, which emphasizes the development of fundamental knowledge, is, from the standpoint of treatment, a roundabout method of production, but like other roundabout methods it is calculated to enhance the ultimate yield. Because of the greater understanding to which fundamental research leads, the future lag between the commencement of research on a disease and the discovery of a cure should be reduced.

This lesson, which medical scientists have begun to learn, is also one which experience has driven home recently to American engineers who generally had to draw on Europe in the past for advances in engineering theory. We economists, who are just beginning to study the formation of intellectual capital, should not be among the last to recognize that the economies of more roundabout methods of production apply to the production of knowledge as well as to the production of other goods.

As suggested above, however, for that roundabout process to be as productive as possible, it seems essential that the boundaries of the terrain explored reflect not only those established by received theory but also those set by nature, for the gap between these two remains large.

DISCUSSION

MORRIS A. ADELMAN: Mr. Schmookler's castigation probably benefits our souls: we have done the things we should not and not done the things we should. But it is unproved and probably untrue that the "discovery" that new knowledge is more important than new physical capital came as a surprise to most economists. Hence most of what he says is beside the point. Truly, in discussing national economic growth and development, too many (even one is too many) emphasize tangible capital formation. The main cause is not a bad academic tradition, but rather an unjustified bad conscience, producing a wish to believe that our only advantage lies in a massive inheritance of material wealth which, to complete the popular notion, was somehow stolen from the underdeveloped countries, say through rigging the terms of trade.

In the analysis of markets generally, including technical change, static versus dynamic models, exogenous or endogenous, etc., is mostly false opposition, a neglect of what Morris Cohen called the "principle of polarity" that "opposite categories like . . . rest and motion . . . must always be kept together though never identified." Schumpeter (whom Professor Schmookler puts outside the "orthodox tradition" for unstated and, I fear, unacceptable reasons) demonstrated that in the static framework, competition eroded business profit. The only escape from living on subsistence rations was innovation—creating new profits while the old were being destroyed. The purely competitive, static, stationary framework is necessary though not sufficient to explain why new production functions appear and the system is gradually transformed. Hence the law of variable proportions, etc., does help explain the drive toward technical changes. To deny this is to overlook what business activity is all about: the pursuit of profit.

As Professor Schmookler truly says: "An industry in long-run equilibrium in the neoclassical sense will not be in equilibrium at all." An equilibrium system is needed to explain not why nature is at rest but why she is not. The unceasing complaints, of which Professor Schmookler's is one of the most restrained and reasonable, against theories of pure or perfect competition reflect confusion between description and analysis and neglect of the principle of polarity.

The idea that necessity is the mother of invention is as true as that invention is the mother of necessity. The hope of achieving ship-to-shore communication made people try, three-quarters of a century ago, to put the Hertzian waves to work. Their initial success gradually exploited on an increasing scale could once be called the radio industry, then radio-television, then radio-television-radar or other exploitations of the vacuum tube, and turned, with the development of solid state physics and its application, into a vast amorphous field we now call electronics, none of it foreseeable or foreseen. At every stage, needs and preferences are as much shaped by what is available as vice versa.

Finally, I sympathize altogether with Professor Schmookler's demand that

we think less of the public issues of the moment and more of knowledge for its own sake.

In Professor Markham's review of literature, however critical of particular results, cheerfulness not only keeps breaking through, but he makes no attempt to repress it. Possibly we tend to overvalue these studies. Compelled for so long to hear so much empty talk about concentrated power and conglomerate giantism, it is refreshing to see people trying to find things out rather than tell us what we must do to be saved. But if attacks on bigness would give us stagnation, not progress, it by no means follows that the more bigness or the more market power, the more progress. This is why regression analysis is educational even where it fails. All firms have size, in a continuous spectrum from very little to very much, and the real questions are not either/or but more or less. To be sure, regression analysis gets us into some uncomfortable dilemmas. If in one work we take all large corporations, neglecting industries, the results are meaningless. There is no economic logic in treating the one hundred biggest industrials or the two hundred biggest something else as though they were a competitive group vis-à-vis the rest of the economy. But if we begin subdividing firms by industry divisions, the cell numbers dwindle alarmingly. Some refinement of methods on the purely statistical side is no doubt to be expected, but they will need supplementing with nonstatistical evidence on market structure. I do not think the performance of the steel industry and the lag in the oxygen process and continuous casting can be understood except in terms of a rather unusual type of industry cohesion.

Professor Mansfield takes an impressive pioneering swipe at what he truly calls the enormous difficulties, conceptual and practical, of estimating marginal rates of return from industrial R and D. His plea for extreme caution in interpreting his results is well taken. There is only time to state summarily some worries about his methods. First, neglect of time lags between R and D expenditures and their fruition: whatever the mean, I would expect a wide and very skewed distribution. Second, the extent to which greater productivity in the innovating industry is competed out of it to the benefit of customers and suppliers: as earlier stated, "passing-on" is the heart of our economy, but it varies with both natural and contrived barriers to imitation. I think this is a separate problem from the depreciation expressed by the coefficient lambda.

The marginal returns seem entirely private, since the spread between costs, including wages and receipts, could well reflect monopoly power.

In petroleum, the enormous diversities between domestic and foreign, and within each group, may dominate the results. Moreover, the problem of measuring value added in the combined domestic and foreign production and refining-marketing sectors is a research project in itself. Finally, I must confess to not understanding why the models assume all technical change as cost reducing, not product improving. It would perhaps be an even more grievous deficiency than Mansfield indicates, since the division between the two must vary greatly among industries. But in either case the result of an innovation is to increase output measured in value terms. The models seem, therefore, to represent the combined effect of the two classes. I have probably missed something, but since others may miss it, too, Mansfield might well add a sentence or two in time for the printed *Papers and Proceedings*.

ZVI GRILICHES: I would like to support Schmookler's sermon by elaborating on one of his points. The fact that standard economics had no theory of technical change explains, I think, why when we got around to trying to measure it, we had to measure it as the "residual." Because it was an empty box, we proceeded to define it as everything that cannot be explained by standard theory. This procedure while inefficient was quite valuable, since it pointed out quickly that the empty box was of enormous proportions, accounting for more than two-thirds of the observed growth in output. This was clearly an unsatisfactory state of affairs and led to several proposals for reformulation. Unfortunately, the two major recent reformulations—"embodiment" and "learning by doing"—do not appear to be of much help. The hypothesis of embodiment, while potentially very fruitful, in practice turned out to be nothing more than a relabeling of an already empty box. Nor have facts been found to support the learning by doing hypothesis which tried to connect the rate of growth of productivity (or technical change) with the rate of investment. There is some evidence that the rate of investment affects the level of patenting or R and D effort, but very few links have been forged at the aggregate level between these and the rate of technical change as conventionally measured.

This, however, may be more a reflection on the way we measure "technical change" than on the relevance of the particular hypotheses. In my own work, I have abandoned the idea of technical change as the residual requiring my model to explain all of the growth in output. This is achieved by considering such additional variables as the accumulation of human capital, the improvement in the quality of physical capital, the capitalized value of R and D expenditures, the uncounted public (external) investments, and the contribution of economies of scale. While some of this may appear to be a tautological redefinition of technical change, it has important implications for research strategy, telling one which additional pieces of information to look for.

Some of these difficulties are also illustrated by Mansfield's otherwise very interesting paper. He sets out to do two things: (1) to connect conventional measures of technical change with data on R and D input and (2) to compute the implied rate of return to R and D. The second part is very useful, even though the dependent variable chosen (residually measured technical change) is unlikely to be very well measured. It is important to realize, however, that all the computed rates of return are estimated assuming that the model is correct. The only empirical support provided in the paper for the link between R and D expenditures and the production function is one correlation, connecting the rate of embodied technical change with the rate of growth in R and D investment in nine chemical and petroleum firms. This finding is subject to some doubt, since in other cases where such comparisons could have been made, the results apparently are not as good. There is almost no correlation between the estimated embodied and disembodied technical change for these firms (the rank correlation is only .2), nor is there much correlation between these rates at the aggregate two-digit level ($\rho = .1$). If one tries to reproduce similar numbers at the aggregate two-digit level using published R and D data for 1956-62 (this is legitimate, since it is assumed

that the rate of growth of R and D is constant in each industry), one gets nonsignificant negative relationships between the disembodied rate of technical change (as measured by Massel for fourteen industries) or the embodied one (as measured by Mansfield for nine industries) and the rate of growth in R and D expenditures. Thus, as much as I would like to believe in the influence of R and D expenditures on productivity, I have still to bring in a verdict of "not proven" (at least at the aggregate two-digit manufacturing industries level).

Because one is trying to explain residually derived measures of technical change, be they embodied or disembodied, the whole procedure is very brittle. These constructs are subject to substantial errors of measurement and to the influence of short-run cyclical variations. Also, since they are both supposed to be measuring the same thing, the lack of correlation between the two measures illustrates the range of uncertainty implied by such a procedure. If technical change is measured residually, using the same numbers and procedure, one should get essentially the same results whether one assumes that it is embodied or disembodied—there is only one residual in the system, and it does not matter where one puts it. (See Jorgenson's forthcoming paper on this.) An operational difference between the embodied and disembodied hypotheses arises only if one adds the convenient but unjustified assumption of a constant rate of x-type technical advance. This is a very weak reed to lean on. The embodiment hypothesis is very suggestive, but to be fruitful it must lead us to the use of new and independent pieces of evidence. One cannot get much from relabeling alone.

RICHARD A. TYBOUT: Schmookler has made a strong case for the treatment of technology as an endogenous variable. His comments bear the weight of years of achievement directed to this very end. If technology is an endogenous variable—and I agree that it is—then "economic theory is inaccurate even with respect to the phenomena with which it purports to deal."

The growing importance of empirical economics undoubtedly has had much to do with the renaissance of the economics of technical change. However, I would also argue from the sociology of learning that we will proceed by analogy with subjects with which we are already familiar. Attractive though it may appear to take the promised land by air drop behind the lines, I am afraid that most of our progress must be made on the ground. The way is rough, but there are stout hearts in our company.

Mansfield has pioneered again, this time in the estimation of marginal rates of return from R and D expenditures. His model separates returns from a firm's own R and D program from imported technologies. He does not claim to have made all the important distinctions, but it would be hard to find a more important goal for research in this area today. Mansfield's coefficient of external technology, a_1, is a residual at best. It includes gains in labor productivity (in the organizational model), productivities from knowledge as a collective good, benefits from improved equipment made available by suppliers and like effects that will have to be separately identified before questions of social spillover can be posed, let alone resolved. It is also possible

that a_1 is correlated with the internal return from a firm's own R and D if labor education and retraining are integrated with development itself. Maintaining one's status in a patent pool might induce the same kind of correlation.

Some of the most interesting difficulties can arise from the new-product problem, though it is not clear at the moment how serious this problem will turn out to be. As Mansfield recognizes, his rate of return refers to whatever the output of R and D might be, including new products. The latter may represent increased utility of output per unit input of the firm, or they may simply provide a means of achieving temporary monopoly profits, though the requirement of higher than normal profits is not necessary if growth is maximized subject to a minimum profit constraint. I refer to recent work by Robin Marris and others on the managerial theory of the firm wherein new-product R and D is a part of sales promotion.

Two kinds of results would be expected from new-product R and D: (1) an overstatement of the return to the R and D actually conducted (as distinct from derived monopoly) and (2) more important for Mansfield's model, a variation among firms in the same industry in the rate of return on own R and D. The latter could result from differences in the extent to which a low return is acceptable for the sake of growth through new product markets. Mansfield reports that he regressed b (or b'), his total rate of returns, against rate of growth with nonsignificant results. If the same kinds of results are obtained in general for firms in the same two-digit industries (not chemicals and petroleum combined), then at least the second aspect of the new-product problem can be ignored.

The new-product problem would hardly seem to be ameliorated, as Mansfield holds, by his choice of the chemical industry. For example, Monsanto Chemical Co., with the third largest sales in the industry in 1963, estimated in its annual report for that year that over 50 percent of its sales and more than 60 percent of its profits were derived from more than 500 products introduced in the preceding ten years. I agree with Mansfield's view that a_2, the elasticity of output from own R and D, "would be expected to vary considerably from one industry to another," and for this reason, feel quite uncomfortable about the necessity of combining five chemical firms and five petroleum firms to get the a_2 used in Table 1.

It would seem that there is an opportunity to draw further inferences on the state of the real world and/or the validity of the model by comparing the chemical firms in Table 1 with the chemical industry in Table 2. There is a disturbing reversal in the relative importance of organizational and capital-embodied rates of return for these two groups. Is this due to the fact that a high value of x would be appropriate, or to an unrepresentative collection of firms in Table 1, or to an extraordinary distribution of lower limits? If the firms in Table 1 are unrepresentative, then we can infer that there is a high variation in the ratio of organizational to capital-embodied technology within an industry. A comparison of the relative b and b' values in Table 1 with those in Table 2 suggests that this is the case. Insofar as the Table 2 rates of return are lower limits, this fact need not prevent a comparison by using either the Table 1 values of a_2 for the chemical industry in Table 2 or the

Table 2 values of a_2 (which are zero) in Table 1 for the chemical firms. Note that it is not appropriate for the purpose at hand to get an industry value of a_2 that will internalize to the industry R and D cross effects external to the individual firms. The two concepts have quite different policy significance and require different a_1 concepts to go with them. Mansfield's discussion does not make the distinction, which may account for his conceptual inability (except in the special case in note 19) to estimate industry-wide a_2 values.

Markham has summarized the current state of knowledge on the relation of firm size to R and D expenditures. To interpret the weak relation he finds, it seems in order to recall the late Joseph Schumpeter's distinction between monopoly profits as a necessary and as a sufficient condition for R and D support. Schumpeter emphasized interindustry competition in his analysis of "creative destruction." His example was the Aluminum Company of America in competition with other producers of primary metals, not the giants of the tobacco industry. Markham interprets R and D as an aspect of oligopoly strategy. This is in accord with the original Schumpeterian view (as opposed to the neo-Schumpeterian) and suggests an emphasis on new product R and D. It is on the problem of relating the composition of R and D output to growth and market position that the larger payoffs seem in store for Markham's analysis.

Markham refers to our failure to understand oligopoly price and output policy. I consider this failure less of a warning than an encouragement. If R and D is an important aspect of oligopoly strategy, then we cannot expect to succeed until it has become an endogenous part of oligopoly theory.

THE NEW NATIONAL ECONOMETRIC MODEL: ITS APPLICATION

THE BROOKINGS-S.S.R.C. QUARTERLY ECONOMETRIC MODEL OF THE UNITED STATES: MODEL PROPERTIES*

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To determine the comprehensive properties of any nontrivial model of the economy is difficult. To do so for as large a system as the Brookings-S.S.R.C. model, one with over 150 structural equations and 75 identities, is an order of magnitude more difficult. There is much in which one might be interested: the specification of variables; the magnitudes of coefficients and elasticities in individual equations; the stability of these coefficients over time; the structure of the complete model; the predictive capabilities of individual equations and the system as a whole; the intertemporal and interequation covariances of the error terms; the independence of the exogenous variables from common trends and cycles; the sensitivity of individual equations and the entire system to estimation and observation errors, parameter shifts, and changes in initial conditions; the responsiveness to alterations in the values of policy instruments or other variables; and so forth.

The specification of equation variables and their coefficients, errors of estimate, and residual characteristics may be found in a volume on the model to be published in the near future.[1] As to the other items, we must, unfortunately, content ourselves with a discussion of only three; namely, due to time and space limitations, only the rough system structure, some parameter values and predictive capability of a representative set of individual equations, and the response multiplier can be covered. But first, some preliminary remarks on the current status of the model are in order.

A model building enterprise such as ours, that seeks to meld the theories and empirical findings of a large number of econometricians scattered across the country, is plagued by difficulties of coordination from the outset. Not only may each individual's equation specifications differ from those that the model's synthesizers would select, but, also, there is

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[1] *The Brookings-SSRC Quarterly Econometric Model of the U.S. Economy*, James S. Duesenberry, Gary Fromm, Lawrence R. Klein, and Edwin Kuh, editors (Rand McNally and North-Holland, 1965).

a tendency for incompatibilities in variable definitions and data sources to arise. Additionally, sometimes there are errors in copying and key-punching data and in performing transformations on the variables. Obviously, such incompatibilities and mistakes can cause coefficient estimates that are biased, inconsistent, and inefficient. Therefore, much effort was expended recently to minimize these problems in the present version of the model. All equations were refit (by ordinary least squares; simultaneous estimates will be run at a later date) to a compatible set of verifiable data. As might have been expected, when correct data were utilized, some of the coefficients and their errors of estimate changed significantly. Consequently, modifications in a number of equation specifications have been made and others may be in order. These will be undertaken in the near future. In spite of the need for these compatibility and specification changes, we think that the fresh and original ideas that came from the widespread group of contributors were extremely fruitful.

The research on the model, of course, is expected to continue over a long period. Thus, the equations will be subjected to periodic review for structural change and agreement with contemporary theory. In the results reported below, however, for the most part, we have employed the refitted initial specifications of the contributors to the original complete model.

Model Structure

As is now well known, we have constructed a block recursive system; this is, a system in which blocks of simultaneous equations feed into one another in a recursive causal chain. The endogenous values in each block are determined by the equation parameters, predetermined variables from lower order blocks, and exogenous variables.

From a block recursive ordering for a modified version of the system, it can be seen that investment anticipations, the lowest order block, depend only on lagged values of industry output and capital stocks. Here, as in many other sectors, the current endogenous variables also depend on their own lagged values.

In turn, investment realizations are a function of current and lagged anticipations and lagged investment, output, and profits. Residential construction also is a function of predetermined variables. Consumption, on the other hand, is intricately tied into a large simultaneous block of the system, since it depends on current prices and disposable income, as well as lagged values of itself. Inventory change, orders, labor requirements, and factor income formation also make up a large simultaneous block. Finally, we have market determination of prices, wages, and interest rates. These may be simultaneous in some cases with real demand, but they can be separated into industry blocks, as is shown in a later section on solution of the model.

The above ordering indicates the general structure of the model but gives little indication of the specific form of the equations or the nature of their parameters. These will now be discussed, following which a method of solution of the system will be outlined and multiplier responses to changes in the exogenous variables will be derived.

Some Equation Estimates

It is impossible to present equation estimates for the complete model within the confines of this brief article. Therefore, only a partial set of functions for the durable manufacturing sector will be shown.[3] (See Table 1; symbols are listed in Table 2. Summary statistics for the remaining equations may be found in Table 3.)

Generally, these functions and those entering into the more aggregative results that follow were fitted to quarterly, seasonally adjusted annual rate data for the period 1949–60. The initial quarter did vary somewhat from one equation to another and depended upon data availability and the lag structure of the included variables.

Because derivations and explanations for the functions of Table 1 may be found in the previously cited volume on the model, only a few comments on their form will be made here. The investment equation presented is an alternative to the two-stage process of investment intentions and realizations that probably will be employed in future simulations with the model. The alternative equation reflects the intentions-appropriation-delivery lag on current expenditures. The coefficients and elasticities seem reasonable, except for the positive sign on the long-term interest rate lagged one quarter. The standard error of estimate (.558) is somewhat high, but acceptable.

The inventory equation is of a type that has now become standard. The most interesting variable—and one not included in past inventory analyses—is the index of hours worked in manufacturing. This has an extremely high elasticity and most likely reflects the change in work-in-process inventories as production is increased. Unhappily, the coefficient of the sales term $(GNP^{54}_{D} - INV^{54}_{D} + GNP^{54}{}_{IC})$ is negative. However, it is not significant at the 95 percent confidence level. Also, the variance of the residuals is high.

The changes in hourly compensation of production and other workers in manufacturing takes into account the influence of increases in the cost of living, unemployment, profits, and past wage changes. As it stands, the equation mirrors the impacts cited by writers who have stressed the institutional and bargaining determinants of wage determination.

[3] At present, the other production sectors are agriculture, nondurable manufacturing, contract construction, trade, regulated industries, and residual industries. Within the next year, we hope to disaggregate to the two-digit level for manufacturing industries.

TABLE 1

PARTIAL SET OF DURABLE MANUFACTURING EQUATIONS: 1949–60*

INVESTMENT IN PLANT AND EQUIPMENT

$$I^{54}_{BUS_{MD}} = -.8648 - \underset{\substack{(2.52)\\(-.998)}}{.1450}\, K^{54}_{MD_{-1}} + \underset{\substack{(4.45)\\(1.001)}}{.0868}\, X^{54}_{MD_{-1}} + \underset{\substack{(4.78)\\(1.161)}}{.1037}\, X^{54}_{MD_{-5}} + \underset{\substack{(2.30)\\(.629)}}{1.1075}\, RM_{GBL_{-1}} - \underset{\substack{(-2.54)\\(-.631)}}{1.1663} RM_{GBL_{-5}}$$

$\bar{R}^2 = .759 \quad Se = .558 \quad DW = 0.56 \quad \Lambda = .047$

INVENTORY INVESTMENT

$$\Delta INV^{54}_{MD} = -115.02 + \underset{\substack{(2.34)\\(.012)}}{.0521}\, \Delta O^{54}_{U_{MD}} - \underset{\substack{(-2.21)\\(-4.53)}}{.0820}\, O^{54}_{U_{MD}} + \underset{\substack{(4.63)\\(173.)}}{147.05}\, J_{HP_{MD}} - \underset{\substack{(-1.25)\\(-34.2)}}{29.178}\, \frac{WPI_{MD}}{WPI_{MD_{-1}}}$$
$$- \underset{\substack{(-1.66)\\(-15.2)}}{.1144}[GNP^{54}_D - \Delta INV^{54}_D + GNP^{54}_{IC}] + \underset{\substack{(1.71)\\(15.3)}}{.5152} INV^{54}_{MD_{-1}}$$

$\bar{R}^2 = .665 \quad Se = 1.512 \quad DW = 1.49 \quad \Lambda = .274$

COMPENSATION OF EMPLOYEES PER MAN-HOUR

$$\frac{RWSS_{MD} - RWSS_{MD_{-4}}}{RWSS_{MD_{-4}}} = .007 + \underset{\substack{(6.62)\\(.262)}}{.8196} \left[\frac{1}{4}\sum_{i=0}^{3}\left\{\frac{CPI - CPI_{-4}}{CPI_{-4}}\right\}_{t-i}\right] + \underset{\substack{(2.72)\\(.476)}}{.0012}\, \frac{1}{RU^*}$$
$$+ \underset{\substack{(1.26)\\(.545)}}{.1717} \left\{\frac{1}{4}\sum_{i=0}^{4}\left[\frac{Z_{BU_{MD}}}{X^{54}_{DM}}\right]_{t-i}\right\}$$
$$- \underset{\substack{(-3.27)\\(-2.71)}}{.2770} \left[\frac{RWSS_{MD} - RWSS_{MD_{-4}}}{RWSS_{MD_{-4}}}\right]_{-4}$$

$\bar{R}^2 = .691 \quad Se = .014 \quad DW = 1.17 \quad \Lambda = .109$

EMPLOYMENT OF PRODUCTION WORKERS

$$E_{P_{MD}} = .7788 + \underset{\substack{(15.03)\\(.562)}}{.0671} X^{54}_{MD} - \underset{\substack{(-5.94)\\(-.359)}}{.0431} X^{54}_{MD_{-1}} - \underset{\substack{(-4.15)\\(-.126)}}{.0253} K^{54}_{MD_{-1}}$$
$$+ \underset{\substack{(1.95)\\(-1.00)}}{.0720} \Delta H_{P_{MD_{-1}}} + \underset{\substack{(12.9)\\(.815)}}{.8149} E_{P_{MD_{-1}}}$$

$\bar{R}^2 = .979 \quad Se = .081 \quad DW = 1.37 \quad \Lambda = .005$

* t statistics and elasticities, respectively, are shown immediately below the coefficients to which they apply; elasticities are evaluated at the variable means.

TABLE 2

LIST OF VARIABLES AND DEFINITIONS

All monetary variables are in billions of current dollars, seasonally adjusted. Monetary flow variables are at annual rates; stock variables are end of period. Lags or leads in variables are indicated by the general subscript i or plus or minus numbers; e.g., a lag of one period is $t-i$ with $i=1$, or simply (without t)-1.

Superscripts:

54 = constant 1954 dollars

Subscripts:

Subscripts are defined with the variable to which they apply

Producing Sector Subscripts:

GF = federal government
GS = state and local governments
MD = durable manufacturing
MN = nondurable manufacturing
O^* = all industries except agriculture, manufacturing, and trade
T = wholesale and retail trade

Variables

C = personal consumption expenditures, including imputations, billions of dollars
C_{DA} = personal consumption expenditures for new and net used automobiles and parts, billions of dollars
C_{DEA} = personal consumption expenditures for durable goods other than new and net used automobiles and parts
C_{NF} = personal consumption expenditures for food, billions of dollars
C_{NEF} = personal consumption expenditures for nondurable goods other than food, billions of dollars
C_S = personal consumption expenditures for services, billions of dollars
CCA = capital consumption allowances, billions of dollars
CPI = consumer price index, 1954 = 1.00
CPI_{SR} = rent component of consumer price index, 1954 = 1.00
$CURR$ = currency liabilities of the Treasury and Federal Reserve, less commercial banks' currency holdings, end of quarter, billions of dollars
DD = private demand deposit liabilities of commercial banks less interbank deposits, cash items in process of collection, and Federal Reserve float, billions of dollars
Δ = first difference operator
DW = Durbin-Watson statistic
E = employment, billions of persons
E_O = employment of overhead workers, billions
E_P = employment of production workers, billions
EX = U. S. exports of goods and services, billions of dollars
G = government purchases of goods and services, billions of dollars
G_{CD} = government purchases of durable goods, billions of dollars
GNP = gross national product
GNP_D = durable goods component of gross national product, billions of dollars
GNP_{IC} = construction component of gross national product, billions of dollars
H_P = average work week of production workers, hours
HU_{STS} = number of private housing units started, millions
HU_{VAC*} = adjusted vacant available housing units, millions
I_{BUS} = business gross investment in plant and equipment, billions of dollars
I_{CNFR} = GNP expenditures on nonfarm residential construction, billions of dollars
I_{PDE} = gross private domestic investment in producers' durable equipment, billions of dollars
INT_{BUS} = personal interest income paid by business, billions of dollars
INV = business inventory stock, billions of dollars
INV_D = inventory stock of durable goods, billions of dollars
$IVAC$ = corporate inventory valuation adjustment, billions of dollars (IVA = total; $IVAN$ = noncorporate; $IVAC = IVA - IVAN$)
J_{HP} = index of hours worked by production workers, 1954 = 1.00
K = stock of business capital, billions of dollars
Λ = Theil inequality coefficient (U in his notation)
M_{FIN} = imports of finished goods and services, billions of dollars

TABLE 2 (*continued*)

LIST OF VARIABLES AND DEFINITIONS

M_{EFIN} = imports of crude materials, crude foodstuffs, and semimanufactures, billions of dollars
O_U = manufacturers' unfilled orders, billions of dollars
P_{ICNFR} = implicit price deflator for GNP expenditures on nonfarm residential construction, 1954 = 1.00
PV = implicit price deflator of gross product originating, 1954 = 1.00
$\bar{R}^2$ = percentage explained variance corrected for degrees of freedom
RE = undistributed corporate profits, billions of dollars
RM = interest rate, percent
RM_{FRB} = Federal Reserve Bank of New York discount rate, percent
RM_{GBL} = average yield during quarter on U. S. securities maturing or callable in ten years or more, percent
RM_{GBS3} = average market yield on three-month U. S. treasury bills, percent
RU = rate of unemployment, fraction
RU^* = five-quarter average of RU centered on $t-2$, fraction $RU^* = \frac{1}{8}RU_{-4} + \frac{1}{4}RU_{-3} + \frac{1}{4}RU_{-2} + \frac{1}{4}RU_{-1} + \frac{1}{8}RU$
RW = compensation per employee, dollars per employee
$RWSS$ = compensation of employees per man-hour, dollars per hour
Se = standard error of estimate
TC = corporate profits tax accruals to government, billions of dollars
$TIME$ = time trend, 1947:1 = 1.
TP = personal tax and nontax receipts (or payments), billions of dollars
TW = contributions for social insurance, billions of dollars
TX = indirect business tax and nontax accruals, billions of dollars
TX^* = indirect business tax and nontax accruals plus business transfer payments, billions of dollars
V_G = government transfer payments to persons, billions of dollars
WPI = wholesale price index, 1954 = 1.00
X = gross product originating, billions of dollars
Y_D = disposable personal income, billions of dollars
Y_{ENT} = proprietors' income, billions of dollars
Y_P = personal income, billions of dollars
Z_{AU} = corporate profits after tax but before inventory valuation adjustment, billions of dollars
Z_B = corporate profits before taxes including inventory valuation adjustment, billions of dollars

Finally, employment of production workers in durable manufacturing is dependent on the current level and change in output originating, the capital stock, the change in hours worked, and the beginning of period employment. These, too, are theoretical, classical influences on the industry's employment.

As a group, the functions for durable manufacturing are tolerably accurate over the period of fit, although the inventory equation might be regarded as an exception. However, a significant fraction of the error in this function arises from the inability to account for inventory behavior during the 1959–60 steel strike period. For the most part, the residuals for 1961–62 are equivalent or superior to those during the period over which the equations were estimated. (See Table 3.) The exceptions are inventory investment and employment and hours of production workers. These residuals are explained by the failure accurately to trace the recession in early 1961.

TABLE 3

SUMMARY STATISTICS FOR DURABLE MANUFACTURING EQUATIONS

Dependent Variable*	$\bar{R}^2$	Se	DW	Δ	Residual Errors 1961–62	
					Se	$\lvert e \rvert$
I^{54}_{BUS}	.759	.558	0.56	.047	.610	.256
ΔINV^{54}	.665	1.512	1.49	.274	2.540	1.140
$f(RWSS)$†	.691	.014	1.17	.109	.012	.014
E_P	.979	.081	1.37	.005	.139	.146
H_P	.939	.199	1.99	.002	.258	.164
E_O	.998	.017	0.61	.004	.016	.010
ΔY_{ENT}	.596	.032	1.42	.344	.015	.009
IVA	.928	.214	1.60	.105	.112	.092
$IVAN$	.733	.015	1.06	.217	.009	.008
ΔCCA	.271	.038	0.53	.216	.264‡	.216‡
$\Delta(TX^*_{MD}/TX_{ALL})$	.729	.002	2.13	.267	.003	.002
TC	.968	.153	2.64	.010	n.a.	n.a.
PV	.974	.015	0.92	.006	.014	.009
WPI	.951	.019	0.59	.008	.010	.021

* Unless otherwise specified, for durable manufacturing.
† $(RWSS_{MD}-RWSS_{MD-4})/RWSS_{-4}$.
‡ Does not take into account revision of the tax laws in 1962 granting investment tax credits.

Output originating and profits may also be derived for the industry. Output originating is calculated by multiplying the inverted matrix of input-output coefficients by a vector of industry final demands. This latter vector is determined by a set of regressions of final demands on GNP components. As an alternative to this process, it is also possible directly to relate industry output originating to a linear combination of appropriate GNP components; e.g.,

$$X^{54}_{MD} = f(C^{54}_{DA}, C^{54}_{DEA}, I^{54}_{PDE}, G^{54}_{CD}, EX^{54}_{D}). \tag{1}$$

Given the endogenous determination of all other income items, profits may then be estimated as an endogenous residual.[8] The identity used is:

$$Z_B = PV\cdot X^{54} - RWSS[52(E_P\cdot H_P + E_O\cdot 40)] - Y_{ENT} - INT_{BUS} - CCA - TX^* - IVAC. \tag{2}$$

These computations were performed for 1961–62 and produced residuals for actual less predicted profits of about $2–3 billion, or an error of 15–20 percent. Most of the difficulty could be traced to a predicted overstatement of the rate of compensation of 2–3 percent; further efforts will have to be devoted to improving the accuracy of the compensation equations.

[8] Profits after tax are derived from the additional identity: $Z_{AU}=Z_{BU}-TC$, where $TC=f(Z_{BU})$.
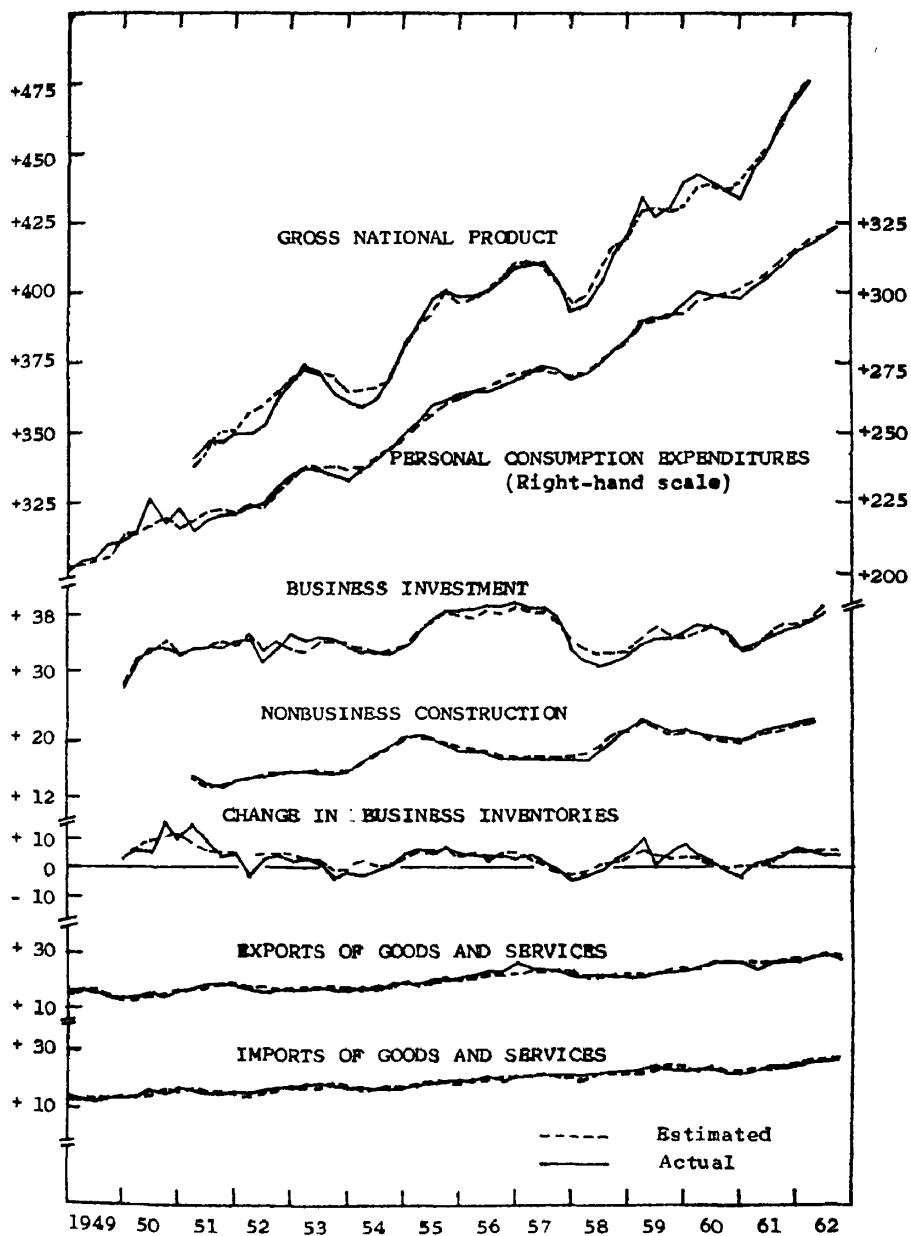
FIGURE 1

GROSS NATIONAL PRODUCT DEMANDS
(Billions of 1954 Dollars)

It is also interesting to examine the residuals when the predicted values of disaggregated equations are summed. These are shown in Figure 1. As above, the period of fit was 1949–60 and predicted and actual values were gotten for 1961–62. Notwithstanding the deceptions of graphic scale, the aggregation across equations produced surprisingly small total predictive errors. Individual equation errors frequently were of opposite sign and tended to cancel each other. This, of course, may not be true of complete system solutions.

The System as a Whole

Even though each individual equation or block of equations looks reasonable or statistically good (high correlation, small sampling errors, expected qualitative effects, random residuals, etc.), the whole group of equations may or may not function well as a system. It remains for us to determine how the complete model extrapolates or generates simulated time paths of endogenous variables. This system, by its very size, however, is difficult to treat as a whole.

The estimation of any single equation, once data were available, was not difficult, because each equation is linear in the parameters. In our final system, however, there are many nonlinearities in the form of combinations of endogenous variables. This is a major complication at the present stage of testing and applying the whole model. Recursiveness was of great assistance in simplifying estimation procedures, and would help a great deal in algebraic solution for endogenous variables. Unfortunately, the system is not fully recursive. In a maximum delineation of recursive ordering, we are still left with a large simultaneous block.

The first step in solving the system is to decide what is exogenous or predetermined and what is endogenous. We have a large agricultural sector and a substantial labor supply sector. More work needs to be done in consolidating and sharpening the equations in these groups. For the present discussion, we shall assume that agricultural income, labor supply, family formation, and government purchases of goods and services are given. This greatly reduces the complexity of the system. The remaining variables to be determined are *GNP* expenditures (consisting of several components of consumption, fixed and inventory capital formation, imports, and exports), orders, factor incomes (decompositions of wages, profits and rentier income for individual sectors), output originating by sector, prices, wage rates, and employment by sector, interest rates and currency and demand and time deposits.

A stage below the exogenous category are endogenous variables that are functions of predetermined variables alone. An example is housing starts, which depends only on lagged or exogenous variables. The equation is:

$$(3)\quad HU_{STS} = .3140 - \underset{\substack{(-4.3896)\\(-0.2027)}}{0.1272}\,\frac{1}{3}\sum_{i=1}^{3}(RM_{GBSi})_{-i} + \underset{\substack{(4.5259)\\(0.3987)}}{0.3966}(HU_{STS})_{-1}$$

$$- \underset{\substack{(-2.1766)\\(-0.2865)}}{0.2470}(HU_{VAC*})_{-1} + \underset{\substack{(4.3448)\\(1.3884)}}{0.0131}TIME$$

$$+ \underset{\substack{(1.3884)\\(0.4922)}}{0.6908}\left[\frac{CPI_{SR}}{P_{ICNFR}}\right]_{-1}$$

$$\bar{R}^2 = .847 \qquad Se = .0606 \qquad DW = 1.4403$$

(Symbols are defined in Table 2; t statistics and elasticities at the means, respectively, are reported parenthetically.)

Other examples are the business investment equations, which, together with residential construction, form the first wholly predetermined block. For a stated extrapolation period, the first quarter of 1961 in the present context, we assign prior or given values to each of the right-hand variables and compute the fixed extrapolation values of the left-hand (dependent) variables. This can be done for the fixed investment equations, but it is not possible for inventory investment.

Consumer spending, inventory investment (via an orders-backlog sequence), imports, tax receipts, and government transfers are all *GNP* components of the model that depend, in part at least, on current activity or market conditions. When effective demand for total output is determined within our model, the production functions establish labor requirements. (Because fixed capital formation depends only on lagged variables, the requisite capital input variables of the production functions are predetermined from quarter to quarter.)

The determination of wages, prices, and interest rates in terms of activity, employment, unemployment and monetary developments enables us to translate labor requirements into the wage bill and durable expenditures into interest income. The national accounting identities then yield profits as a residual, completing factor shares. This is a loosely and roughly telescoped way of presenting the structure and causal sequence within the model.

If the system were completely linear, it would not be difficult, at least in principle, to obtain a solution and calculate simulation-forecast-multipliers. Our system, however, is nonlinear in the variables (not in the unknown parameters that we estimate from the sample), and this poses difficulties in application. The kind of nonlinear structure en-

countered in the model is subtle and important. It comes about in our model building in a way that is likely also to occur in a wide variety of other models. In fact, we might argue that it is inherent in the nature of economic structure and that it is unrealistic to think that an economy can be represented by the standard formulation

$$Ay_t + Bz_t = \epsilon_t \qquad \begin{array}{l} y_t = \text{endogenous variable vector} \\ Z_t = \text{predetermined variable vector} \\ \epsilon_t = \text{residual vector)} \end{array}$$

found in most econometric textbooks.

Since our space is severely limited, and since the Brookings-S.S.R.C. model is large and cumbersome, let us examine the nonlinear character of the system through a simplified, highly condensed model.

Fixed Investment

$$(4) \qquad \overset{54}{I}_{BUS} = a_o + a_1\overset{54}{X}_{-1} + a_2\overset{54}{K}_{-1} + RM_{GBL_{-1}}$$

Nonfarm Residential Construction: wholly predetermined

$$(5) \qquad \overset{54}{I}_{CNFR} = b_o + b_1\frac{1}{3}\sum_{i=0}^{2}(HU_{STS})_{-i}$$

Inventory Investment: durable and nondurable manufacturing

$$(6) \qquad \Delta \overset{54}{INV}_{MD} = c_o + c_1\overset{54}{X}_{MD}$$

$$(7) \qquad \Delta \overset{54}{INV}_{MN} = d_o + d_1\overset{54}{X}_{MN}$$

Consumption Function

$$(8) \qquad C^{54} = e_o + e_1\left[\frac{Y_D}{P}\right]$$

Output Decision Equations: durable and nondurable manufacturing

$$(9) \qquad \overset{54}{X}_{MD} = f_o + f_1C^{54}$$

$$(10) \qquad \overset{54}{X}_{MN} = g_o + g_1C^{54}$$

Labor Requirements: durable and nondurable manufacturing

$$(11) \qquad E_{MD} = h_o + h_1\overset{54}{X}_{MD}$$

$$(12) \qquad E_{MN} = j_o + j_1\overset{54}{X}_{MN}$$

Personal Tax Function

$$(13) \qquad TP = k_o + k_1Y_P$$

Corporate Savings Function

(14) $$RE = m_o + m_1 Z_B$$

Price Formation: markup over unit labor cost

(15) $$P = n_o + n_1 \frac{RW(E_{MD} + E_{MN})}{X^{54}}$$

Compensation Rate

(16) $$\Delta RW = q_o + q_1(L - E_{MD} - E_{MN}) + q_2 \Delta P$$

Interest Rate Determination

(17) $$RM = r_o + r_1 RM_{FRB} + r_2 \left[\frac{P \cdot X^{54}}{DD + CURR} \right]$$

Identities

Gross Output

(18) $$X^{54} = X^{54}_{MD} + X^{54}_{MN} + \epsilon$$

Gross Expenditure

(19) $$X^{54} = C^{54} + I^{54}_{CNPR} + I^{54}_{BUS} + \Delta INV^{54}_{MD} + \Delta INV^{54}_{MN} + G^{54}$$

Disposable Income

(20) $$Y_D = Y_P - TP$$

National Income

(21) $$P \cdot X^{54} = RW(E_{MD} + E_{MN}) + Z_B + RM \cdot P \cdot K^{54}$$

Personal Income

(22) $$Y_P = P \cdot X^{54} - RE$$

The symbols used above are defined in Table 2.

In this condensed model lags are deleted except to show how those in fixed capital formation start the system going. Nevertheless, all equations include possible lags or cumulative variables. These and other predetermined variables are consolidated in the constant term of each equation. In the actual model, there are several types of consumption and inventory investment. Here they are condensed except to show how a decomposition of output affects different inventory components. It is this decomposition that introduces an input-output structure into the complete model. There we have many price levels. Here we deal with only one. Capital in the production functions can be considered to

be included in the constant terms for a one-period solution, since fixed investment is determined in a pure lag relationship. In the simplified model, national income and product are assumed to be identical and foreign trade is neglected. Taxes are used without adjustment for transfers, and all profits are assumed to arise in the corporate sector. Wage rates are determined by the familiar adjustment pattern to unemployment and price change. The interest rate depends on the discount rate and velocity.

In many respects, the condensed model resembles the main processes of the Brookings-S.S.R.C. model, at least those that give rise to special problems in solving the system. An important property of the model is that if market variables (wage rates, prices, and interest rates) are given, a linear real system determines output, employment, and the composition of final demand. Equations (4) through (13) and (17) through (21) can be solved linearly for

$$I^{54}_{BUS},\ I^{54}_{CNFR},\ \Delta INV^{54}_{MD},\ \Delta INV^{54}_{MN},\ C^{54},\ X^{54}_{MD},\ X^{54}_{MN},\ E_{MD},\ E_{MN},$$
$$TP,\ RE,\ X^{54},\ \epsilon,\ Y_D,\ Z_B \quad \text{and} \quad Y_P,$$

if values are assumed for RW, P, and RM. The stock of fixed capital (K^{54}) is determined once investment is explained. Next, if E_{MD}, E_{MN}, and X^{54} are given, values of RW, P, and RM can be derived from linear equations. A proposed method of solution is, first, to assume initial values of RW, P, and RM for some given quarter. Second, solve the linear equations for E_{MD}, E_{MN}, X^{54} and the other 13 unknowns. Third, using E_{MD}, E_{MN}, and X^{54} in equations (15) through (17), obtain (linearly) second estimates of RW, P, RM for the same quarter. The whole process is repeated until the values calculated on successive iterations remain approximately constant.

On small systems of about 35 equations, this procedure has been found to converge. We are attempting to employ this method on the complete Brookings-S.S.R.C. model. As a first step, given initial values for wages, prices, and interest rates, a 62×62 linear block of real output and employment equations has been solved. In this solution, the business investment and housing equations were pure lag relationships and agricultural income and labor supply were exogenous. This yields a "real multiplier" matrix. Successive iterations will then produce compatible solutions for prices and outputs for later periods.

The inverse matrix of the system of jointly dependent variables, given lags, exogenous variables, wage rates, prices, and interest rates corresponding to the first quarter of 1961, is an impact multiplier matrix of one calendar quarter duration. Our inverse has 3,844 entries and is therefore difficult to summarize in this paper. However, some of the entries refer to well-known concepts in macroeconomic analysis and

can be cited individually. For example, the system produces autonomous expenditure impact multipliers of real *GNP* with respect to the constant term of equations explaining expenditures and tax variables. (See Table 4.) Analogous values can be computed for changes in exogenous variables. More complicated multipliers associated with coefficient parameter changes (e.g., tax rates) or prices and wages would not be constant and would have to be evaluated by solving the system under successively altered conditions and examining the change in the soultions for jointly determined variables.

TABLE 4

ONE-QUARTER IMPACT MULTIPLIERS: CONSTANT DOLLAR GROSS NATIONAL PRODUCT WITH RESPECT TO AUTONOMOUS CHANGES IN EXPENDITURE AND TAXES WITH GIVEN WAGES, PRICES, AND INTEREST RATES

(Figures rounded to nearest tenth)

C_{DEA}^{54}	1.0
C_{DA}^{54}	1.0
C_{NF}^{54}	1.3
C_{NEF}^{54}	1.2
C_{S}^{54}	1.0
ΔINV_{MD}^{54}	1.1
ΔINV_{MN}^{54}	1.1
ΔINV_{T}^{54}	1.0
$\Delta INV_{O^*}^{54}$	1.0
M_{FIN}^{54}	−1.1
M_{EFIN}^{54}	−1.1
TP	−0.5
$TX_{GF} + TX_{GS}$	−0.05
TC	−0.04
TW	−0.4
V_G	0.4
G	1.2

The whole set of 3,844 entries in the inverse matrix is too difficult to summarize easily and, indeed, many of the entries may turn out to be unreasonable on closer examination. Those elements about which we have some good a priori hunches look reasonable. It must be emphasized, however, that these are one-quarter impact multipliers. In a system like ours with many lags, longer term multipliers may be different. In most cases they will be larger (there are positive effects of lags). But some variables are affected negatively by cumulated stocks, and longer term multipliers associated with them may be smaller.

These calculations reported here are merely first steps in learning the properties of this large-scale system. In the months to come we expect to have many more concrete results.

ECONOMETRIC MODELS: IS A NEW AGE DAWNING?*

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This paper is meant to provide perspective on the uses of large-scale econometric models, such as the Brookings-S.S.R.C. model, by the economics profession. To do so requires a lightning sketch of econometric model building—as distinct from the generation of descriptive statistics, or theoretically-oriented but deterministic quantitative analysis. First, however, one must recognize that two wings of the economics profession will not find useful econometric models of any scale whatever. At one extreme are those who distrust most explicit theory and who put principal reliance upon institutional-historical study and tabulated material. At the other end of the analytical spectrum are those who are primarily concerned with the deductive implications of pure theory.

Between the two World Wars much effort was devoted to quantitative statistical study of agriculture markets, which reached its peak in the studies of Henry Schultz [17] [18]. In the same period Tinbergen's classic business cycle research [21], in part stimulated by Keynesian theory, provided the origins of modern quantitative business cycle analysis. National income accounting theory supplied further impetus, interacting with Keynesian constructs and leading to coherent bodies of data about national economies. Our domestic Tinbergen, Lawrence Klein, first developed econometric models as part of the methodological trench warfare which centered about the Cowles Commission from the mid-1940's to the early 1950's [6]. If the flowers that now blossom on that battlefield could only speak, they would, I think, tell us that the Cowles Commission won: good quantitative economics requires both appropriate economic theory and appropriate statistical methods. The exchange between Koopmans [10] and Vining [23] well illustrates that controversy. Paul A. Samuelson's insistence on operationalism in economic theory [15] provided concurrent intellectual support for testable propositions. However, the earlier polemics failed to stress enough that most economic theory lacked adequate descriptive content about behavior and institutions: the majestic phrase "a priori knowledge" cloaked massive areas of ignorance that confrontation with the data would soon enough reveal.

In business cycle theory, for instance, it became evident that the imposing and illuminating apparatus of differential and difference equa-

* Research support by the National Science Foundation in connection with the Brookings-S.S.R.C. model is gratefully acknowledged.

tions (mixed or pure, linear or nonlinear) provided at most an invaluable way of posing problems. These abstract mathematical procedures plus economic intuition (Samuelson's multiplier-accelerator article [16] and Frisch's random shock pendulum cycle model [2] offer excellent examples) do not begin to reflect many crucial aspects of behavior which subsequently have been shown to be of critical quantitative importance. In its original full generality the multiplier-accelerator model is a second-order difference equation so that, depending on parameter magnitudes, "anything can happen." It is quite remarkable, nevertheless, how much of these original propositions still remain basic to an understanding of business cycle behavior. Similarly, the main testable proposition from demand theory is that a simple or composite commodity's own compensated price elasticity should be negative and that in principle all relative prices and income determine quantity demanded (by an individual). Thus theory has provided only the most general sort of guidance, which many econometricians (see for instance Klein and Goldberger [9] and Suits [19]) found insufficient, turning instead to a sort of quantitative institutionalism; i.e., modeling behavior equations more and more closely on attributes which theory originally had perceived dimly or not at all.[1]

Econometrics presently stands at the intersection of theory and experience, as a much more sharply defined apparatus for developing and testing relationships than previously had existed, or been part of accepted practice. And not a few look with nostalgia on the empirically less insistent atmosphere of the past. The final ingredient in this list of the main foundations of large-scale quantitative economics has been the computer. It takes less time to whip off a multiple regression, together with an awesome battery of test statistics, than it does to read this paragraph. While far from an unmixed blessing, big fast computers are essential to estimation and manipulation of large econometric models.

The potential users of large-scale models like the Brookings-S.S.R.C. model can be classified for present purposes into three groups: forecasters, policy-makers, and research economists whose primary aim is better understanding of economic structure. While it is still too early to reach secure judgments, it appears reasonable to suppose that macroeconomic forecasters using present aggregate techniques will do at least as well and in some instances better than forecasts based on large detailed models. Their present mixed bag of tricks seems to work quite effectively: anticipations and consumer survey data in the context of standard aggregative models, running from the

[1] This compressed set of propositions is intended to be descriptive of the way matters seem to be rather than an expression of a "pro-theory" or "anti-theory" judgment.

recent five-equation Friend-Taubman entry [1] to the more elaborate Suits thirty-five equation forecasting model [19] have produced worthwhile results. Since quantitative analysis should be judged by its results, am I not tossing in the sponge before the struggle has begun? I think not, and for these reasons. To begin, the Brookings-S.S.R.C. venture is intended to model or reflect reality to an order of magnitude more closely than any previous venture of which I am aware; explicit ambitious attempts to portray behavior econometrically will often falter. In the interests of cumulative research this is all to the good. By pushing hard against the limits of knowledge, information about the sources of error—equation specification, messy data, or pure random noise—can be most effectively isolated since the detailed structure will be exposed to examination rather than being obscured inside a massive aggregate. Forecasters quite legitimately can ignore these matters, since the nature of the structure is frequently less essential to forecasting success than its persistence into the future. When the underlying structure is stable, collinearity among the predetermined variables will not damage reasonably accurate prediction and at times negative covariances among the errors can compress the error variance of an aggregate. While appropriate prior restrictions lead to most efficient estimates (Klein [8]), uncertainty about model structure or the validity of identifying restrictions (Liu [11]) will inhibit model formulation and structural estimation but not the straight forecaster. In the face of uncertainties about model structure, the forecaster will choose any and all variables which minimize prediction variance—and quite rightly so; while equally correctly, the research economist primarily interested in structure will be more cautious about accepting variance minimizing formulations of dubious theoretical worth. In short, the a priori beliefs of structural estimation often entail larger error variances than a nonstructural, forecasting approach.

Furthermore, the Brookings-S.S.R.C. model is sensitive to dynamic specification, despite strong initial emphasis on the short-run lag structure. While dynamic theory has existed for quite some time, serious efforts to measure short-run dynamic economic reactions are really only a matter of the past ten years, at most. Again, forecasters can choose to avoid these difficulties with impunity by forecasting over annual intervals instead of facing the more demanding task of predicting a quarterly time path. In the short run, at least, it seems that the price of efforts to achieve greater structural realism could be larger error variances, although in the long run improved structural knowledge and data will most certainly improve forecasting efficiency.

How about the next group on our list: the policy-makers? It is a fact of life (or might turn out to be so on closer examination than I

have given the matter) that at least a dozen business corporations use more sophisticated econometric models than U.S. government economic policy centers such as the Bureau of the Budget, the Council of Economic Advisers, and the Treasury. The reasons why are readily enumerated.

Much government economic policy energy goes into forecasting large national income aggregates. As indicated above, this can be done quite effectively with small-scale, casually structured models. Besides, a few quick telephone calls will reveal the latest ten or fifteen opinions about next year's GNP. At the other extreme, much economic policy is concerned with exceedingly fine-grained problems. For example, most comprehensive econometric models are too coarse to indicate the effects of small changes in the parity price of rye or corn. Legislation typically is concerned with highly specific and detailed content, for which global models, however intricate, are not yet appropriate, although grafting finely detailed equations to macro models is one significant development which will be mentioned later.[2] Another reason may be ideological: economic models, which smack of planning, could perhaps raise congressional or other politically influential hackles. The reasoning seems to be that policy whose detailed implications are unclear is preferable to the same policy whose implications are better known: "Speak no evil, see no evil, compute no evil" appears to be an element of Washington economic thinking on these matters.

But there is much to be grateful for. If I may paraphrase one of the founding fathers: "Econometrics is still in its infancy. We should wait another twenty or thirty years before making any significant claims or we shall be discredited." The day is still far off, as it should be, when the President of the United States will request economic guidance from a computer without human mediation, although there have been times when better advice might have been elicited from our transistorized robots. At the same time, one might expect that governmental economic expertise will rely more on econometric capability for analyzing alternative policy implications instead of current primary dependence on the expert back-of-the-envelope calculation.

It is the third group on the list—economists interested in economic structure—who have the most to gain in the near future from large-scale econometric models. Even so, it is to be doubted that dramatic new breakthroughs are just around the corner. At the beginning of the 1960's, it can be asserted that econometric model building had reached a plateau. Several models had yielded various insights about macroeco-

[2] Karl Fox in an unpublished paper has broken ground on the operational requirements for a highly detailed regional agricultural econometric model suitable for policy purposes, which could be grafted onto a macro model of the sort envisaged here.

nomic interdependence. These implications have been effectively summarized and presented by Goldberger [3] and Klein [7], who surveyed the "state of the arts" at about the same time. In brief, the most outstanding shortcoming is that the models were dominated by consumer demand relations to the exclusion of at least several important sectors. Production was treated in a cursory manner while monetary relations and price-wage behavior were thrust into the background. Many equations were so aggregated that any chance of portraying genuine underlying behavior seemed hopeless. While no miracles are about to spring full-blown from the brow of this new model, these earlier binding restrictions have to some extent been weakened by substantial disaggregation so that a more realistic system now exists.

At the same time that the level of aggregation was diminished, more production and price-wage behavior has been made explicit. Consumer final demand aggregation, except for housing, remains at levels similar to those of some previous models. The expanded scope of investment behavior represents a major advance in realism, since industry demand for investment is directly geared to industry experience. More detailed inventory equations are also tied to industry variables. As a naïve philosopher, I use the word realism in this context without apology because I believe that meaningful economic structures only exist in quite fine detail. While fully aware that appropriate levels of aggregation depend upon the objectives of an investigation as well as equation properties, I believe that large aggregates across time, space, and equations, typifying most previous models, are unlikely to reveal further interesting insights about structure. Increased disaggregation is a necessary but, alas, not a sufficient condition for improved understanding of the economy.

In this connection, Marc Nerlove's view of econometric models as economic history deserves mention [13]. Estimated equations, in his view, have coefficients and a hypothesized structure which provide a convenient summary of events over a historical interval. Thus econometric models should be viewed much as raw time series, simple averages, national income, and product and other descriptive statistics that are used to describe economies. The interesting substance of this position is the increased component of theory compared with more conventional versions of economic history—a methodological point well made several years ago by Meyer and Conrad [12]. There seems little doubt that this form of history represents an advance beyond less structured approaches to the past. History in this format, of course, reflects current fashions in economic theory as much as it does genuine systematic modes of behavior. This perspective is implicitly less optimistic about the potentialities of econometrics than the belief that

meaningful structures can be estimated, but it could turn out to be the more realistic.

As a research device, the Brookings-S.S.R.C. model can provide essential support for the quantitative study of various subsectors. The monetary sector will serve as an illustration. The core model now has a condensed six-sector monetary set of equations, since for the next year or two it is our intention to explore the short-run dynamics of the entire system, which does not seem to rely critically upon a detailed monetary structure. Yet the opportunity exists for monetary economists with econometric interests to weld a more complex structure of monetary equations onto a reasonably complete and tested set of relations. The existence and accessibility of the model will avoid a detour from their central interest in order to construct such a system, which must inevitably be done with some haste and still absorb much time and energy. In short, the possibility of expanding Adam Smith's pin factory to the fabrication of economic knowledge becomes a more genuine possibility; as in the manufacture of pinheads, two heads are better than one (*ceteris paribus!*). Since the Brookings-S.S.R.C. model is not a monolithic fixed construction, it is possible to combine a particular monetary structure with different investment equations or other sectors with which the monetary sector is thought to interact significantly.

Through the efforts of Holt and others [5], we now have computer programs that will enable simulation and other (e.g., forecasting) solutions of large systems. This new capability places us in an intriguing situation—one of promise, but also some uncertainty. On the positive side, we shall be able to manipulate large complex dynamic systems in order to observe their properties. Much remains to be learned even though some understanding of such systems can be found in the theory of differential equations and the literature of control engineering. These techniques have been translated into an economic context by Phillips [14], Holt [4], Theil [20], and Tustin [22]. The blithe enthusiasm of simulation devotees has yet to be sustained by much visible evidence. The main benefits of simulation lie in either: (1) exploring the properties of systems whose specific operating characteristics are already well known but for which analytical solutions are difficult or impossible; (2) sometimes revealing incongruous systems behavior when plausible (equation-by-equation) specification proves incompatible with other parts of a larger system.

Unlike some relatively simple, readily observable physical systems, which have been successfully simulated, seldom are economic systems so well specified that great reliance can be put on the first, "solution" category. Problems related to the second category, verification

and testing, are of uncertain quality and power, since scientific or even sensible intuitive test criteria are not developed to a widely acceptable extent. A positive merit of simulating large-scale econometric models is that various alternative specifications of a given subsystem can be readily compared, provided the remainder of the system is well specified. For instance, much controversy still exists on the correct way to specify investment behavior. It should be possible to gain further insight into the plausibility of different investment formulations by simulating each under different economic conditions.

Other limitations deserve some discussion as well, but this discussion will be short, since the economics profession is strongly aware of them. First, the quality of most available statistics is weak. We simply do not possess internally consistent bodies of data with numerous strategic cross-classifications of variables by industry, let alone region or demographic categories. In many areas of research, computing capability and theory of estimation and behavior have clearly outstripped the ability of our statistical agencies to produce pertinent data for testing and estimation, even though matters are constantly improving.

Second, there quite likely exist inherent limitations of a random nature, beyond which we cannot penetrate, although we are still far from reaching those ultimate limits in most instances. Many economic activities are fundamentally evolutionary and irreversible; their "laws" of behavior are never likely to be fully understood. Even when fully understood, the intrinsic random component of some processes may be large. Inherent random variation in investment behavior, for instance, is likely to remain substantial, even though averaging can improve the prediction of average behavior, which is the goal in most problems of quantitative economic analysis. It is also true that index number problems of aggregation and quality change cannot be easily managed or totally eliminated.

Third, it is trite but true that the limits of theory are great: much work remains to be done. If we experimentally attacked economic entities with the terrible ferocity that physicists exhibit toward atomic nuclei, it would be considered justifiable grounds for complaint on behalf of the injured parties, because we are not yet all that certain about what to do with the pieces. As long as economics remains fundamentally a nonexperimental science, the unknowable will bulk large.

Fourth and finally in conclusion, the dangers of substituting too much computer brawn for analysis should not be minimized. We may become overmechanized dinosaurs who ultimately will become extinct in an impenetrable underbrush of magnetic tape. But do not despair: there abound many statesmen to warn us lemmings marching to the computer room that we will drown in a sea of printout.

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DISCUSSION

MARC NERLOVE: On a previous occasion, before the advent of the Brookings-S.S.R.C. model, I was asked to comment on the state of the art of econometric model building. At the time I had the temerity to respond with a passage from *Through the Looking Glass.* Having elicited a certain amount of amusement then, I venture now to repeat the quotation:

> "And yet it was a very clever pudding to invent."
> "What did you mean it to be made of?" Alice asked, hoping to cheer him up for the poor Knight seemed quite low spirited about it.
> "It began with blotting paper," the Knight answered with a groan.
> "That wouldn't be very nice. I'm afraid—."
> "Not very nice *alone,*" he interrupted, quite eagerly: "but you've no idea what a difference it makes mixing it with other things—such as gun powder and sealing-wax."

Now that I have tasted the delights of the upwards of 150 equations of the Brookings-S.S.R.C. model, I must hasten to assure you that, regardless of the merit of gun powder and sealing-wax, string and paper clips really do make a substantial difference!

Were the new model a mere multiplication of equations, sectors, and subsectors, it might perhaps be dismissed with such a comment, but it is more—far more—than that. By bringing together the skills and knowledge of more than thirty experts and allowing each to explore intensively a relatively small segment of the overall model-to-be, Klein *et al.* have created a model which is not only very large and complex in comparison with previous models but one which in certain areas exhibits remarkable ingenuity and which most assuredly has great potential utility in a variety of ways. It is a model, too, which raises serious methodological issues and problems of a magnitude which may approach those raised by Tinbergen's pioneering League of Nations study of the United States economy. (As most of you know, it was Tinbergen's work that led, via criticism by Haavelmo and others, to the development of simultaneous equations estimation techniques at the Cowles Commission after the second World War.)

In economics, as in other sciences, we seek to understand the past so as better to predict and control the future. To this end it is necessary to marshal whatever data, statistical or otherwise, and whatever theory, intuition, or hypotheses may be at hand into an internally consistent and manageable whole. In economics, where adequate data are frequently lacking, it is especially important that the structure be such as to allow theory and intuition to play a major role. To the extent that econometric models of the Brookings-S.S.R.C. type or that contemporary economic history (such as that contained in Aaron Gordon's *Business Fluctuations* or Harold Vatter's *The American Economy in the 1950's*) are directed to this goal they are merely somewhat different ways of doing the same thing. From this viewpoint, it would appear that the specter of "structural change" which haunts econometricians is a red herring; that which is so called reflects nothing more than our lack of ability to formulate theories of past development consistent with available data or of

available data to support and confirm relevant theories. While no one would deny the existence of substantial problems of this sort, it is perhaps inaccurate to characterize the view of econometric model building as economic historiography as inherently pessimistic. Perhaps the most significant contribution of econometric model building in general—and of the Brookings-S.S.R.C. endeavor in particular—is that the activity itself makes clear the limitations of both theory and available data and forces us to clear and rigorous thinking about the theoretical framework imposed. Much of which is considered as structural change is, in fact, nothing more than shifts in the relative importance of different sectors for good and sufficient economic reasons. The very size of the Brookings-S.S.R.C. model which permits a very significant increase in disaggregation is, thus, an important step in the exorcism of structural change. I think it is clear, though not without exception, that the high degree of disaggregation of the Brookings-S.S.R.C. model has led on the whole to more economically meaningful relationships than have hitherto been incorporated in macroeconometric models; it remains to be seen, however, whether these relationships will prove more or less stable than the more aggregate incorporated in previous models, for it may be that aggregation is capable of concealing theoretical weakness, data limitations, or inherent randomness. This is a point which Kuh rightly stresses.

The concrete contributions of the Brookings-S.S.R.C. model to economic analysis and econometric methodology are numerous. In the present discussion I should like to call your attention to two areas which I found of special interest but which were not covered in the papers as presented: the Eisner-Jorgenson investment realization and anticipations sector and Fisher's contributions to the development of appropriate estimation procedures for large-scale models.

In the version of the model estimated in June, 1964, investment realizations and plans are explained by four sets of two equations each. The four sets refer to durable and nondurable manufactures, regulated industries, and a residual category. Eisner's investment realization equations attempt to account for the relation between and difference between investment anticipations and their realization. Previously anticipatory data had been used to modify investment forecasts or, at best, as an exogenous variable in the investment equations. Eisner's equations explain why, on the basis of sales and profitability variables and some rather complicated lags, and by how much actual investment differs from plans laid two quarters ago. Jorgenson's explanation of investment intentions in terms of user costs of capital and a complicated distributed lag structure resulting from technical delays between the conception and implementation of investment plans is one of the most ingenious and original pieces of the model. The implications for both the theory of investment behavior and distributed lag analysis of all types are likely to be far-reaching.

In comparison with previous models, the number of truly exogenous variables (variables uncorrelated with current or past stochastic disturbances in the system's equations) is relatively small in the Brookings-S.S.R.C. model. The main exogenous variables appear to be population variables (but not

marriages or labor force variables!), instruments of monetary control, and certain government expenditures. The number of lagged endogenous variables entering the model, however, is very large and exhibits an interesting structure. Fisher has pointed out that the equations of the model may be grouped into sectors arranged in such a way that lagged endogenous variables of one sector appear as explanatory variables only in that sector and in lower sectors. Without going into excessive technical detail, it may be remarked that the great number of "predetermined" variables in the model as a whole preclude estimation by usual simultaneous equations techniques. Instead, Fisher suggests, as I understand it, an instrumental variable approach which makes ingenious use of the "block" structure of the model referred to. This procedure is as follows: All predetermined variables in a sector, exogenous as well as lagged endogenous, are used as instruments in the estimation of the equations of that sector, but in addition sufficient current and lagged exogenous and endogenous variables from "higher" or causally prior sectors according to the block structure of the model, are introduced using *ad hoc* but sensible rules for deciding when to stop. By means of a two-stage least squares procedure certain linear combinations of these are then used together with the predetermined variables of the sector as instrumental variables in the final estimation.

Such an approach has much to recommend it; however, the estimates so obtained will be very sensitive to the dynamic structure of the model, since this determines the block structure. And this it seems to me is a serious catch, for in a quarterly model the stochastic disturbances will surely be serially correlated in perhaps quite complicated ways. Since the estimates of the parameters in any one sector—and indeed the division into causally ordered sectors in the first place—depend crucially on the assumed lag structure, anything which gives an erroneous view of that structure will tend to alter the estimates very substantially. Since the presence of serial correlation (which cannot be properly tested for in relationships involving lagged endogenous variables) does just this, it follows that unless the lag structure is well determined a priori the usefulness of the estimates is moot. A detailed examination of the various equations suggests, moreover, that for the most part the lag structure is generated in a largely empirical fashion.

If it were possible to determine lag structures in large-scale quarterly models in a theoretically acceptable empirical fashion and estimate the other parameters of the model in a manner not falling afoul of the collinearity of the entire set of predetermined variables and do all this with the limited number of observations typically available in economics, then I should say the accomplishment would be at least as great as that of the development of the original simultaneous equations methods. In this respect, at least, if not in others as well, the Brookings-S.S.R.C. model should stand, as does Tinbergen's League of Nations model of the United States economy, a landmark in the development of econometrics.

R. A. GORDON: Although the program does not announce this, it is clear what the planners of this session intended my role to be; namely, to offer a

noneconometrician's reactions to this huge experiment in econometric research. It should be noted, however, that I am not a completely unprejudiced observer. As the first chairman of the S.S.R.C. Committee on Economic Stability, under whose auspices this project was begun and carried to just short of its present stage, I played a small role in helping these model builders to get started in their undertaking.

I should like to emphasize three characteristics of the Brookings-S.S.R.C. model. First is that it is based on quarterly, not annual, data; second is the extent of disaggregation; and third is the fact that a number of variables and relationships that are all too frequently excluded in theoretical and econometric models are here explicitly introduced, again with an unusual degree of disaggregation.

I need not elaborate upon the value of a quarterly model for studying the dynamic characteristics of the American economy, particularly with respect to relatively short-run and cyclical behavior. I should like to emphasize even more the great advance that has been made in the extent of disaggregation. In the early years of model building, I think it is fair to say, many econometricians were overly optimistic as to what could be learned from highly aggregated models. In my opinion, this is still too often true of those who construct theoretical models. Those who have worked on the new model are to be complimented on their attempts to disaggregate inventory and noninventory investment, the very considerable detail with which they have treated the government sector, their detailed treatment of employment-output relations, their intensive consideration of the agricultural sector, and their disaggregative approach to price and wage behavior.

The third feature of the new model to which I should like to call attention is its coverage of sectors and relationships which are all too frequently neglected in aggregative model building. The inclusion of a financial sector is one example; another is the detailed treatment of residential building, including consideration of demographic factors. I have already referred to the separate treatment of agriculture. More notable is the detailed treatment of prices and wages and of employment. Other examples could also be mentioned.

Professor Kuh briefly evaluates the potential usefulness of the model, in something like its present form, for three possible classes of users: those interested in forecasting, those concerned with policy applications, and research workers who seek to gain further understanding of economic behavior. I agree with him that the new model will not immediately be of much help to those concerned chiefly with making aggregative forecasts. He may, however, underestimate the potential usefulness of the model, in all its interrelated parts, to policy-makers (even if the latter have not so far been inclined to make much use of such econometric help as has been available). But I think he is quite right that the model will be of greatest usefulness to a wide range of research workers concerned with many different aspects of economic structure and behavior.

For the time being, I think the Brookings-S.S.R.C. model may well be a case in which the sum of the parts is greater than the whole. The set of equations for each sector represents a piece of independent original research by a

specialist. Thus each segment of the model—whether concerned with inventory investment, foreign trade, aggregate income shares, the demand for cash, or something else—can be the starting point for further research by those with a particular interest in these topics. And at the same time, all of the rest of the model is available to test for indirect effects and for the sources of change in the independent variables that are endogenous to the model. I have no doubt that the Brookings Institution and those directly in charge of the model will be glad to cooperate with other scholars who wish to pursue research along these lines.

I should like now to pass on to a different point, one to which Professor Kuh has briefly alluded. Econometric research presumably represents a partnership between theory stated in mathematical form and the study of actual data with the help of the best available statistical methods. But so far, while dynamic aggregative theorizing has posed important questions and offered a limited number of useful insights, the blunt fact of the matter is that pure theory has not yet advanced us very far in understanding the dynamic behavior of the American and other economies. As a result, econometric research in this area inevitably takes on a strongly pragmatic character. This pragmatic quality is found throughout the model we are discussing. Thus, if it turns out that liquid assets seem to influence consumer spending on services but not on durables, that is the way the regressions are left, whatever theory may say to the contrary. Clearly, an important line of inductive research is thus opened up—in which we can further test and refine our empirical relations while at the same time we seek to reformulate our theory to bring it into closer touch with what has been observed. The twain, of course, will never meet, but research of this sort should help to bring them closer together.

Let me end on a word of caution. The model as a whole has not yet been really tested. In varying degrees, all of the regressions stand in need of further work. In a good many cases, the standard errors associated with regression coefficients are uncomfortably large. Certainly, as further work is done, various of the equations will have to be revamped to some degree. And we can be certain, also, that the mere passage of time will generate a variety of changes in parameters. In this connection, I should also like to cite Marc Nerlove's comment that "econometric model building can be described as a way of writing economic history." It is a quantitative way of writing history and omits much that cannot be readily quantified. One lesson to be learned from history is that "things change"—including the relationships described in an econometric model fitted to a particular economy in a particular time period.

But granted all this, it seems to me that this model represents an important step forward. It is an experiment that had to be tried.

INVITED STUDENT DISSERTATIONS

INCOME AND THE AGGREGATE ALLOCATION OF EFFORT*

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This paper is an appeal for a more careful use of microeconomic insights in studies of the relationship between rates of income and the aggregate allocation of human effort to income acquisition or to leisure. Use of microeconomic models to study essentially macroeconomic relationships is justified by the convenient fact that smaller and more tractable descriptions of behavior embody a rough test of consistency with our own experience—a running exercise of intuitive empiricism. Where there are no significant aggregation problems, this use of micro theory is unexceptionable. But too often, such models ignore important difficulties that arise in moving from the analysis of an isolated individual to that of society. When these complications are suppressed, a simple extrapolation from the individual to society may lead to the use of inadequate data and an inappropriate model in the analysis of pressing macroeconomic questions.

The allocation of effort to income and leisure is a case in point. Whether, as incomes increase, people typically will give more or less of their effort to income-producing activities is a question of quite central importance for fiscal and labor policy, for development planning, and, simply, for understanding growing economies. Yet the answer to this question has been sought in the restricted context of the single idealized consumer of microeconomic analysis.

The traditional formal theory of an isolated individual provides a quite useful point of departure for analyses of the allocation of effort, but it needs significant modification before it can represent the more general response of a typical member of a society. This paper presents the two most important of such modifications: (1) accommodation of a society's ability to change its allocation of time to income acquisition either through changes in the average amount of time worked or through changes in its rate of labor force participation and (2) recognition that differential incomes within a society may produce a demon-

* This paper reports, in part, on a dissertation submitted to the Department of Economics, Stanford University [25]. I received significant help and encouragement on that study from John Gurley, Kenneth Arrow, and, at an early stage, Hendrik Houthakker. For their critical reading of earlier drafts of the present paper, I want to thank Robinson Hollister, John Gurley, and John Power. Its preparation was supported by a grant from the Center for Development Economics, Williams College, and by the Research Center in Economic Growth, Stanford University.

stration effect that would seriously affect the applicability of conclusions based on cross-section observations on the response to changing incomes.

Since these developments of the formal theory do not remove the long-standing theoretical indeterminacy of people's response to higher incomes, their purpose can only be that of guiding empirical study. While the present limitation on space keeps me from giving a complete description, I will include a brief, departing summary of the international cross-section study which this analysis supported [25].

I

The traditional analysis of the allocation of effort is a straightforward variant of commodity demand analysis [19] [8] [23] [17] [6]. Income, as generalized command over economic goods and services, is purchased with time. Leisure is the reservation demand for time. If an individual is a rational price-taker, is free to vary his allocation of effort, derives all income from the expenditure of effort, and if his indifference curves are normally convex and continuous, then he will maximize utility by choosing that combination of income and leisure at which the price of income in terms of effort (the inverse of the wage rate) equals the marginal rate of substitution of income for leisure. But an individual's response to a change in the effort-price of income is indeterminate because both the level of income and the relative price of income and leisure are changed. Income and substitution effects, in this case, pull in opposite directions. Students of the theory of income and leisure can only appeal to empirical study to remove that indeterminacy.

The relationship to be discovered empirically is described by the slope of the price-consumption curve—the curve that shows, on any given preferences, the reallocation of time associated with a changing rate of income.[1] With time—as man's ultimately constrained commodity—on the vertical axis and income on the horizontal axis of an indifference map, the sign of the slope of the price-consumption curve is opposite to that of the individual's labor supply curve. A negatively sloped price-consumption curve (a "normal" labor supply curve) indicates dominance of the substitution effect. With such preferences as these, imposition of a proportional income tax on a previously untaxed individual would increase his consumption of leisure. On the other hand, a positively sloped price-consumption curve (a backward bending labor supply curve) indicates dominance of the income effect—imposition of proportional taxation would increase work effort.

[1] The existence of income earned without the expenditure of effort does not modify the main points made in this paper; hence its consideration is omitted here. However, see [25].

Any extension of this formal microanalysis to more than a single individual invokes, as I shall, the fiction of an average response (in the Marshallian sense of representative).[2] But more fundamental differences exist between the analysis of an isolated individual and that of a society.

II

The first of these differences derives from the fact that societies are made up not only of workers but of those—like children, some women, and the aged—who do not allocate any time to income but, instead, depend for income on transfers from those who do. This recognition of interdependence, however, cannot be made simply by a footnote reference to Samuelson's conditions for household utility functions [21], followed by a return to the formal analysis of an isolated individual. In this case, interdependence means that a society, unlike a single individual, can vary its allocation of effort to income and leisure by changing how much time the "economically active" individuals allocate to income, by changing how many individuals are economically active, allocating any time to income acquisition, or by changing both.

Surprisingly, in previous analyses of the allocation of effort to income and leisure, only Douglas [3] recognized (and then only implicitly) that these two dimensions of the allocation of time enter the society's response to higher incomes. Typically, the distinction between individual and household or social analysis has been slighted in both price [4] [14] and public finance theory [17].

The appropriate measure of a society's allocation of time to income and leisure is easily stated. For an isolated individual, leisure is the residual,

$$L = t_o - t_i. \tag{1}$$

where t_o is the period during which flows are measured and t_i is the time he allocates to income. For a society the same definition is applicable. But in a society with a population P, the total amount of time available is Pt_o per period t_o. The time allocated to the acquisition of income by all members of the society is the sum of all such individual allocations, $\sum_{i=1}^{P} t_i$. The total leisure of the society, then, is the residual, total time available less total time worked,

$$L_s = Pt_o - \sum_{i=1}^{P} t_i. \tag{2}$$

If, out of the population of P, only A economically active individuals

[2] The distribution assumption implicit in the use of an average response in demand analyses bears a heavy weight [9]. Therefore, the distribution of both property income and effort-price have to be considered in a more thorough analysis [25].

allocate any time to income, the P-A values of t_i that equal zero can be eliminated. Then

$$(3) \qquad L_s = Pt_o - \sum_{i=1}^{A} t_i$$
$$= Pt_o - A\left[\sum_{i=1}^{A} t_i\right] \Big/ A$$
$$= Pt_o - At_w$$

where t_w is the average amount of time allocated to income by those A economically active individuals. Finally, dividing both sides of (3) by population, gives average per capita leisure (L_{sc}),

$$(4) \qquad L_{sc} = t_o - \frac{A}{P} t_w.$$

This is a measure of social leisure which is sensitive both (a) to changes in the average amount of time (t_w) allocated to income by those who are economically active—the average work week (or year) —and (b) to changes in the proportion of the society (A/P) which is economically active—the participation rate. Only in the special case where all members of "the society" allocate some time to income activity—as in a labor union [1] [16]—does A equal P. Then (4) properly reduces to an individualistic statement of leisure for the representative individual.[3]

[3] This is a significant improvement over past definitions of social leisure, but it is still inadequate in dealing with three types of problems:

First, it obscures an interesting question of the relationship between the two components of leisure as the effort-price of income varies. A representative man, responding to average values of per capita income and leisure, would have the utility function

(9) $U_r = u(Y_{sc}, L_{sc})$,

where Y_{sc} is per capita income. If all income accrues from effort expenditure, the constraint is

(10) $t_o = L_{sc} + p_e Y_{sc}$.

Since per capita income is total income divided by population, we can substitute the identity

(11) $Y_{sc} = At_w/P_{pe}$

and (10), rearranged, into (9) to yield

(12) $U_r = u\,(At_w/P_{pe}, t_o - At_w/P)$.

For any given effort-price and population, then, this representative individual will be indifferent between all combinations of participation (A) and average allocation of time to income (t_w) that leave their product (At_w) unchanged. Thus it is assumed that a society is insensitive to the form which leisure adjustments take in response to changes in the effort-price of income.

Second, it draws too neat a separation between work and leisure, assuming that nonwork is always leisure. While obviously inaccurate, this would be an empirically important issue only if there were a systematic relationship between the proportion of leisure time which adds to income (and to disutility) and the level or rate of income attained. There is little evidence to suggest that such a relationship exists.

Finally, shorter hours and higher incomes may induce increased participation either, as

Thus, either of two methods of adjusting effort is available to a society and the allocation of effort to income and leisure will not be revealed through one of these components alone. A study like Long's [12] which shows some stability of the participation rate during a significant rise in real per capita income, does not necessarily indicate that there is no systematic relationship between income and the allocation of effort. It measures only the participation component, ignoring changes in the average amount of time allocated to income. Nor, as Scitovsky alleges [23], does a reduction of the average work week with rising real per capita income, by itself, indicate a negative correlation between income and the allocation of effort to income. Together these facts would describe a society that gives less time to income acquisition as incomes increase—a society that has an "inelastic demand for income," in Robbins' terms. But neither one, alone, establishes such a relationship.[4]

III

The second modification of the formal microeconomic theory is required by the possibility of demonstration elements in the consumption of income and leisure. To this point, income has been defined as a generalized flow of purchasing power over economic goods and services. And it has been assumed that that was all it could represent. While this is the orthodox assumption underlying analyses of the allocation of effort, it gives an incomplete view since it ignores the fact that an individual's income may give him something more than simple title to economic goods and services. Income may give him additions to (or subtractions from) satisfaction as a mark of social distinction (or embarrassment) if it is an income that is high (or low) relative to those of other members of his society. Discussions in the general literature by Veblen [24], Pigou [18], Knight [10], and, most recently, Duesenberry [5] have emphasized the importance of an individual's relative income as a determinant of the satisfactions he gets from in-

Long has speculated, because a shorter work week allows wives both to discharge their duties at home and to enter the work force [12] or, as suggested to me by H. S. Houthakker, because, simply through greater division of labor, an increasingly complex society may induce additional participation by offering a greater variety of occupations. These possibilities would give alternative explanations for the observed relationship between income and total work effort only if that relationship were positive. Since all available evidence indicates that it is negative [3] [2] [25], the desire for more leisure with higher incomes must have dominated these contrary effects.

[4] Since this paper was written, Harberger has published an analysis of the differential effects of income and lump sum taxes on the allocation of time [27] that again points up the need to measure work and leisure in the manner described in this section. Harberger (*a*) estimated the elasticity of aggregate labor supply from hours data alone at the same time that he (*b*) pleaded for consideration of: "1. Longer vacations. . . . 2. A higher incidence of early retirement. 3. Less labor force participation of women. . ." [27, p. 50]. All of these variables can be captured in the aggregate measure developed above.

come. Yet relative incomes have not been recognized as a possible influence on the allocation of effort to income or leisure.

Harrod distinguished between "growing rich oligarchically" (through a higher differential income) and "growing rich democratically" (through higher general incomes) [7]. This is a distinction that is central to analyses of the allocation of effort to income acquisition or leisure, for it is the democratic response that is of primary importance to the aggregate questions of economic development, public finance, and the supply of labor.

The influence of relative incomes on studies of the allocation of effort is evident if we define a demonstration effect as an increase in an individual's valuation of income relative to leisure as his effort-price of income is increasingly different from the mean effort-price of his society. All that need be considered, for present purposes, is the increase in the relative valuation of income induced by effort-price differentials. But there might also be a consequent welfare effect that would, on an indifference map, shift the indifference curves laterally, toward the average income at the average effort-price. Thus the demonstration effect would imply that the status component of a lower (higher) differential effort-price not only changes the relative valuation of income and leisure, but also increases (decreases) the total satisfaction derived from any given combination of income and leisure. In the present context, though, it is enough to say that an individual susceptible to a demonstration effect will value income relatively more than leisure at any given effort-price if that effort-price is different from the average prevailing in his society (to accept Duesenberry's initial standard of comparison [5]).

This definition implies, I think correctly, that the status element of a differential effort-price does not affect preferences for income and leisure symmetrically—that it is income rather than leisure through which the additional satisfactions of status and prestige are manifested.[5] But this definition of the demonstration effect breaks with earlier discussions in attributing additional (or reduced) satisfactions to differences in effort-prices (i.e., differences in rates of income) rather than differences in income levels. When, as is usual in such discussions of the status element of income, only the incomes of individuals within a given institutional setting are considered, the distinction between a demonstration effect defined on income levels and one defined on effort-price of income disappears if the amount of time the individual allocates to income acquisition is institutionally determined. In the ex-

[5] Even Veblen's "conspicuous leisure" was a result of, and not a substitute for, high income. "A life of leisure is the readiest and most conclusive evidence of pecuniary strength . . . *provided always that the gentleman of leisure can live in manifest ease and comfort*" (italics mine) [24, p. 43].

treme, the level of income is equal to the ratio of the (fixed) time worked and the effort-price.

This correspondence between effort-price and income level breaks down, however, when we consider those smaller segments of a society where individuals have the freedom to vary their allocation of time [2] or when we accept the fact that demonstration effects may be relevant between households, forcing recognition that the allocation of effort to income acquisition can be varied through variations of hours or participation. These require a somewhat more careful specification of those differentials that give a status element to income since different individual (or family) levels of income may then be due either to simple differences in tastes for income and leisure that would appear even when all individuals faced a uniform effort-price for income or to differences in the effort-prices paid for income by different individuals. It is clearly more in keeping with the sense of the demonstration effect, as it has traditionally been used, not to define it on differences in income levels that are due simply to nonuniform tastes, but, rather, to define it on differences in the effort-prices paid for income.

The straightforward price-consumption curve that describes the response to changing effort-prices in formal micro theory must be modified by demonstration effects when this interdependence is accepted. If the status component of income is an increasing function of differential effort-prices of income, then the representative individual will have two different preference maps—hence two different price-consumption curves—depending on whether he considers his effort-price of income as varying along with the average for his society or, alternatively, he considers his effort-price as varying while the average paid by his society remains constant.

In the first situation there would be no demonstration element affecting his preferences since, by assumption, there is no difference between his effort-price and that of the average to change his relative preferences for income and leisure. These preferences would describe, in Harrod's phrase, the democratic response to changing income—the response to be expected with changes in the general level of income. The price-consumption curve summarizing these preferences would be the same as that of the formal micro theory where demonstration elements are assumed absent, a priori. It is shown as *P-C* of Figure 1.

In the second situation, the representative man considers his effort-price as varying while the average for his society remains constant. Here an entirely different set of preferences would emerge. The status element of differential effort-prices would systematically increase the relative valuation of income as the individual's effort-price differential
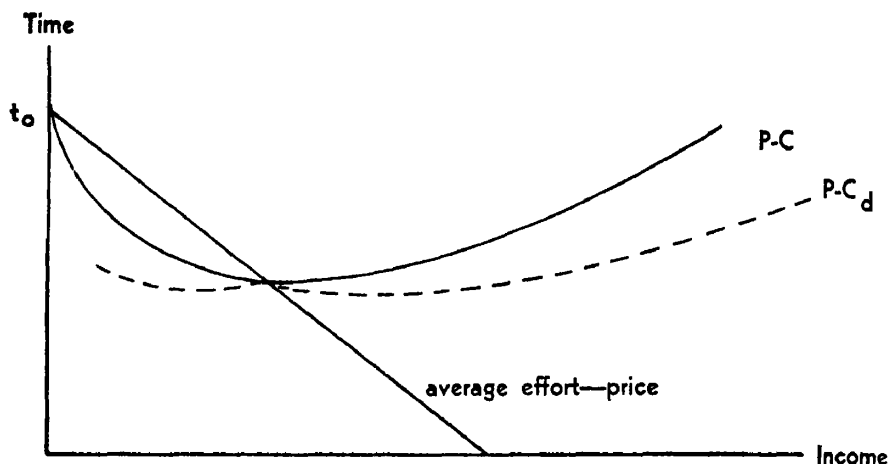
FIGURE 1

increased. On an indifference map, the indifference curves would—compared to those describing the democratic response—shift downward with the higher relative valuation of income as the effort-price differential increased. Such a shift of preferences is summarized in the curve P-C_d of Figure 1. It describes the response to changing individual effort-price of income alone while the average of the society remains constant. This is the oligarchical response.

If the object of an empirical study is to discover the typical response to general changes in the effort-price of income without the intrusion of demonstration effects, the implication of Figure 1 is obvious. When demonstration effects influence the relative valuation of income and leisure, individuals facing either higher or lower than average effort-price for income will distort their allocation of time toward income relative to that which would be optimal in the absence of income differentials.

There is real danger, then, of drawing false inferences if the results from data reflecting demonstration elements are assumed to describe the typical response to change in the general level of income. A cross-section study of a community of individuals susceptible among themselves to a demonstration effect of differential incomes could reveal a positive, negative, or insignificant correlation between the rate of income and the allocation of time to the acquisition of income. Yet with an increase in general rates of income, that same community (given constant distribution of effort-price differentials) could show strongly negative correlation between the rate of income and the allocation of effort to income.

This is a significant modification to the theory of the isolated individual, since it strongly recommends that empirical studies of the allocation of effort use data sufficiently aggregated that the likelihood of a demonstration effect between observations is minimal.[6] To be sure, when differential incomes increase the relative attractiveness of income, the average allocation of time to income observed in the aggregate data will be increased. But so long as there is neither an equally strong demonstration effect between aggregates nor a significant correlation between income and the strength of internal demonstration effects, aggregate data should be clearly superior to single society cross-section data as a source of information on the typical response to changing incomes.

IV

These, of course, are only two of a number of differences between the formal microeconomic theory and the theoretical model necessary to discover the typical social allocation of effort revealed in aggregate data. In addition to the level and distribution of income (including property income) noted earlier, a more detailed specification of the relevant aggregate variables includes taxation and provision of government benefits [26], the structure of income payments, the influence of new products and institutions that change tastes for income and leisure, and the macroeconomic demand for effort—the supply of income.

This larger analytical framework has been used to determine the relationship between income and the allocation of effort to income acquisition or leisure that is revealed in international aggregate cross-section data [25]. The data most appropriate to the theoretical development were those from eighteen countries that reported general hours of work, aggregate participation rates, and general wage rate or per capita income level. It was deemed wise, however, to verify results from these fully aggregated data by using twenty-nine observations of manufacturing sector hours and wages as well as the alternative and more reliably reported participation rates for men aged twenty to sixty-four. Wherever possible, the analysis was based on averages of the variables over an eight-year period in the 1950's.

The striking result from these data was that regression analysis in

[6] Two other advantages of aggregate data are also worth noting. First, aggregate data are more likely to justify the assumption of freedom to vary effort. Work institutions may be assumed to respond to changing general preferences even while they severely constrain the expression of different individual preferences since the group may be able to change work institutions more readily than any individual could [23]. Second, aggregate data from international cross-section or over time embody a far wider range of values of the variables than do data from comparable segments of a society. The international cross-section study mentioned below [25, Chap. vi], for instance, used an income range of 2242 percent in contrast to a range of 87 percent in the Schoenberg and Douglas study [22].

both double-log and reciprocal form yielded a consistent and highly significant negative correlation between income and the allocation of effort to income acquisition. The international aggregate data indicate that, as rates and levels of income rise, societies systematically spend less of their time on income acquisition and more on leisure. Put in Robbins' more precise terms, the effort-price elasticity of the demand for income was estimated from this sample (on the assumption of constant elasticity) to be —.86 to —.93. These elasticities are between zero and minus one at better than a 97.5 percent level of confidence. It should be noted, too, that these estimates are very close to those that appeared in Douglas' analyses of U.S. time series data from single industries [3].

These results, combined with analyses of time series data that are currently under way, will, I hope, remove the indeterminacy surrounding the typical response to changing rates and levels of income.

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AN ADAPTIVE PRODUCTION FUNCTION*

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I. *Introduction*

It is well known that firms typically undertake a variety of activities, such as formal training, industrial engineering, and operations research programs, that are peripheral to their direct production processes. In general these techniques are used to increase the flow of a process's value added and thus to make the process "more efficient" in the eyes of the entrepreneur. Without paying any attention to these activities, management scientists and economists have reported on the observed phenomenon of firm learning or manufacturing progress where the number of direct labor hours needed to produce a unit of product decreases predictably as the firm gains experience in the production of the product. This is equivalent to saying, as before, that the firm's rate of value added increases.

It would seem reasonable to suppose that firm learning should be related to these peripheral activities. If a firm expends resources in selecting and training personnel for a given process, the process should be more efficient at the outset than with a lesser or nonexistent effort on personnel selection. Also a firm using operations research and industrial engineering methods on a given process would expect the process to become more efficient or to exhibit a faster rate of learning than without the use of these techniques.

Economists have overlooked both the phenomenon of firm learning and the techniques by which the firm may influence its rate of learning in constructing production functions of firms. Typically these functions show the maximum rate of output measured in value-added terms that can be obtained from given rates of inputs. The efficiency level of the input factors is assumed to be invariant and their adaptation to one another and the process instantaneous and complete, so that the firm on selecting the rates of input for the various factors will begin and maintain its production at the maximum rate. This, of course, is at variance with the phenomenon of learning and the application of techniques by firms to enhance production rates.

The purpose then of this paper is to present a production function

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of the firm that embraces the concept of firm learning and that is useful in relating the personnel, industrial engineering, and operations research programs carried on by the firm to learning. The function—termed the adaptive production function—will be shown to be capable of influencing a firm's input factors, employee training, and capital expansion decisions. Factors influencing the learning rate or the speed of adaptation of a firm to a given process will also be discussed along with some economic questions involving the relationships among training, learning, and the labor supply function.

II. *A New Type of Production Function*

Traditional learning functions are of the form

$$l_k = ck^{-b} \qquad b > 0 \tag{1}$$

where l_k is the number of direct labor hours required to produce the kth unit of output and c is number of hours required to produce the first unit. If total production time is proportional to direct labor hours, the time t to produce x units would be given by

$$t = \int_0^x ck^{-b}dx = \frac{cx^{1-b}}{1-b} \tag{2}$$

and the rate of output by

$$\frac{dx}{dt} = \frac{x^b}{c} \cdot \tag{3}$$

Since $b > 0$, a shortcoming of the function is that the rate of output can theoretically increase without bound as the firm gains experience in the manufacture of a particular product! Another shortcoming of this function is that it has no theoretical basis but is strictly an empirical phenomenon (see, for example, [2] and [4]). On the other hand, the typical microeconomic production function of the single product firm is usually written

$$P = f(v_1, v_2, v_3, \cdots, v_n), \tag{4}$$

where P is the rate of product output and $v_1, v_2, \cdots, v_n$, the rates of input of the various factors. As noted above, in most economic models of the firm, production is assumed to commence and remain at the maximum rate given by the function.[1]

To combine the concept of learning and the typical microeconomic

[1] An exception to this is found in "aggregate production functions" that purport to depict the output of an entire economy and usually contain a multiplicative time variable. This variable assumes the role of a shift parameter so that the function will exhibit "technical change" or, perhaps, learning over time. Because it multiplies the function, this type of technical change is analogous to a simple scale factor.

production function into an adaptive production function, we begin by assuming that the firm on starting production of a new product chooses an economic production function similar to (4) based upon the engineering constraints of the new process. This function may be one of the common forms of production function, e.g., Cobb-Douglas, CES, etc. However, we do not assume that the firm begins at the maximum rate of output P given by the function but at a lesser rate Q and adapts through learning toward the production rate P. That is, as the firm gains experience, it is able to increase Q, since employees learn their jobs better, employees are interchanged for better utilization of work-force, waste of final product is reduced, etc.

We assume that the experience or stock of knowledge that the firm has on a given process at a specific time can be summarized in the stock of product it has produced up to that time. Then as the firm produces more of a given product, it increases its experience on it and thus is able to come nearer to the desired rate of output P. We then denote the rate of output after q units have been produced by $Q(q)$.[2]

During the learning or adaptation period, we assume that

$$Q(q) < P. \tag{5}$$

Because improvement is assumed between the qth and $(q+1)$st item,

$$Q(q+1) - Q(q) \geq 0. \tag{6}$$

If input rates are fixed, the maximum improvement after q units have been produced is $P-Q(q)$. Then the rate of adaptation (or synonomously the rate of learning) exhibited by the firm or the process will be denoted by μ and defined by

$$\mu = \frac{Q(q+1) - Q(q)}{P - Q(q)}. \tag{7}$$

We substitute the continuous approximation $dQ(q)/dq$ for $Q(q+1) - Q(q)$ in (7), solve the equation for $Q(q)$, and obtain

$$Q(q) = P[1 - e^{-(a+\mu q)}] \tag{8}$$

where a is a constant of integration that is an indication of the initial efficiency of the process for

$$Q(0) = P[1 - e^{-a}]. \tag{9}$$

Thus $Q(0)$ is an increasing function of a.

Note that (8) is a typical production function with an adaptation term grafted on to it. The function, termed the adaptive production

[2] If aggregate output (output including spoilage) is proportional to time as in machine paced operations, $Q(q)$ can be replaced by the more familiar $Q(t)$.

function, adequately describes the observed phenomenon of firm learning for as the firm accumulates experience, i.e., q increases, the production rate approaches the desired rate P.[3] In addition it can easily be related to the programs carried out by the firm that affect learning since μ, the rate of learning, certainly should be influenced by such variables as employee training and experience, the amount of preproduction planning done by the firm, etc. In a following section we shall discuss these variables at length; here we assume that these variables are exogenous of Q, denote them by $y_1, y_2, \cdots, y_m$ so that, formally

$$\mu = g(y_1, y_2, \cdots, y_m). \tag{10}$$

Now, if we assume that g is at least twice differentiable, we can expand (10) in a Taylor Series, drop all terms higher than first order, and obtain

$$\mu = \beta_o + \sum_{i=1}^{m} \beta_i y_i. \tag{11}$$

Substituting the expression for μ given by (11) in the adaptive production function (8) relates the production rate $Q(q)$ to these factors. We can also relate these factors to the initial efficiency a in a similar manner.[4]

With one exception, it can be shown that the profit maximizing firm upon using the adaptive production function will choose factor inputs so that, as in the standard theory, their marginal products are proportional to their prices for all sets of factors. The exception occurs when the quantity used of a particular factor decreases the rate of adaptation μ. In this case, less of that factor will be used at the outset of production by the profit maximizing firm and its usage will be increased as production continues.[5] An obvious example of this is the labor input to a new process, for if the rate of adaptation is dependent upon

[3] It can easily be shown that (8) is an excellent approximation to the output rate given by the traditional learning curve formulation in (3) and in fact, it may be the true underlying function when (3) is fitted. For complete discussion of this, see Levy [6].

[4] To see how these y_i variables can be categorized, measured and used to estimate μ and a, see Levy [7]. In the cases tried thus far, (8) has provided a good fit to the learning that firms achieve, and good estimates of how particular variables may affect the firm's learning rate and initial efficiency on a process have been obtained.

[5] If f_i and f_j are the marginal products of the ith and jth factors respectively, w_i and w_j their respective prices, β_i is the coefficient of $v_i = y_i$ in the expression for μ [11], and v_j does not affect μ, then the profit maximization condition is given by,

$$\frac{(f_i)(1 - e^{-(a+\mu q)}) + \beta_i P e^{-(a+\mu q)}}{w_i} = \frac{(f_j)(1 - e^{-(a+\mu q)})}{w_j}.$$

With decreasing returns to v_i in the expression for P assumed and $\beta_i < 0$, less of v_i will be used than that given by the standard theory. However, as production continues, i.e., q increases, $e^{-(a+\mu q)}$ approaches 0 and we obtain the usual profit maximizing condition:

$$\frac{f_i}{w_i} = \frac{f_j}{w_j}.$$

the joint actions of a group of workers, we should expect it to be a decreasing function of the number of workers in the group.

Finally, we note that if we assume the adaptive production function to be homogeneous in the factor inputs and to exhibit decreasing returns to increases in factors beyond a certain point, then expressions for cost functions showing both rate and stock of output can be derived. These functions will show that increasing the rate of output with stock of output held constant increases costs at an increasing rate and increasing the stock of output with rate of output constant increases costs at a decreasing rate.[6] These results which can be derived from the adaptive function form the major assumptions of Alchian [1] and Hirshleifer [5] in their works on cost functions that exhibit learning, and thus they provide a more fundamental theory for their assumptions.

III. *Types of Firm Learning*

In the last section we postulated some factors denoted by $y_1, y_2, \cdots, y_m$ that were supposed to influence both the firm's rate of learning (μ) and initial efficiency (a) on a new process. To identify these factors we divide firm learning into three classes which we shall call planned or induced learning, random or exogenous learning, and autonomous learning.

Planned or induced learning results from the firm's allocating resources to activities that are designed to increase either a process's initial efficiency, its rate of output, or both. Before embarking upon a new process, a firm may build models or prototypes designed to identify difficulties that may occur during actual production. In doing this, the firm hopes to anticipate these difficulties and either to eliminate them or handle them quickly when they occur. The firm may expend resources for selecting, testing, and training the personnel to be used in the process, writing exact raw material specifications, and doing special tooling for the production run. These techniques carried on prior to production are called preplanning and are used to improve the process's initial efficiency. We should also expect that the less the firm preplanned, the more opportunity there would be for learning during the production run. Thus, we suggest that the amount of preplanning done by the firm is inversely related to its rate of adaptation and enhances its initial efficiency.

Planned learning also occurs during the production run when the firm undertakes programs consisting of time and motion studies, the redesign of raw material specifications, the shuffling of work crews for more efficiency, etc. These activities, designed to speed the flow of product through the plant, are generally grouped under the title

[6] For a deviation of the cost function and their properties, see Levy [7].

"industrial engineering." We assert the greater the amount of industrial engineering done on a specific process, the greater the rate of learning exhibited by the process.

Some improvements in a particular process occur when the firm acquires information unexpectedly from its environment. Suppliers, competitors, customers, and government and trade publications often provide information that can speed production. Firms will take advantage of such knowledge when it occurs, but in general they cannot plan for all aspects of this type of improvement. For this reason we refer to this type of learning as random or exogenous learning.

Employees usually become more adept at performing their assigned tasks as they gain experience in a new process. On man paced jobs, workers learn from experience as past performance and mistakes provide hints directed toward increasing current output. In machine paced operations, the operators learn to correct run troubles more rapidly as the process becomes more familiar to them. Working in groups, employees become more accustomed to each other's actions and responses, and thus the group's rate of output increases. We call the improvement due to this on-the-job training or learning of workers "autonomous learning."

Determinants of autonomous learning may include the employee's general experience, specific experience on similar processes, sex, age, education, etc., when the process consists of jobs assigned to single workers or a group of workers in which there is a single key employee. In other group work where the output depends upon joint actions of all the people in the group, a weighted index of each person's attributes may be a possible way of measuring the effect of a given attribute on the group's autonomous learning.

A further development along these lines might be used to distinguish between autonomous and planned learning. However, this distinction becomes quite fine when we consider that the firm may use incentive schemes, formal training programs, etc., to influence autonomous learning. However, the effects of a training program should enhance the employee's initial efficiency and will generally reduce his rate of learning (or at least not affect it).[7] Then the effect is in the same direction as posited for preplanned learning, and so we need not be overly concerned with the practical infeasibility of forming all the distinctions our model implies.

When the adaptive production function has been fitted with actual data, the above hypotheses about the direction of the effects of the types of activities carried out by firms on learning have been shown to be true in the situations studied. There should be little argument about the effects of industrial engineering and operations research techniques on

[7] For a discussion of the effects of pretask training on subsequent performance, see [11].

learning, for the literature in these fields is replete with numerous examples of how firms achieve cost savings[8] with these techniques. Significant variables in autonomous learning have been found to be the size of work groups, workers' sex, experience, and formal training for given jobs.[9] Moreover, in actual situations, it has been shown that the adaptive function is useful as a rough guide in budgeting and evaluating formal training for firms.[10]

IV. *Some Implications of the Adaptive Function*

One of the primary reasons given for the relative success of one of the country's largest manufacturers of computers is that it has the best "software" to accompany its equipment. One interpretation of this is that the program languages and routines that are accessible to this particular company's computers enable its purchasers to learn to use its equipment faster than that of its competitors. Thus even though all computers available to a firm may theoretically have exactly the same capabilities, the firm will show a preference for that computer which is easiest to learn to use.

However, if we look at the traditional economic theory which assumes instantaneous adaptation to new equipment, the firm should be indifferent among computers that sell at the same price and have the same capacity. Here, the demand by a firm for a specific type of capital, given that all other factors are fixed, is a function of the firm's product price and the prices and marginal products of both the particular and competitive equipment. If the prices and marginal products are the same for all manufacturers, then theoretically the firm should not care which equipment it purchases. On the other hand, if we use the adaptive production function to derive a firm's demand for a given piece of equipment, the equipment's adaptation parameters μ and a and those of competing equipment would also be included with the above listed variables in the demand function. Formally, assuming all other factors fixed, we can write

$$D_1 = h(p, f_1, r_1, \mu_1, a_1 \mid f_2, r_2, \mu_2, a_2). \tag{12}$$

Then a firm's demand for a particular piece of equipment (which we call type 1) is a function h of the firm's product price p, the marginal product f_1 of Type 1 equipment assuming complete adaptation, the

[8] We have defined learning as increases in the rate of production. However, if output is measured in value added and if inputs are constant to a process, then increases in the rate of production are synonomous with reductions over time in the cost of producing a given volume of product. This cost decrease results from learning which reduces spoilage, produces better scheduling, etc.

[9] Levy [6] contains a complete discussion of the effects of the variables discussed in this section in a variety of actual situations.

[10] Levy [7].

price r_1 and adaptation parameters μ_1 and a_1 of Type 1, and the marginal product, price, and adaptation parameters of competing equipment, Type 2.

We note that

$$\frac{\partial D_1}{\partial r_1} < 0; \qquad \frac{\partial D_1}{\partial \mu_1} > 0; \qquad \frac{\partial D_1}{\partial a_1} > 0 \tag{13}$$

and hence

$$\frac{\partial r_1}{\partial \mu_1} > 0 \quad \text{and} \quad \frac{\partial r_1}{\partial a_1} > 0. \tag{14}$$

Thus in our computer example, if we assume that $r_1 = r_2, f_1 = f_2$ and that either $\mu_1 > \mu_2$ or $a_1 > a_2$, we would expect the firm to show a preference for the computer with the software (Type 1) that enhances learning. In addition, from (14) we also see that because of the faster adaptation to Type 1 equipment, the firm would be willing to pay more for it than competitive computers. Finally, we note that any capital manufacturer's expenditure on improvements that make its equipment easier to learn to use will enable it to charge a higher price for the equipment without any loss of sales. This is in contrast to the standard economic theory conclusion that a rise in price of its product (a leftward shift of its supply curve) results in less quantity demanded if its demand curve is negatively sloped. Here, because of the change in adaptation of the equipment, the demand curve facing the equipment supplier also shifts and may shift enough to compensate for the supplier's rise in price.

Moreover, if we consider both equipment replacement and capacity expansion decisions of firms and embed the adaptive production function in this type of decision model, it is simple to show that adaptation plays an important role in determining both the type and timing of capital purchases by firms. In addition, when such models have been used with actual data, the costs associated with adaptation have been shown to be quite significant.[11]

The adaptive production function is also more useful than the standard theory in explaining the relationships among labor training, wage rates, and employment by the firm. In the usual theory, the perfectly competitive firm will employ units of labor[12] until the value of labor's marginal product is equal to the wage rate given by the market. When workers are given training external to the firm, as, for example, by the government, and are willing to work for the same wage rate as before, the labor supply curve to the industry will shift downward and to the

[11] For a complete decision model of this type patterned after Smith [10] and Manne [8] and fitted with actual data, see Levy [7].

[12] Labor units here are equivalent to the notion of "efficiency units" described by Robinson [9]. Thus one labor unit may refer to more or less than one worker.

right. Hence more labor units will be employed at a reduced wage per unit to the industry's firms.

Using the adaptive production function the individual firm's demand for labor is obtained by solving

$$p = \frac{w}{f_L(1 - e^{-(a+\mu q)})}, \tag{15}$$

where p is the product price, w is the wage rate, and f_L is labor's marginal product. If the firm continually produces the same product, the adaptation term approaches unity, and the demand for labor reduces to an expression equivalent to the standard theory. However, if the firms are job shops or firms where product runs have a limited duration,[13] the average rates of adaptation and the initial efficiencies of workers will influence the firm's demand for labor. From (15), we note that in such cases we would expect less employment by the firm than that given by the usual analysis. Moreover, when training is added to the analysis, it will also affect the firm's demand curve in addition to the industry labor supply curve. For if workers receive training that increases their efficiency, assuming complete adaptation, the training should enhance either or both their rates of learning and initial efficiencies. This will cause a rightward shift of the individual firm's demand curve and thus a similar shift in the industry demand curve. Compared to the standard case, more labor units will be employed at a higher wage as long as the labor supply curve is upward sloping. In fact, the newly trained workers might be able to increase their wage rate without damaging employment as much as predicted by the usual theory.

The issue of training is quite timely. Faced with widely spread structural unemployment, the federal government has enacted the Manpower Development and Training Act (MDTA) which directs the government to undertake training of the unemployed. An important question based on this Act is where and by whom should this training be given. As we shall see, the adaptive function sheds light on this question.

Most of the training of workers carried on thus far under MDTA has been either at private or state supported vocational schools. The worker on completion of his course of study is then helped by the U.S. Employment Service to obtain a job in which he can apply his new trade. An obvious alternative to this is the government's subsidizing of firms unwilling to train on their own to hire and train unskilled work-

[13] These are perhaps the more commonly occurring types of business. Some obvious examples are firms like automobile or aircraft manufacturers where model changes are the rule, garment and needle trades, printing and packaging shops, machine shops and foundries, etc.

ers. In this manner the workers could simultaneously be trained and guaranteed a job.[14] Which method is better as far as its effect on employment and wage rates?

To answer this question, we first cite Becker's distinction [3] between "firm specific" and general training. The former refers to training that raises a worker's marginal product only in a given firm while the latter is defined as training that enhances the worker's marginal product in all firms of a given industry. We next note that under the current federal legislation, workers are given general training for jobs that are potentially available while subsidization of in-plant training implies that workers will receive both general and firm specific training.

To motivate our discussion of which is the better method, we assume that there are no economies and diseconomies of scale when the government gives general training; that is, the cost per worker of achieving a certain average level efficiency of a labor group is independent of the size of the group. We further assume that the government offers the following plan to the firms in a given industry. Either the government will undertake the general training of potential workers as is now being done or it will subsidize individual firms to do their own training. Either training method would of course shift the labor supply curve to the industry. However, it would seem reasonable to suppose that by letting the firm preselect its workers and give them training on its own equipment, the combination of general and firm specific training would cause the labor supply curve to shift at least as far as any shift in the curve caused by the alternative general training alone. Moreover, we should expect that under firm specific training, the firm's demand curve with adaptation should shift further than under general training for at a minimum, specific training should enhance the worker's initial efficiency more than general training. Thus, it is easily seen that specific training given by firms will cause more employment of labor units at a higher wage per unit than general training. Further, if this training is selectively given to the more unskilled workers, employment should increase. Thus we conclude that the government would be well advised to offer the above option to firms.

V. *Summary*

We have introduced an adaptive production function which is useful in explaining the observed phenomenon of firm learning and the reasons firms carry on plant activities peripheral to direct production. We have discussed how firms learn and why the adaptive function has different implications from standard economic theory for firms' decisions on

[14] This in-plant subsidizing of worker training is in fact currently being done in newly built plants by some states, notably South Carolina and Massachusetts, as an inducement for firms to move their operations into the state.

capital purchases and the hiring and training of workers. Thus, hopefully, the adaptive production function furnishes a step in the direction of making economics more useful for adjusting and describing the actual behavior of economic units.

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TECHNICAL CHANGE, CAPITAL LONGEVITY, AND ECONOMIC GROWTH*

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I. *Introduction*

The specification of a criterion which may be used to determine the optimum longevity of capital in an economy has been the subject of some controversy in the literature. The contributions to the solution of this problem fall into two distinct categories. One approach to the problem of determining the optimal longevity of capital emphasizes microeconomic criteria based on profit maximization or cost minimization [1] [2] [12] [14]. A second approach emphasizes macroeconomic criteria based on the impact of changes in the longevity of capital on the level and rate of growth of aggregate consumption and output [3] [4] [8] [9]. To the extent that economic policy is oriented toward the prevention or elimination of technological obsolescence, it is important to know the relationship between these two criteria.[1] However, these two approaches to the problem of determining an optimal longevity of capital have been developed more or less independently of one another with no consideration having been given to the possibility that these two approaches might lead to identical results.

It is the purpose of this paper to determine within the context of a neoclassical growth model the conditions which must be satisfied in order for the micro- and macroeconomic criteria to lead to similar results. In the next section a macroeconomic growth model is specified and the equilibrium properties of this model are described. In Sections III and IV, respectively, the macroeconomic and microeconomic capital replacement criteria are explicitly derived. The implications for the life of capital of each of the two criteria are compared in the final section and the conditions which must be satisfied for their equivalence are developed.

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[1] A related point which will become more apparent below is that it is important to know the relationship among the level and rate of growth of aggregate consumption and output, the longevity of capital, and the share of investment in output. Policies oriented toward the prevention or elimination of technological obsolescence may well have an impact on the share of investment in output and hence on the level and rate of growth of output. Conversely, policies oriented toward increasing the share of investment in output may well affect the longevity of capital.

II. *A Neoclassical Macroeconomic Growth Model*

In order to develop the interrelationships among the rate of technical progress, the life of capital, the share of investment in output, and the level and rate of growth of aggregate consumption and output, a well-known macroeconomic growth model is introduced in this section [7] [8] [9] [13]. It is assumed that two factors of production—capital and labor—cooperate to produce a single homogeneous output which may be used equally well for either consumption or capital accumulation. In addition the model is characterized by the following properties.

a) Gross investment at time t is a constant fraction of output at time t and is assumed to embody the latest known production techniques at the time of its construction. In addition, capital is assumed to possess infinite durability and the only factor governing its removal from the production structure is obsolescence.

b) The supply of labor is given by an autonomous pattern of exponential growth and is assumed to be continuously fully employed.

c) The capital-labor substitution possibilities at the time capital is constructed are given by a vintage-type Cobb-Douglas production function. After capital has been put into operation, fixed coefficients of production prevail and it is possible to alter the labor-capital ratio only by scrapping old capital and constructing new capital.

Assumption (*c*) forms the basis of the synthesis of short-run fixed coefficients of production and long-run substitutability of the factors of production [6]. The output obtainable at time t from vintage v capital, $Q_v(t)$, is assumed to be given by

$$Q_v(t) = Ae^{\mu t+\lambda v}K_v(t)^a L_v(t)^b \qquad a + b = 1 \tag{2.1}$$

where $K_v(t)$ is the stock of capital constructed at time v and still in existence at time t, $L_v(t)$ is the labor operating vintage v capital at time t, μ is the rate of organizational technical progress, λ is the rate of embodied technical progress, a and b are, respectively, the elasticities of output with respect to capital and labor input, and A is a scale factor. At any instant t in time the possibilities for substituting capital for labor are given by (2.1) with $v=t$. However, after capital has been constructed the capital-labor ratio is assumed to be fixed.

The equilibrium growth path of output to which these assumptions give rise may be developed in the following way. Suppose that output is expanding at a constant relative rate equal to β. Since gross investment at time t is a constant fraction of gross output at time t, gross investment must also be expanding at the relative rate β. Since capital is not subject to physical deterioration, capital constructed at time v and still in existence at time t is equal to the gross investment at time v so that

$$K_v(t) = I(v) = Ie^{\beta v} \tag{2.2}$$

where $I(v)$ is gross investment at time v and I is the rate of gross investment at time zero. If the age of the oldest capital in use is θ, then the total stock of physical productive capital in use at time t is given by

$$K(t) = \int_{t-\theta}^{t} K_v(t)dv = Ig(\beta, \theta)^{-1}e^{\beta t}. \tag{2.3}$$

In equation (2.3) the expression $g(\beta, \theta)$ is inserted as a shorthand expression for $\beta/(1-e^{-\beta\theta})$.

According to assumption (*b*), the labor force is growing exponentially and may be written as

$$L(t) = Le^{nt} \tag{2.4}$$

where $L(t)$ is the supply of labor at time t, L is the labor supply at time zero, and n is the relative rate of growth of the labor force. Letting $L_v(t)$ denote the labor which is working with vintage v capital at time t, the total labor force is also equal to

$$L(t) = \int_{t-\theta}^{t} L_v(t)dv. \tag{2.4'}$$

Differentiating both (2.4) and (2.4′) with respect to t and equating the results gives the equation

$$L_t(t) = nLe^{nt} + L_{t-\theta}(t). \tag{2.5}$$

This equation states that the labor available at time t to work with new capital is equal to the net growth in the labor force plus the labor released from capital which is retired at time t. This result holds because of the assumption that the labor-capital ratio is fixed after capital is constructed. It is implicitly assumed that if any of the capital of a particular vintage is scrapped, then all of it will be scrapped. The equilibrium solution of (2.5) for the labor which operates vintage v capital is[2]

$$L_v(t) = Lg(n, \theta)e^{nv}. \tag{2.6}$$

Equations (2.1), (2.2), and (2.6) may be combined with the following two equations to obtain an expression for total output at any given point in time in terms of the parameters of the model.

$$I(v) = sQ(v) \tag{2.7}$$

$$Q(t) = \int_{t-\theta}^{t} Q_v(t)dv \tag{2.8}$$

[2] This equilibrium solution of (2.5) is obtained by noting that $L_{t-\theta}(t) = L_{t-\theta}(t-\theta)$. Making this substitution in (2.5), the resulting difference equation is of the form $l(t) - l(t-\theta) = nLe^{nt}$ with the equilibrium solution given in (2.6).

Equation (2.7) states that the constant fraction s of output is devoted to investment at each point in time and (2.8) states that the total output at time t is obtained by integrating over the output of all vintages of capital between t and $t-\theta$. Using (2.1), (2.2), (2.6), (2.7), and (2.8) the equilibrium growth path in terms of the parameters is

$$Q(t) = s^{a/b}A^{1/b}Lg(n, \theta)g(\sigma, \theta)^{-1/b}e^{\beta t} \tag{2.9}$$

where $\sigma=\lambda+a\beta+bn$.

The equilibrium growth path of this model possesses several interesting properties. The equilibrium rate of growth of aggregate output, $\beta=n+(\mu+\lambda)/b$, is independent of both the longevity of capital and the share of output devoted to gross investment. The growth rate is also invariant with respect to changes in the mix of technological progress. Regardless of whether technological change is of the embodied type, entirely of the organizational type, or some mixture of the two, the equilibrium rate of growth of output will be the same provided the level of technical progress (i.e., the sum of the two components) is the same in each case. On the other hand, the level of the equilibrium growth path depends critically on the investment ratio, the longevity of capital, and the mix of technological progress. These properties are such that this model provides a convenient framework within which to compare the macroeconomic capital replacement rule derived in the next section with the economic life of capital as determined by profit maximization.[8]

III. *Capital Accumulation and Replacement*

It has been shown above that the equilibrium rate of growth of output is independent of both the investment ratio and the longevity of capital. However, the equilibrium level of output does depend on both of these variables. These properties of the equilibrium growth path of output are utilized in this section to derive a capital replacement rule which must be satisfied in order for the level of the equilibrium consumption path to be a maximum. The longevity of capital which satisfies this rule will be referred to alternatively as the optimal longevity of capital or the optimal capital replacement period. In view of the similarity between the capital replacement rule and the "Golden Rule of Accumulation" derived by Phelps [11] for a somewhat simpler growth model, it is of interest to compare the two rules within the context of the model of the preceding section.

Consider first the determination of an optimal investment-output ratio or what has come to be called the "Golden Rule of Accumulation."

[8] One of the more important reasons why this model is particularly well suited to this problem is that different sets of values for the longevity of capital and the investment-output ratio do not give rise to intersecting equilibrium growth paths of aggregate output and consumption. For a discussion of this and related points, see [5] and the references there cited.

Since consumption is equal to gross output less gross investment at each point in time, the maximization of the equilibrium growth path of consumption is equivalent to the maximization of

$$C(t) = (1 - s)Q(t) \tag{3.1}$$

where $Q(t)$ is given by (2.9). According to (2.9), the equilibrium level of output will be larger the larger is the investment ratio. However, with a larger investment ratio there is a smaller fraction of output available for consumption. Maximizing $C(t)$ with respect to s leads to

$$\bar{s} = a. \tag{3.2}$$

This marginal condition states that the level of the equilibrium time path of consumption in this simple growth model will be maximized provided that the investment ratio is equal to the elasticity of output with respect to the capital stock. It is of interest to note that the golden rule of capital accumulation in this model is independent of the longevity of capital. Regardless of the prevailing longevity of capital, the golden rule states that the percentage of gross output which should be devoted to gross investment in order to maximize the equilibrium level of consumption is exactly the capital elasticity of output.

Suppose now that the investment ratio is given and consider the problem of determining the longevity of capital. With a given investment ratio, it is a matter of indifference as to whether the equilibrium level of consumption or the equilibrium level of output is maximized. In either case the optimal longevity of capital will be the same. For the purposes of exposition it is somewhat simpler to consider the problem of maximizing the equilibrium growth path of output. Since total gross output at time t is obtained by integrating over the output of all vintages of capital between t and $t-\theta$, the change in output resulting from a change in the longevity of capital may be obtained by differentiating equation (2.8) with respect to θ.

$$\partial Q(t)/\partial\theta = Q_{t-\theta}(t) + \int_{t-\theta}^{t} [\partial Q_v(t)/\partial\theta]dv \tag{3.3}$$

This expression for the rate of change of gross output with respect to a change in the longevity of capital expresses the fact that an increase in the longevity of capital increases output by adding an older vintage of capital to the production structure as represented by the first term on the right-hand side of (3.3). At the same time, as the second term on the right-hand side of (3.3) indicates, an increase in the longevity of capital decreases output because the output of all vintages of capital between t and $t-\theta$ is decreased. The reason for this is that as the capital of vintage $t-\theta$ is added to the production structure, the labor required to

operate this vintage must be drawn from the capital of newer vintages. This reduces the labor left to work with newer vintage capital and hence decreases the output of the newer vintage capital. Simplification of (3.3) with the help of (2.1) gives

$$\frac{1}{Q(t)} \frac{\partial Q(t)}{\partial \theta} = g(\sigma, \theta)e^{-\sigma\theta} - bg(n, \theta)e^{-n\theta}. \tag{3.3'}$$

When this equation is set equal to zero, the optimal capital replacement period, denoted by $\bar{\theta}$, is obtained by solving

$$g(\sigma, \bar{\theta})e^{-\sigma\bar{\theta}} - bg(n, \bar{\theta})e^{-n\bar{\theta}} = 0 \tag{3.4}$$

for $\bar{\theta}$.

In order to gain a further understanding of the optimal capital replacement period and the marginal condition by which it is determined, it is instructive to consider the way in which changes in the parameters of the model affect the optimal replacement period. This could be accomplished by subjecting (3.4) to implicit differentiation. However, the same end can be achieved by working out a few numerical examples with plausible values substituted for the parameters. The results of these numerical examples are set out in Table 1. Various values of the rate of organizational technical progress (μ), the rate of embodied technical progress (λ), the rate of growth of the labor force (n), and the labor elasticity of output (b) have been assumed. From these values the equilibrium rate of growth of output (β) and the optimal capital replacement period ($\bar{\theta}$) have been derived. In addition, the average age of the capital stock ($m(\bar{\theta})$) and the equilibrium capital-output ratio (K/Q) corresponding to both the optimal capital replacement period and the optimal investment quota have been calculated.

By comparing the entries in lines two and three of Table 1, it is easy to see that the optimal capital replacement period and the labor elasticity of output vary inversely. Since the capital elasticity of output varies inversely with the labor elasticity of output, it follows that the optimal capital replacement period varies directly with the elasticity of output

TABLE 1

THE OPTIMAL CAPITAL REPLACEMENT PERIOD

μ	λ	n	b	β	$\bar{\theta}$	$m(\bar{\theta})$	K/Q
.00	.02	.005	.70	.034	22	9.7	4.6
.01	.01	.01	.70	.039	33	13.2	5.6
.01	.01	.01	.80	.035	27	11.4	3.5
.01	.01	.005	.80	.03	21	9.4	3.1
.01	.02	.01	.80	.038	22	9.3	3.0
.005	.01	.01	.80	.029	30	12.7	4.0
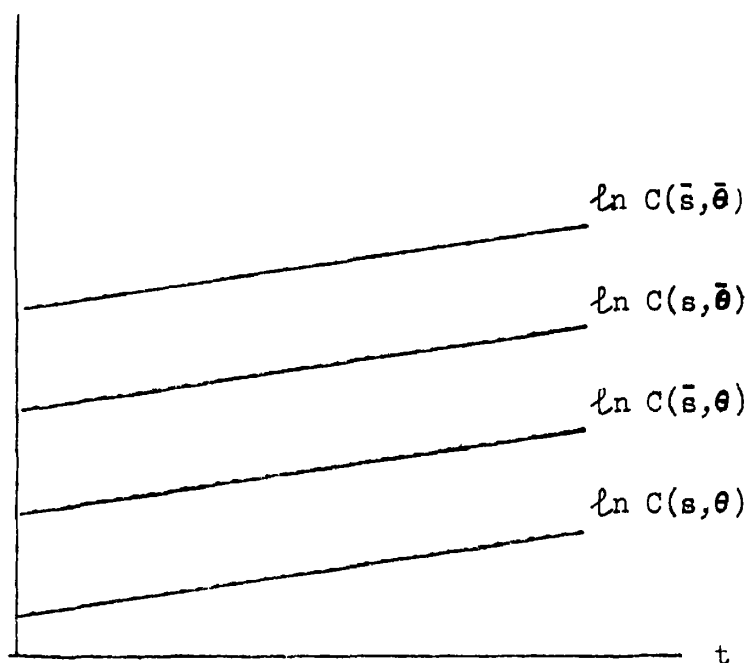
FIGURE 1

EQUILIBRIUM CONSUMPTION TRACKS FOR DIFFERENT VALUES OF THE INVESTMENT RATIO, s, AND THE LONGEVITY OF CAPITAL, θ.

with respect to capital input. By comparing lines three and four of Table 1, it follows that the optimal longevity of capital and the rate of growth of the labor force vary directly. Similar comparisons of the various entries in Table 1 show that the optimal capital replacement period varies inversely with both the rate of embodied technical progress and the rate of organizational technical progress.

The basic argument of this section may be summarized very briefly by referring to Figure 1. In this figure time is measured on the horizontal axis and the logarithm of consumption at time t is measured on the vertical axis. The parallel consumption tracks correspond to various pairs of values of the investment ratio and the longevity of capital. These consumption tracks are parallel because they have a common slope equal to the rate of growth of output. The highest attainable consumption track, labeled $lnC(\bar{s}, \bar{\theta})$, is the consumption track which corresponds to the optimal investment ratio and the optimal longevity of capital. All other possible consumption tracks are lower than this optimal track. Since the optimal investment ratio and the optimal longevity of capital are independently determined, these lower consumption

tracks correspond to nonoptimal values for either the investment rati
the longevity of capital, or both.

IV. *The Economic Life of Capital*

In this section the problem of determining the economic life of capit
within the context of the macroeconomic growth model set out in Se
tion II is considered. The volume of gross investment at each point i
time is considered to be exogenously specified. However, the type o
capital which is constructed, that is, whether more or less labor is use
in conjunction with this capital, and the period of time for which it wi
be operated are both considered to be endogenous variables. It is a
sumed that the type of capital which is constructed and the econom
life of capital are both determined by maximizing total discounte
profit. In order to determine discounted profit, it is necessary to intro
duce the wage rate and the interest rate explicitly into the analysis. Th
wage rate is assumed to adjust so that full employment is continuousl
maintained and the interest rate is assumed to adjust so as to annihilat
any pure profit or loss.

Let $R_v(t)$ denote the net return to vintage v capital at time $t \geq v$. The

$$R_v(t) = Q_v(t) - w(t)L_v(t) \tag{4.1}$$

where $Q_v(t)$ is the output of vintage v capital at time t, $L_v(t)$ is the labo
required to operate vintage v capital at time t, and $w(t)$ is the wage rat
at time t. The return to vintage v capital at any time is equal to the ou
put less the operating cost at that time. Let $r(t)$ denote the rate of in
terest at time t. The total discounted profit obtained from the operatio
of vintage v capital for θ years is

$$P(v) = \int_v^{v+\theta} R_v(t)E(t, v)dt - I(v). \tag{4.2}$$

The function $E(t, v) = \exp\left[-\int_v^t r(x)dx\right]$ represents the discount facto
which converts the net revenue of time $t \geq v$ to its present value at tim
v. Total discounted profit is obtained by subtracting the initial cost o
vintage v capital, $I(v)$, from the total discounted net revenue obtaine
from this capital over its life.

The type of capital which will be constructed at time v may be ob
tained by maximizing (4.2) with respect to $L_v(v)$ $(= L_v(t))$.

$$\int_v^{v+\theta} [\partial Q_v(t)/\partial L_v(t)]E(t, v)dt = \int_v^{v+\theta} w(t)E(t, v)dt \tag{4.3}$$

This condition states that the amount of labor which is used in conjunc
tion with vintage v capital will be increased up to the point at which th

discounted marginal product of labor is equal to the discounted marginal cost of labor. This condition is, of course, similar to the familiar condition that labor will be utilized up to the point at which the marginal product of labor is equal to the wage rate. In fact, (4.3) reduces to this condition if both the marginal product of labor working with vintage v capital and the wage rate are constant over time.

Within the context of the simple growth model of the preceding section equation (4.3) may be interpreted in another way. Once the interest rate and the longevity of capital are specified, the condition of continuous full employment of the labor force, (2.6), may be used to determine the amount of labor which is available to work with new capital at each point in time. With $L_v(t)$ thus determined by equation (2.6), equation (4.3) may be interpreted as imposing a condition on the time path of the wage rate. Specifically, the wage rate must adjust so that the amount of labor demanded to work with capital of vintage v is equal to the amount of labor actually available to work with vintage v capital. In this way the equilibrium time path of the wage rate is determined.

The economic life of capital may be determined by maximizing total discounted profit with respect to capital longevity. Setting the partial derivative of (4.2) with respect to θ equal to zero yields

$$Q_v(v + \theta) = w(v + \theta)L_v(v + \theta). \tag{4.4}$$

This condition states that, in order to maximize profit, capital should be retained in the production structure only so long as the net revenue obtained from that capital is nonnegative.

If the time path of the rate of interest is regarded as being exogenously determined, then equations (4.3), (4.4), and (2.6) may be solved simultaneously for the equilibrium values of the time path of the wage rate and the economic life of capital which equilibrate the system. But if the rate of interest is exogenous, there is very likely to be a pure profit or loss resulting from the operation of capital. To preclude this possibility, the interest rate is assumed to adjust so that pure profits or losses are annihilated. This leads to the equilibrium condition

$$P(v) = 0. \tag{4.5}$$

This condition simply states that the rate of interest adjusts so that the total discounted net revenue obtained from the operation of vintage v capital is equal to the initial cost of this capital. The three equilibrium conditions (4.3), (4.4), and (4.5) together with (2.6) simultaneously determine the equilibrium values of the wage rate, the interest rate, and the longevity of capital.

It is possible to determine the general properties of the equilibrium

time paths of the wage and interest rates without actually solving thi system of equations explicitly. It may be shown that for the uniforr replacement period θ to persist over time, the equilibrium wage rat must be increasing over time at the relative rate Δ/b, where Δ is th sum of the relative rates of embodied and organizational technica change. In addition, the interest rate must be constant over time if th uniform replacement period θ is to obtain. These two properties of th wage and interest rates may be used to simplify conditions (4.3)–(4.5) Substituting $r(t)=r$ and $w(t)=w(v)e^{(\Delta/b)(t-v)}$ into (4.3), performing th indicated integration, and solving for $w(v)$ leads to

$$(4.3') \qquad w(v) = b[Q_v(v)/L_v(v)]g(r-\mu, \theta)^{-1}g(r-(\Delta/b), \theta).$$

If there were no technical progress in this model so that $\mu=\Delta=0$, thi equation would state that the wage rate is equal to the marginal produc of labor at each point in time. However, since technical progress is pres ent in this model, the wage rate is equal to the marginal product of labo multiplied by the factor $g(r-\mu, \theta)^{-1}g(r-(\Delta/b), \theta)$.

Substituting the value of the wage rate at time $v+\theta$, $w(v+\theta) =w(v)e^{(\Delta/b)\theta}$, into (4.4), and recalling that $Q_v(v+\theta)=Q_v(v)e^{\mu\theta}$ anc $L_v(v+\theta)=L_v(v)$, the economic life of capital is obtained by solving

$$(4.4') \qquad e^{\mu\theta} = bg(r-\mu, \theta)^{-1}g(r-(\Delta/b), \theta)e^{(\Delta/b)\theta}$$

for θ. This simplified equilibrium condition which must be satisfied by the economic life of capital states that capital is retained in the produc tion structure only so long as labor's share in output is not greater thar total output. Since labor's relative share of vintage v output is increasing more rapidly than is vintage v output, a finite economic life will satisfy this condition.

The third equilibrium condition, the zero profit condition, simplifies to

$$Q_v(v)g(r-\mu, \theta)^{-1} - w(v)L_v(v)g(r-(\Delta/b), \theta)^{-1} = I(v)$$

when $r(t)=r$ and $w(t)=w(v)e^{(\Delta/b)(t-v)}$ are substituted into (4.5). Eliminating the wage rate from this equation by substitution from (4.3′) and dividing both sides of the resulting expression by $I(v)$ leads to

$$(4.5') \qquad (a/s)g(\sigma, \theta)g(r-\mu, \theta)^{-1} = 1.$$

In this equation, $s^{-1}g(\sigma, \theta)$ is the initial output-capital ratio, $Q_v(v)/I(v)$, of vintage v capital, a is vintage v capital's relative share in the output which it produces at time v, and $g(r-\mu, \theta)^{-1}$ is the factor which converts the total output of vintage v capital over its life to its present value. This condition thus states that the total discounted return to each unit

of vintage v capital must be equal to the initial cost of each unit of vintage v capital which is, by definition, one.

The formal problem of determining the economic life of capital within the context of the equilibrium growth model of Section IV has now been solved. By solving equations (4.3′)–(4.5′) simultaneously, the equilibrium economic life of capital may be obtained. Fortunately an explicit solution of these equations is not required for the task at hand.[4] By comparing the equations which determine the economic life of capital with that determining the optimal capital replacement period as described in the previous section, the conditions which must be satisfied for their equivalence may easily be determined.

V. *The Economic Life of Capital and the Optimal Capital Replacement Period*

The optimal capital replacement period is obtained by solving

$$e^{-\sigma\bar{\theta}} = bg(n, \bar{\theta})g(\sigma, \bar{\theta})^{-1}e^{-n\bar{\theta}} \tag{5.1}$$

for $\bar{\theta}$. The economic life of capital is obtained by solving (4.4′) for θ. Noting that $\mu=\beta-\sigma$ and $\Delta/b=\beta-n$, (4.4′) may be rewritten as

$$e^{-\sigma\theta} = bg(r - \beta + \sigma, \theta)^{-1}g(r - \beta + n, \theta)e^{-n\theta}. \tag{5.2}$$

By comparing (5.1) and (5.2), the conditions under which the optimal capital replacement period and the economic life of capital coincide may be determined.

By inspection a sufficient condition for the optimal capital replacement period and the economic life of capital to coincide may be determined. Equations (5.1) and (5.2) will be identical if the rate of interest is equal to the rate of growth of output. Thus $\theta=\bar{\theta}$ if $r=\beta$. To determine whether this is also a necessary condition, suppose that $\theta=\bar{\theta}$. Then (5.1) and (5.2) together imply that

$$\frac{g(x + \sigma, \theta)g(n, \theta)}{g(x + n, \theta)g(\sigma, \theta)} = 1, \tag{5.3}$$

where x is inserted in place of $r-\beta$. If $x=0$ is the only solution of this equation, then $r=\beta$ is not only a sufficient condition for θ and $\bar{\theta}$ to coincide; it is also a necessary condition. It may be shown that $x=0$ is in fact the only solution to (5.3) so that $r=\beta$ is both a necessary and a sufficient condition for $\theta=\bar{\theta}$.

An alternative but equivalent condition for the equality of the optimal

[4] The existence of a solution to the set of equations (4.3′)–(4.5′) and comparative static properties of this solution may be shown by an argument analogous to that of Phelps [9].

capital replacement period and the economic life of capital may be obtained from the zero profit condition. Substituting $\beta-\sigma$ for μ in (4.5′), this equation may be rewritten as

$$(a/s)g(\sigma, \theta)g(r - \beta + \sigma, \theta)^{-1} = 1. \tag{5.4}$$

From this zero profit condition it follows that the rate of interest is equal to the rate of growth of output if the investment ratio is equal to a. Moreover, this equation asserts that the rate of interest is equal to the rate of growth only if the investment is equal to a. This means that the economic and optimal lives of capital coincide if and only if the investment ratio is optimal.

A third condition which is implicit in the preceding two may be obtained for the wage rate. Substituting from (2.1) and (2.6) for $Q_v(v)$ and $L_v(v)$ in (4.3′), the full employment wage rate may be rewritten as

$$w(t) = (s/a)bs^{a/b}A^{1/b}g(\sigma, \theta)^{-1/b}g(r - \beta + n, \theta)e^{(\Delta/b)t}. \tag{5.5}$$

Here the zero profit condition (4.5′) has been used to eliminate $g(r-\mu,\theta)^{-1}$ from (4.3′). The marginal product of labor at time t within the context of the equilibrium growth model may be determined by differentiating (2.9) with respect to $L(t)$. This leads to the expression

$$\partial Q(t)/\partial L(t) = bs^{a/b}A^{1/b}g(n, \theta)g(\sigma, \theta)^{-1/b}e^{(\Delta/b)t}. \tag{5.6}$$

Comparing (5.5) and (5.6), it is found that the full employment wage rate will be equal to the marginal product of labor if and only if the investment ratio is optimal and hence the interest rate is equal to the equilibrium rate of growth of output. Thus a third condition that $\theta=\bar{\theta}$ is that the full employment wage rate be equal to the marginal product of the total labor force.

VI. *Summary and Related Problems*

This paper has been devoted to an examination of two alternative capital replacement criteria which have been advanced in the literature as normative standards for mature economies. These two criteria were compared within the context of a growth model the operation of which may be summarized as follows. At any time there is a certain amount of labor available to work with new capital as determined by the net growth of the labor force and the retirement of old capital. There is also at any time a certain amount of new capital constructed as determined by the investment ratio and the level of output. It was required that the capital be constructed of such a type that full employment is continuously maintained. The equilibrium growth path of gross output was then determined by considering the investment ratio and the age at which

capital is retired as parameters, the values of which were specified from outside the model.

A macroeconomic replacement rule was derived by maximizing the level of the equilibrium growth path of consumption. For this growth model, the replacement rule was seen to be independent of the investment-income ratio. A second criterion, profit maximization, was used to determine the economic life of capital. These two criteria were seen to yield the same life of capital provided the rate of interest is equal to the rate of growth of output which in turn is true if and only if the investment-income ratio is optimal; i.e., the golden rule of accumulation is being followed. The introduction of profit maximization conditions into the macroeconomic growth model clearly establishes the interdependence of the economic life of capital and the investment-income ratio.

This formulation of the problem discussed in this paper suggests two related questions. The first has to do with the magnitude which is being maximized—in this case, the level of the equilibrium growth path of consumption. It was repeatedly emphasized that this paper was concerned only with alternative equilibrium growth paths of output and consumption. Much more difficult problems arise in the process of comparing the dynamic time paths of these variables while in the process of moving from one equilibrium time path to another. In such cases the problem of evaluating intersecting time paths of the variables must be faced squarely.

A second problem suggested by the discussion of this paper is concerned with the interrelationship of the investment-income ratio and the longevity of capital. Economic policies oriented toward increasing the rate of growth have been concerned with increasing the investment-income ratio. Very little thought has been given to the possibility that policies which attempt to encourage an increase in the level of investment may be more or less offset by an unforeseen change in the longevity of capital. In designing economic policies, both of these parameters must be kept in mind.

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DISCUSSION

VINCENT F. BOLAND: Though they deal with widely separated subjects, the papers presented here have a great deal in common. Each author concerns himself with the modification of established empirically testable models, and each is preoccupied with the implications of his model for economic growth. In each paper great respect for theoretical foundations is demonstrated, and this is certainly a most healthy symptom in empirically-oriented work. However, if any trend is discernible from this small sample, one might deplore the fashion which puts the best young minds to work testing the theories of others rather than developing theories of their own. This is especially so because experience in economics, as well as in most other disciplines dependent on abstract reasoning, consistently indicates that really original theoretical work is almost a monopoly of youth.

In Dr. Levy's highly suggestive discussion of an adaptive production function, some confusion arises in consideration of the implications of the function for employment policy. This confusion appears to be related to his use of Mrs. Robinson's "efficiency units" for the measurement of labor inputs. (In fact, Dr. Levy's unit appears more similar to Mrs. Robinson's "corrected natural unit"—a device which she very properly disowned in 1934.) The use of such a transformation begs certain essential questions and requires that a reverse transformation be executed before the results of the analysis can be compared with the real world. Thus, the employment of "more labor units . . . at a reduced wage per unit" may well mean reduced employment in terms of natural labor units. In succeeding paragraphs, increases in both the supply schedule of labor and the demand schedule for labor are attributed to quality improvements resulting from training. The change in the supply schedule implies that labor is measured in terms of corrected or efficiency units, while the change in the demand schedule implies that the measurement is in natural units. In fact, the result of increased efficiency of labor, measured in natural units, is ambiguous. If demand for labor measured in terms of efficiency units is inelastic within a sector, the greater the increase in labor productivity the lower the level of employment. Increased efficiency of labor can cause technological unemployment of labor as directly as can increased efficiency of capital. The distinction between increased efficiency of labor and increased efficiency of capital vanishes if we take a reasonably long view and regard capital as indirectly applied labor.

Dr. Winston's proposal to incorporate both the "participation rate" and the "average amount of time worked" into empirical studies of labor force data is obviously well worth while. It is certainly true that many empirical studies have concentrated entirely on participation rates, while textbooks place heavy emphasis on individual equilibrium. Thus the "backward rising" supply of labor deduced in the textbooks finds little empirical support in Long's standard study of labor force data or in other similar work. Dr. Win-

ston's summary of the empirical testing of his model indicates that he has provided some badly needed support for conventional theory.

The model as it stands differentiates between marketed time and nonmarketed time rather than between work and leisure. Therefore it does not provide a real test of the conventional analysis of individual equilibrium. Dr. Winston justifies this approach by maintaining that there is "little evidence" of any "systematic relationship between the proportion of leisure time which adds to income and the level of income attained." Real justification would require contrary evidence rather than lack of evidence. While it may not be well quantified, there is some evidence of systematic withdrawal from subsistence agriculture at certain stages of development and of systematic movement of women into the "household sector" at other stages. Though development of adequate data would be difficult, studies which differentiate between all productive use of time and true leisure would be of great interest.

The pattern of demonstration effects discussed by Dr. Winston seems to involve very peculiar psychological assumptions. For example, if the effort price of an individual's services remained constant while the "normal" effort-price varied continuously from a level above that of the individual to one below it, the rate at which he would be willing to substitute leisure for income (the slope of the indifference curves) would first increase and then decrease. Movement in a uniform direction would seem more consistent, though one may do what one pleases with psychological assumptions. In Dr. Winston's model, any distribution pattern increasing the degree of inequality of wages would also increase the supply of labor.

The main conclusion of Dr. Howrey's paper, that the life period of capital which maximizes the consumption flow will be attained under competitive market conditions if the interest rate is equal to the rate of growth in output, depends on the assumption that the production function is homogeneous of order one. While this assumption is mentioned in passing in one footnote, it is not introduced explicitly into the analysis. It enters implicitly in the substitution of (β-n) for Δ/b in developing equation (5.2). If the possibility of increasing returns to scale is allowed, the result must be modified.

As a minor sidelight on Dr. Howrey's paper, it may be mentioned that there is an alternative solution to equation (5.3), namely, $n = \sigma$; that is, the growth in output which is not attributable to organizational technical progress is measured entirely by the increase in the labor force. However, this solution is almost degenerate, as it implies that the marginal productivity of capital, as well as the rate of embodied technical progress, is zero.

HANS BREMS: Winston has shown us some of the pitfalls in aggregating individual supply-of-labor functions. But he hardly does justice to them by calling them "indeterminate." Anyone willing to specify the form of Winston's *utility function* may easily derive the individual's supply-of-labor function from it. For example, let the utility function be of the constant-elasticity-of-substitution type. Then an elasticity of substitution greater than one is the case of income and leisure being close substitutes, and the Hicksian substitution effect will overwhelm the income effect: A reduction of the effort-price p_e

of income will make the individual spend more time earning income. If the elasticity of substitution equals one, the substitution and income effects will cancel, and a reduction of p_e will have no effects upon hours worked. Finally, an elasticity of substitution lying between zero and one is the case in which income is such a poor substitute for leisure that the income effect swamps the substitution effect. A reduction of p_e will make the individual spend less time earning income. So there is no indeterminacy. But you do not get something for nothing: with no input of premises you do not get any output of conclusions.

As for the positive part of Winston's paper, I have two comments. First, it is highly misleading to say that an increase in the relative valuation of income shifts the indifference curves downward. Part of any given indifference curve will shift downward; part will shift upward. It is better to say that the curve is tilted. Look at the simplest possible form of a utility function having positive and declining marginal utilities; i.e., the Cobb-Douglas form $U = MY^{\alpha}L^{\beta}$, where α and β are parameters lying between zero and one, and where M is a positive multiplicative factor depending upon the units of measurement. Here, a higher relative valuation of income manifests itself in a higher α and a lower β. As we know, the elasticity of an indifference curve derived from such a utility function is constant:

$$\frac{dL}{dY}\frac{Y}{L} = -\frac{\alpha}{\beta}$$

So in a double-logarithmic scale the indifference curves are a family of straight lines which are the steeper the higher α and the lower β. My second comment on the positive part of Winston's paper is a question: Why should the demonstration effect manifest itself precisely in a "higher relative valuation of income as the effort-price differential increases"? Winston seems to offer neither theoretical nor empirical evidence to this effect.

Howrey's paper is a member of the larger family of "vintage" growth models and a member of the subfamily in which there is substitution between capital and labor before the capital is in place, but none afterward. The subfamily descends from Leif Johansen's April, 1959, and Massell's April, 1962, papers in *Econometrica*. In fact, Howrey's first two parts, setting out the growth model and determining the socially optimal retirement age, exactly reproduce Massell's paper. In such vintage models, within period v the whole economy produces one commodity. A fraction of the output of it is a flow consumed within period v. The rest is a stock of vintage v producers' goods, set aside and added to the already existing stocks of previntage v producers' goods. Producers' goods of different vintage are different, so viciously speaking, food does not differ from machines, but machines of vintage v differ from machines of vintage $v - 1$. Perhaps a coming subfamily of the vintage models will abandon the one-commodity assumption and produce two goods: consumers' goods and producers' goods. This would make it possible to include qualitative growth; we consume not merely more goods but also better ones. It would also make possible a distinction between men operating the machines and men building them.

Howrey's last two parts—determining the privately optimal retirement age and the conditions under which it coincides with the socially optimal one—are new and highly welcome. To determine the privately optimal retirement age, Howrey introduces wage and interest rates parametric to the firm but variable to the economy. The time path of the wage rate is determined by the condition that it clears the labor market at all times. The time path of the interest rate is determined by the condition that at time v, the net worth of all capital of vintage v be zero. But another condition seems to be imposed by Howrey; i.e., that the interest rate be stationary. Otherwise, Howrey says, the uniform privately optimal retirement age will not obtain. One wonders if the stationary rate of interest is really a condition. Couldn't it simply be a result? We know that in old-style Cobb-Douglas growth models with completely disembodied technological progress a stationary interest rate was a result found by solving the model. We also know that in such models, the stationary interest rate did not survive the abandonment of the Cobb-Douglas production function in favor of a constant-elasticity-of-substitution production function. Kendrick and Sato (in the December, 1963, *A.E.R.*) found that the rate of return on capital would have fallen had the savings ratio been constant. Whether a condition or a result, the stationary interest rate in Howrey's model might also not survive the next notch of generalization; e.g., the abandonment of the Cobb-Douglas production function.

Howrey's most important finding is that if the ratio of gross investment to output equals the elasticity of output with respect to capital input—if in other words the Golden Rule of Accumulation is followed—then and only then will the rate of growth equal the rate of interest, and the socially and privately optimal retirement age will coincide. Now Howrey's model is a completely aggregated, one-commodity model with technological progress. Almost thirty years ago, Von Neumann published his completely disaggregated growth model without any technological progress in it. In this model, too, the rate of growth of all process levels equaled the rate of interest. The equilibrium rate of growth of all process levels was the rate of growth of the slowest growing commodity or commodities—commodities growing faster than that became free goods. The equilibrium rate of interest equaled the internal rate of return of the most profitable process or processes. Processes less profitable than that would remain unused. The similarity of results in two such strikingly dissimilar models as Von Neumann's and Howrey's reminds us that we still are not done with our generalization job.

David McCord Wright: These three papers are all econometric papers. They all suffer from the difficulties which the method, as such, has in coming to grips with the growing and, secondarily, the capitalist world. Space is lacking to review the larger issues, as in my "What Is the Economic System?" (*Q.J.E.*, May, 1958). I concentrate, instead, on two practical issues. In any growing society, growth comes through change and causes change. There will necessarily be a constant change in the type of goods wanted and the methods of production used. The process of growth involves a constant revision and redirection of the product mix. Our aggregative technique finds it hard even to

remember this, the essential, growth problem. Worse yet, the "constant revolutionizing of the means of production" causes men to change their ideology and institutions, and these changes reflect back upon the performance of the aggregates. Reality consists both of objective economic forces and of cultural alteration.

Since the modern technique finds these partial, discontinuous, and cultural changes very difficult to handle, there is a tendency either to throw out the econometrics or to throw out reality. What to do? I do not suppose so radical a dilemma myself. Econometric models are useful as long as we remember the rest of the total pattern. What we must ask first is: Does the model bring out anything new? I mean, does it tell us of some really new principle at work? Second, does it point out some new field for practical research? Have we overlooked some real aspect of the problem? Finally, are the models and results any good for prediction? Do they help in forecasting?

I am obliged to say that I find these papers, judged by the principles given, rather disappointing. First comes Dr. Levy, who tells us, in essence, that the first hundred years are the hardest in training someone. The information is not very new. So far as the actual study is concerned, we merely have the record of one enterprise in a certain period. The real factors operating are the managerial and human relations techniques used in this special case. To use the "function" for prediction would require similar or "as good" techniques. The function as such is sterile.

Next comes Dr. Winston who tells us that "keeping up with the Joneses" is important in determining how much a man will keep working as he gets wealthier. So far so good. But again this is scarcely news, and the crucial cultural and anthropological forces at work are not mentioned.

Dr. Howrey's paper despite its impressive expertise I find least satisfactory. He asks why economies (countries) get out of date, and thinks it is because replacement is not made soon enough. But even if replacement is made quickly, there might be forces like featherbedding, security sabotage, strikes, and so on that would force the use of obsolete—or not up-to-date methods—even in the replacement. The whole problem of adjusting the pattern of production and the psychological resistances to change, which is the real force operating here, is completely ignored in Howrey's smooth, one commodity, aggregative model. His work ignores so large a part of the problem that I do not see how any of his conclusions could be of the least practical relevance.

Summarizing, these three papers show intelligence, hard work, and expertise of a narrow sort. But they are far from impressive. What is the difficulty? I believe the matter can best be summed up by a quote from a letter that came to me not long ago: "Our modern graduate students are more interested in solving mathematical puzzles than in studying the economic system as it really operates." Reality is not a tidy sewing machine. It is both analytic and spontaneous. It is more important to be accurate—which means imprecise in many cases—than to be merely logically tidy but irrelevant.

DEFENSE ECONOMICS: APPLYING ECONOMIC CRITERIA

USING COSTS TO SELECT WEAPONS

By STEPHEN ENKE
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Introduction

Twenty years ago professional economists and military officers hardly spoke to one another—they were barely acquainted. When, in today's Pentagon, generals and admirals do not speak to defense economists, it is for quite another reason. For there has been something of an intellectual and administrative revolution in the interim, so that currently many economists make a significant contribution to selecting among rival weapon systems, costing alternative future force structures, and so on. Partly because of security and inevitable cultural lags, most of our profession remain unaware of what their colleagues have been doing within the Office of Secretary of Defense, at RAND, the Institute for Defense Analyses, and other nonprofit "think factories."[1] This paper attempts to describe all too briefly the evolution, accomplishments, and limitations of what have become known throughout the military establishment as "cost effectiveness studies."

Developing First Principles

The first few economists arriving at RAND in the late 1940's found operations analysts, aircraft and electronic engineers, plus some mathematicians, at work determining the "best" strategic bomber to recommend to the USAF for development and next generation use.

The expected state of technological art that would be available over

[1] The Center for Naval Analyses (then Operations Evaluation Group), a contractor of the U.S. Navy, established an economics division under the author's direction in 1961; it was subsequently expanded and improved by William B. Meckling. The Institute for Defense Analyses, a contractor of the Secretary of Defense, has a growing economics competence that was reorganized by the author from 1962-64 and is now under William A. Niskanen. The Research Analyses Corporation, a contractor of the U.S. Army, has an economics division directed by Robert Grosse. However, RAND has been preeminent in the area of defense economics since the arrival of Charles J. Hitch (now Comptroller of the Department of Defense), David Novick, and the author in 1948. Important contributors to the theory of defense economics while at RAND were Armen A. Alchian, Burton H. Klein, Alain C. Enthoven, Andrew W. Marshall, Jack Hirshliefer, Roland N. McKean, R. T. Nichols, Malcolm W. Hoag, Fred Hoffman, Donald Fort, and others too numerous to mention. Some of their Contributions appear in ***Economics of Defense in the Nuclear Age*** (Harvard Univ. Press, 1960).

the next few years had already been estimated by the engineers. With parametric studies—and using so-called "rubber" engines and airframes—they "designed" in the computer alternative kinds of bombers for the strategic mission. But this mission "job" was variously defined at first and the system constraints were often unclear.

The USAF had by around 1950 a choice among a number of rather different next generation bombers. Some were turboprops, relatively slow and low flying, but with excellent payload-to-range possibilities; however these aircraft, although expected to have slightly better bombing accuracy from lower altitudes at slower speeds, were also expected to be more vulnerable to "enemy" local and area defenses for the same reason. Alternatively, there were various turbojet designs, faster and higher flying, with presumably less bombing accuracy but also much less vulnerability. The earliest analyses assumed nuclear bombs of standard weight, size, and "bang." The capabilities of area defenses and local defenses at targets were stipulated too.

Sometimes the strategic job was defined in part as being the expected destruction of a stated fraction of x targets, these latter being described in terms of size, physical vulnerability to overpressure, and penetration distance through enemy defended skies.

The results of these strategic campaigns, fought in the computer, were found to be very sensitive to other facets of the problem's definition. For instance, were the bombers of the strategic force to be allowed only a single simultaneous strike, or could they have a second strike if they survived the first, etc.? Were the nuclear bombs in fixed supply or was there an available nuclear bomb for every bomber taking off?

In any event, assuming for argument that multiple strikes are possible but that the number of standard bombs available is constrainted, what criterion should be applied to determine the "best" kind of bomber to comprise a homogeneous strategic force in being? Is the best kind of bomber the one of which the minimum number will have to be bought for a force that will just do the stipulated job? Is the best bomber the kind that results in a force that will lose the least number of aircraft and hence crews to enemy action? Or is it the one that will do the job with the least number of nuclear bombs?

The application of each criterion results not only in a different bomber of different performance being best, but a different number purchased and lost, different penetration tactics, different numbers of bombs required and used.

For a long while there was no agreement as to what should be minimized in accomplishing the strategic job. Eventually an engineer suggested that the best bomber might be that which resulted in the least

total weight of aircraft construction for the system—on the grounds that aircraft weight and construction costs supposedly covary. Some economist countered by suggesting that dollars be minimized specifically—and that dollars for keeping the force in being were as important over four to five years as dollars for acquiring it initially. Some other economist made a speech to the effect that dollars are really an alias for generalized resources when it comes to developing, procuring, and maintaining a future military force over a period of a decade or more.

In a crude sense, back at the old Broadway Building in Santa Monica, that was how cost effectiveness, to result later in McNamara-Hitch "Program Packaging," was born into the defense community.

Establishing Use and Opportunity Costs

Of course, even when developing a weapon system that will not go into operation for four years or more, there can be particular inputs to the system that have a value far greater than their costs of production. Back around 1950 the outstanding example was fissionable material—U 258 and Pu 239. At that time even unofficial estimates of the nation's nuclear stockpile suggested that lack of bombs rather than bombers was the most likely limitation. This meant that the economical use and value of fissionable materials had to be examined.

Variable Proportions and Nuclear Weapons. Some early bomber system studies had assumed that either there was a fixed supply of nuclear materials available for the mission or that a bomb would somehow be made available for each bomber on take-off. These nuclear bombs were never costed into the bomber systems—there was no reason to do so when costs were not the criterion—and hence in effect they were treated as free goods. The economists quickly realized that application should be made of the principal of variable proportions; if targets destroyed was the output, the two main inputs were clearly fissionable materials and delivery vehicles.[2]

In cases where the number of targets to be destroyed was not much smaller than the number of bombs presumably available, there was obviously a premium on getting the bomb to the release point near target. One way to accomplish this—but at rapidly increasing cost—was to design and build higher performance bombers. The other option was bomber quantity. One penetration tactic considered was to have several empty decoy bombers "escorting" each bomb-carrying aircraft, serving to saturate the defenses and diverting defensive fire from the plane with the "egg."

Another way to change the nuclear materials to delivery vehicle

[2] See Stephen Enke, "Some Economic Aspects of Fissionable Materials," *Q.J.E.*, May, 1954.

ratio of the system was to increase the kiloton bang of each bomb by increasing its fissionable materials content.

On investigating why the standard bombs assigned to the strategic mission had the bangs they did, it appeared that their specifications had been decided in no small way by an earlier operations analysis that treated delivery as free! Figuratively, these calculations had been made at the bomb release point, as though getting the bomb to the target area was no problem. Target physical vulnerability and air-crew bombing accuracy were thus almost the sole determinants of "optimum" bomb yields and nuclear contents. Yet at that time the strategic delivery system was costing over $5 billion a year and so was hardly free.

Including delivery costs made it seem that a very different kind of bomb would be better, just as including the value of assigned atomic bombs made another kind of bomber seem best, because both inputs were scarce and could be varied in proportions.[3]

Use Values and Production Costs of Fissionable Materials. Obviously, once a complete systems analysis of a strategic or tactical force had been programmed for the computer, it was a simple matter to establish the rates of substitution between one marginal bomber and a marginal kilogram of fissionable material (FM). And once the cost of acquiring and keeping a bomber was determined (see below), the use value of FM was revealed as the delivery costs it "saved." And in the early 1950's the marginal use value of FM appeared to be several times larger than the AEC's marginal cost of producing them—suggesting for example that the Oak Ridge gaseous diffusion plant should be operated more intensively.

Obviously the analysis was complicated by future uncertainties. Fissionable material stocks, unless used in war or for testing, grow every day. The delivery system—because of vehicle phase-outs—ordinarily does not grow as fast. Conceptually, in deciding optimum rates of FM production, one can estimate what annual rentals the Air Force, say, could pay for FM, imagine that the AEC receives these future rental streams, and suppose that the AEC then determines the present discounted capital value of its increments to the stockpile as a sort of marginal revenue.

Even allowing for new users and new uses—made possible by technological advances in "packaging" FM into novel weapons—it was obvious by the early 1950's that nuclear materials would be relatively far

[3] This is what can happen when two independent agencies of the government—here the Department of Defense and the Atomic Energy Commission—each suboptimizes on its own output in isolation. To each the input provided by the other appeared as a free good. Yet no one in the federal government below the Executive Office of the President could officially take action, and in the end a Special Assistant to the President had to be invoked.

less scarce a decade later. Given the inventory accumulations possible from AEC production facilities then in being, the problem was how to have larger stockpiles than would otherwise exist over say the next five years only. There was hence a premium on operating existing plants more intensively and expensively, rather than on building new plants that would have to cut back production later (as some have since done).[4]

Substitution Values in Two Uses. In the earliest days the strategic mission had exclusive claim upon the national stockpile of fissionable materials. However, if a small assignment of extra U 258 and/or Pu 239 could save many millions of dollars for delivery aircraft and crews otherwise needed for that mission, presumably there were also delivery system cost savings attainable from introducing nuclear weapons into tactical and other missions. Hence an urgent question in the early 1950's was whether some of the tactical air squadrons assigned to NATO should not be equipped with small yield nuclear weapons.

One resultant analysis estimated the marginal values in dollars of x kilograms of FM as a substitute for strategic bombers (in the strategic mission) and as a substitute for tactical bombers (in the tactical mission). Not very surprisingly, when the absolute assignment of FM was zero for tactical and all for strategic uses, the marginal value of nuclear weapon materials was much higher in the former use than the latter. The moral, of course, was that the nuclear stockpile needed to be reconstituted in part, with some new type weapons being reserved for tactical air missions in NATO.

(Economics instructors may be pleased to know that these demonstrations to senior generals that resources were being malallocated stressed Edgeworth-type box diagrams. The back-to-back curves were isoquants, showing numbers of strategic and tactical targets destroyed respectively, with a contract curve passing through their points of tangency. The then-current allocations of aircraft and "nukes" were not on the contract curve but at a corner of the box![5])

Costing Military Systems

Application of cost effectiveness criteria and the imputing of values to inputs having higher marginal use values than average production costs (e.g., fissionable materials) obviously first required the costing of alternative and hypothetical delivery systems. It was found that these future systems could best be costed in terms of the military organiza-

[4] It is amusing to remember the rather mixed reception some of the AEC Commissioners gave these ideas of some of RAND's youthful economists; at one stage the intellectual support of J. von Neumann, then still a consultant to the AEC, proved most helpful.

[5] A subsequent Supreme Commander of NATO was among those who thus learned about "contract curves."

tions that would use them in peacetime. This meant costing future B-47 systems by B-47 wings for instance.

However, the early RAND economists quickly discovered to their chagrin that the Air Force did not maintain cost data in such a form that one could readily "cost-out" a B-47 wing, for example. As in other military services at that time, costs were aggregated by object line items, giving actual or predicted disbursements for pay and allowances, ground support equipment, maintenance, POL, etc. Thus detailed estimates had to be made for instance of how much a B-47 wing per se would generate costs for pay and allowances, ground support equipment, maintenance, POL, etc. It was also necessary to estimate the cost of land, buildings, and other facilities used by operational Air Force units. And last but not least, the initial procurement cost of aircraft and spares had to be estimated, even though not yet in production. Thus most Air Force expenditure data, better to estimate future weapon system costs, had to be recast into "two dimensional" accounts, with object costs shown in rows and Air Force organization costs shown in columns.

These estimated costs of future weapons systems, aggregated by wings, distinguished between initial acquisition costs and annual operating costs. Originally, the acquisition cost was estimated as though the entire wing, with all it equipment and facilities, were purchased new. No allowance, in other words, was made for base facilities inherited from preceding systems (e.g., B-36 wings) being phased-out, and no salvage value was credited a new system for the assets it in turn might bequeath in another ten years perhaps to is successor. In later comparisons, as between rival strategic missile and aircraft systems, this clearly was not good enough. (Today, in the Pentagon, account is taken so far as possible of inherited and bequeathed assets.)

These evolutionary cost studies of the RAND Cost Department were an essential part of all cost effectiveness analyses conducted in Santa Monica.[6]

The military organizations costed—Strategic Air Command bomber wings and Air Defense Command interceptor squadrons, for example—had "outputs" in wartime that could be approximately measured and missions that related rather obviously to national objectives. In fact, this fortunate combination of circumstances was and is peculiarly true of most Air Force activities. Unfortunately for the analyst, many Army and Navy missions or outputs are often harder to define.

[6] The Head of the Cost Analysis Department at RAND was and is David Novick. Other notable contributors to the art and practice of costing have been Gene Fisher, Harold Asher, and Robert Grosse. Novick and Grosse played an important role in introducing rather similar methods of costing into the Department of Defense in 1961 and immediately thereafter.

Bases and Logistics

The increasing capability of the Soviet Union to attack our strategic forces on the ground, which became apparent to most planners by the second half of the 1950's, again stressed the importance of the cost effectiveness approach.

Understandably, there were some interests anxious to have the USAF procure a large number of intercontinental ballistic missiles (ICBM's), even though they were extremely vulnerable to enemy action if deployed unprotected on the surface. And yet every study that included probable losses on the ground from enemy action demonstrated that more targets could be destroyed in the Soviet Union, for a hypothetical U.S. budget, if most of these funds were devoted to hardened subterranean silos and only a small fraction of them to ICBM's themselves. After all, given a budget, the job was not to maximize missiles deployed but expected targets destroyed.[7]

Concern about vulnerability led to examination of dispersal, mobility, and hardening, as means of preserving most of our strategic missile and bomber forces. Both dispersal and mobility, if practiced to any extreme, occasion novel and costly logistics problems. It is difficult to pool maintenance facilities and spare part stocks when missile silos are widely dispersed, for example.

Accordingly, starting in the mid-fifties, cost effectiveness and marginal analysis were applied to Air Force logistics. It was soon discovered that there were many expenditure alternatives. To attain a given number of combat ready missiles it was found economical to some extent to have more missiles and a lower in-commission rate. And it was learned that a given in-commission rate could be achieved at minimum cost only by a careful marginal allocation of resources for on-site equipment, technicians, and spares as against centralized stockage and maintenance facilities.

These and other logistics studies also revealed that the support system for any given weapon had to be examined as a whole. Attempts at analytic factoring, considering ways to minimize transportation cost for instance, almost invariably were found to increase costs elsewhere in the system by more than the subsector savings. This is probably one reason why useful economic analyses of support systems had to wait for so long.[8]

[7] In this connection the country owes much to Albert Wohlstetter and his team, who were the first to stress ways of reducing the on-ground vulnerability of SAC.

[8] The RAND Logistics Department was organized in 1953 by the author, and some of its work has been described in "An Economist Looks at Air Force Logistics," *Rev. of Econ. and Statis.*, Aug., 1958.

Pentagon Cost/Benefit Analyses Today

The year 1961 marked the first official adoption within the Department of Defense (DOD) of costing methods and analytic practices roughly similar to those evolved during more than a decade under Project RAND. This was largely the accomplishment of Charles J. Hitch, who became Comptroller of the DOD under Secretary McNamara in January. This extension and formalization of cost effectiveness methodology, together with "two dimensional" accounts, revealed various new problems that have mostly been overcome.

In a sense the Department of Defense, with its military service departments, can be viewed as a giant and complex firm comprising numerous program "elements" that generate costs and benefits. Ideally, a program element is an activity combining men, equipment, and installations, usually undertaken by a single military service, whose effectiveness can be measured and which relates clearly to national security objectives. Thus infantry divisions, Polaris-equipped submarines, and tactical fighter aircraft are each treated as an element.

Ordinarily each element comprises a recognizable military organization. Hence these elements can be costed once a DOD system has been established for reaggregating existing cost data by organization. This was done for most of the military establishment during 1961-62 for the first time.

With certain limits, as outlined below, costing most of the military establishment by program elements permits several important advances in defense management. Where two elements have roughly the same kind of outputs, they can be compared for cost effectiveness, and one element (or system) may then show a marked superiority in output per million dollars. Or economies of scale may dictate the selection of one system and rejection of a rival, even though in cost effectiveness terms they are equivalent. Of course alternative elements (or systems) that compete in this manner not infrequently belong to different military services, and this explains much of the antipathy towards "whiz kid" economists occasionally exhibited by senior military officers.

Perhaps more important, where different and complementary program elements have measurable outputs that relate to the same national objective (e.g., defense of the U.S. from air attack), they can be grouped into a program package. It is then easier for top policy-makers to reach major budget reallocations between packages of elements, such as the packages of Strategic Retaliatory Forces, Continental Air and Missile Defense Forces, and Airlift and Sealift Forces. As the international situation changes, bringing new threats to the national se-

curity, the next annual Department of Defense budget can better take them into account.[9]

Some Practical Problems

There can be little rational dispute as to the advantages of applying cost effectiveness analyses to force structure programming. The real argument concerns the degree to which such methods can properly be applied. Many senior officers have protested that cost effectiveness comparisons among rival elements (systems) are often invalid because the systems are incomparable for one or more reasons.

First, the employment of otherwise comparable weapon systems may be too different for their selection or rejection on the basis of output per dollar. For example, Polaris-equipped submarines and B-52 bombers can both attack strategic targets; their system outputs are more or less measurable in targets destroyed, so their effectiveness might seem comparable on the basis of cost. But B-52 bombers are vulnerable on their bases to enemy attack in a way that Polaris missiles submerged below the water are not; bombers can be recalled prior to penetration in a way that ballistic missiles cannot; and manned bombers may be capable of a limited reconnaissance that intercontinental missiles are not. These differences are enough to give each system a comparative advantage against certain targets at certain stages of all-out war. Some critics argue that, given these differences, cost effectiveness comparisons cannot be conclusive. (The rejoinder of course is that cost effectiveness calculations in such a case are only one of many considerations in making a decision.)

Second, some systems are multipurpose with more than one military "output," and customarily these make a poor cost effectiveness showing when compared against a system designed exclusively for one of these purposes. For example, carrier based aircraft can perform many limited war missions that locally land based aircraft can accomplish, but the former can also be used in naval engagement far at sea and for blockading distant enemy coastlines. How can a cost effectiveness study give credit to naval air forces for such by-product outputs? It is difficult. Hence elements having overlapping outputs often cannot be compared according to cost effectiveness with much validity, except in cases where incremental units of some one element cause significant additions to only one kind of output.

Third, some program elements have no obviously measurable output,

[9] The procedures of the current programming system have been described by Alain C. Enthoven, "Economic Analysis in the Department of Defense," *A.E.R.*, May, 1963, pp. 413-23. Basically, the Office of Secretary of Defense establishes a Five Year Force Structure and Financial Plan each year for the guidance of the military (service) departments in preparing their annual budgets. The services then translate the approved five-year plan into requests for appropriations by object classes.

and this is especially true of general purpose naval and ground forces primarily intended for nonnuclear operations in overseas theaters. Does one measure the activity of an infantry division in terms of ammunition fired, additional terrain secured, or enemy personnel killed, for instance? Is the output of fleet destroyer escorts the number of submarines destroyed, or submarines thwarted from attacking, or escorted ships not torpedoed per unit time? Where an element has no useful measurable output, it is clear that cost effectiveness comparisons are hardly feasible. The best that can then be done, if the effectiveness of rival systems can be made subjectively the same by varying their size or number, is to ascertain which would be cheapest.

Nevertheless, although cost effectiveness assessments cannot be made of all elements, it is possible to classify the costs of almost the entire military establishment by program elements. And this recasting of the DOD's accounts makes better management possible. There is an analogy here between the Secretary of Defense and a householder. They are both purchasing a variety of goods and services that provide different benefits, many of which benefits are hardly measurable. But this does not mean they have no interest in knowing what these items will cost. Previously, while the costs of major items of equipment (e.g., Army tanks) were estimated for programming purposes, the future costs of the organizations that would operate them (e.g., armored divisions) were not routinely ascertained. Top policy-makers now at least have some specific costs by "elements" upon which they can bring subjective judgment to bear.

Some Further Refinements

The art of defense economics is still an evolving one and a brief indication should be given of the kinds of refinement that are possible.

Rate of Discount. Rival systems can have very different service lives. Sometimes one of two competing systems involves much higher initial costs but much lower operating costs subsequently. For example, a nuclear powered naval ship "saves" bunker fuel purchases later. In such cases a rate of discount is needed to obtain comparable present values. Some people feel that this discount rate should reflect the productivity of privately-invested capital; others feel it should be the interest rate that the federal government pays on its debts, although in practice federal budget constraints often result in much higher implicit rates of time preference around November.[10]

Better "Dynamic" Costing. The DOD Comptroller's office, in making financial plans for the future, only considers incremental costs gen-

[10] The Pentagon practice of taking five years' operating costs without discount is equivalent to discounting a ten-year life system at 15 percent, and in this light the current procedure often appears to have more rationale.

erated by program elements. If a new generation missile will inherit silos from a phasing out missile, this sunk cost is certainly not counted twice. However, it is also important on occasion to recognize that certain fixed assets that must be purchased for a coming first generation system may also have considerable value for the system that succeeds it in turn. Thus the discounted salvage values of assets to be bequeathed eventually by the first generation system should be deducted from that system's costs.

Budget Reactions of "Enemy" Planners. When major weapon systems that will seriously alter the balance of power between the U.S. and potential enemy nations are being considered, it is necessary to give thought to the reactions of such enemies. Will U.S. deployment of antiballistic missile defenses around important American cities cause the Soviets to increase their strategic missile force, for instance? And if so, with the result that the net Soviet capability remains unchanged after both nations have added to their forces, has the additional economic cost been larger for the U.S. than the Soviet Union? Conversely, is it possible for the U.S. to project forces that the Soviet Union must plan to counter, but at an unfavorable cost exchange ratio? This approach has difficulties. What constitutes a favorable dollar-to-ruble exchange rate for the U.S., where the net military balance remains unchanged? And when is it better to enforce a less favorable exchange rate because more resources are involved absolutely in some aspect of the arms race? Nevertheless, such an exchange rate approach intrigues those who view the cold war as an economic struggle waged from limited resources.

Conclusions

It is evident that there are many important resource allocation decisions within the DOD that can be improved by the application of traditional economic analysis. These principles are usually elementary. The art lies in recognizing situations where rules of economic efficiency are being violated, demonstrating that there is significant waste as a result, and convincing responsible officials that some alternative is better or even best. Now that economists have come to occupy positions where they can make these contributions and numerous organizations such as IDA are working on weapon selection problems, it is to be hoped that more members of the profession will interest themselves in this important area of research. Improving on the allocation of 10 percent of the national GNP is enough to keep more than a few economists busy assisting the military services and the Secretary of Defense.

MILITARY COST ANALYSIS

By Robert N. Grosse *and* Arnold Proschan
Research Analysis Corporation

Under the influence of Secretary McNamara, it has become an established practice in the Defense Department to support major program decision making with systematic analyses of possible program choices in terms of their military worth and costs. This does not diminish the role of the decision-maker in evaluating the analyses, assessing the nonquantitative aspects, and applying value judgments, but it does provide a basically rational framework for decision making, and one which fosters the imaginative generation of likely alternatives.

The relating of military worth and costs requires an objective expression of the military capability afforded by a given program. This expression may be accomplished in several ways; one or another may be more convenient in a particular problem.

The most common method is to specify a military task to be accomplished, and then determine alternative forces, weapons, and manpower of equal capability required to accomplish the task. The alternative military means for accomplishing the same task are then compared in terms of their costs.

Another method is to assume a certain cost level, and then detemine how much of each alternative force can be secured at that level. Capabilities of the alternative forces are then compared in terms of their potential outputs (e.g., targets destroyed) or in terms of each of their major characteristics deemed pertinent by military planners.

Where it is not possible to relate the alternatives in terms either of constant effectiveness or constant cost, the analysis must relate the alternatives in terms of different degrees of effectiveness and cost.

This very abbreviated description of how costs are related to military worth glosses over many problems in the determination of military worth. What if there are a variety of tasks to be performed by a military force or weapon system, how do we accord each its due? What if two weapon systems are not exactly comparable in all of their possible uses? How do we allow for the elements of a problem that do not lend themselves to quantification?

The measurement of military worth is a big subject in itself—one of primary concern to the practitioners of systems analyses of the kind we have been describing. Our present concern, however, is with the cost estimates to which the military worth will be related.

Military cost estimates are effected through application of the techniques of military cost analysis. Why the differentiation between estimates and analysis? Military cost analysis involves a thorough understanding of the structuring of systems analyses, the use of appropriate cost categories, discrimination between relevant and irrelevant cost elements, consideration of uncertainties, and application of cost sensitivity analyses.

More broadly, these are the elements of military cost analysis:

1. Emphasis on total program cost, to include the entire stream of cost consequences over the life of the program. The costs usually relate to peacetime preparation for war rather than to the consumption of resources in wartime; but in certain cases, it is reasonable to include costs of wartime consumption for a portion of the life of the system. In general, such wartime costs are excluded for strategic systems but are sometimes introduced for limited war and sublimited war systems.

2. Use of cost categories such as research and development, investment, and operating, which highlight the major phases in the life cycle.

3. Consideration not only of total program cost but also of the timing of costs. Involved here is the discounting problem and the importance in governmental programs of budgetary constraints by year.

4. Exclusion of "sunk" costs as a consideration in decision making relative to program choices.

5. Costing in a program only of the net resource requirements (allowing thereby for inheritances of manpower or equipment or any other application of already available resources). A new weapon system can be costed through cost comparisons of two forces: one is the force excluding the weapon system under study, and the other is the force including the weapon system. For use in such comparisons and for use in studies comparing alternative force mixes and deployments, force cost models have been devised. Such models systematically develop all categories of cost comprised in a force.

6. Emphasis on costs of relative rather than absolute accuracy, in consonance with the primary utilization of such costs in the comparison of alternatives.

7. Recognition of the uncertainties inherent in the specification and costing of future force mixes and weapon systems, especially in areas of advanced technology. To attain the utmost in capability and to avoid rapid obsolescence, many weapon system programs aim at achieving major improvements in key performance characteristics over their predecessors. Technical risks are taken for which there are few parallels in nondefense programs. Such risks result in some cases in outright program failures, but more often in completed programs which depart to a greater or lesser degree from the anticipation, in regard to time schedules, or performance characteristics or costs of the

weapon systems. Case histories of missile and other advanced programs have indicated how substantial can be the discrepancies between actual and anticipated costs.

8. As a generalization, the core of the costing problem is the development of the specification of what is to be costed; the actual application of cost factors is the less difficult part of the problem. The military cost analyst plays a useful role in the specifiation process, particularly in regard to ferreting out all of the resources to be required by a program. Since total program cost is the objective, it is necessary to include all of the supporting forces, related procurement items, special training requirements, etc., implicit in the adoption of the force or weapon system. Especially in the area of operating costs is it possible for systems analysts concerned with the overall study to neglect some of the resource implications.

9. An important part of costing is the application of estimating relationships to transform the specification of the ingredients of the force or system in its proper time-phasing into a cost estimate. Estimating relationships are used in such areas as the development of estimates for maintenance workload or training requirements consequent upon the use of major items of equipment and in the translation of workloads to costs. The cost-quantity curve is a particular kind of estimating relationship in which hardware production costs are related to quantity and rate of production. A specific cost estimate may require the use of many relationships, most of which are developed and accumulated over time to be available when needed. A substantial cost estimating research investment is required for the maintenance of a costing capability.

This brief description of military cost analysis should be sufficient to indicate the differences in emphases from the conventional Defense budgeting process. Among the most important differences are: concern in military cost analysis with a force or weapon system rather than with a budget category such as aircraft procurement or depot supply; collection of all costs pertaining to the force or weapon for an extended period of time as against costing for one year; and emphasis on relative rather than absolute accuracy. The budget system highlights categories useful for control of spending (e.g., a military personnel appropriation); administrative aspects receive important attention. Military cost analysis is aimed at providing insight on cost implications of alternative courses of action.

These differences are of sufficient magnitude to explain why military cost analysis developed outside of the Defense Department itself—principally at RAND under the leadership of your chairman, David Novick.

Military cost analysis has not, however, remained independent of

the administrative machinery of the Defense Department. Recognition of the importance of more closely relating the major outputs of defense activity (forces and weapon systems) with resource inputs has led Secretary McNamara to adopt a formal programming system with many characteristics derived from military cost analysis experience.

Chief elements of the programming system are a Defense Department five-year force structure and financial program (in annual obligational authority) expressed in terms of such programs as strategic retaliatory forces and such program elements as the B-52; and systematic review of possible changes using the systems analysis techniques described above.

The programming system is viewed by top management of the Defense Department as the essential link between military planning and budgeting. Mr. Hitch, the Defense Comptroller, has justified this view in these terms: "The job of economizing, which some would delegate to budgeteers and comptrollers, cannot be distinguished from the whole task of making military decisions." He has set the goal of integrating the three related phases of the decision-making process into a single planning-programming-budget system.

The adoption of the programming system has given new stature to military cost analysis. Total program costing as an aid to major decision making is now a fairly well accepted practice in the military departments.

The growing importance of military cost analysis resulting from its diffusion through the programming process has led to greater concern with the validity of the data going into cost estimates. The Defense Comptroller has spearheaded efforts to secure more cost information from contractors and to develop more data within the Defense Department on all aspects of military costs, particularly those relevant to the newer programs. A cost and economic information system has been formally established for the collection and analyses of employment and related economic impact data. The economic information is being collected for analysis of economic impact of procurement by industry and geographic area and for related purposes.

Significantly, the military departments are required under this system to establish cost analysis organizations to organize and manage the program, to insure the validity, comparability, and timeliness of actual cost and related data obtained from contractors, to develop techniques for cost estimating and analysis, to provide a central data storage and retrieval point, to disseminate cost analyses, and to coordinate data and classifications.

Through the establishment of this system, at least a beginning is made toward professional identification of the Defense cost analyst, as

a counterpart to the group of analysts who have been engaged in this activity in such organizations as RAND and the Research Analysis Corporation.

The adoption of the programming process is advancing military cost analysis in still another way. The frequency and volume of data required of the military departments have turned their attention to automatic data processing as a means of making the job manageable. The computer has been applied to many aspects of data preparation and processing. This has furthered orderliness and accuracy in the preparation and updating of basic military data files and has made these files more accessible to military cost analysts.

As the military departments extend the application of computers to costing in the programming process, they come closer to comprehensive force cost models; i.e., systems of relationships and data from which are computed the time-phased resource and cost requirements for the equipping and operating of a force.

Such force cost models for programming will be similar to earlier force cost models for the comparative costing of alternative programs but will retain some points of difference:

First, whereas the comparative cost model is aimed at the identification of the time-phased incremental costs of an alternative program (as compared to the base program), the programming cost model will be aimed at achieving fairly accurate costing of a single official program.

Second, whereas in the comparative cost model, any breakdown of the time-phased incremental cost stream into major program and program element costs is for incidental and secondary purposes, such breakdowns are intrinsic to the purposes of the programming cost model. As a result, problems of joint cost allocation must be faced squarely and solved in the programming cost models.

While military cost analysis has generally gained from the establishment of the programming process in the Defense Department, there is a danger in the formalization attendant upon an administrative process. Interacting to the extent that it does with programming, military cost analysis may find itself unduly influenced toward structuring problems in terms of the classifications of major programs and program elements and the cost categories and five-year periods used by Defense. The efficient use of military cost analysis in studies supporting major decision making requires that military cost analysis remain free to use other classification systems when required by the problem under consideration.

In the last ten years military cost analysis has come a long way. It has achieved a significant role in military decision making. It is all the

more important, as a consequence, that every effort be made to advance the state of the art, that active programs of improvement be pursued in problem areas.

There is, of course, the perennial problem of improving the accuracy of cost estimates. This requires a better understanding of the technology of the systems under study and the degrees of risk involved in achieving the desired physical and performance characteristics. The cost analyst cannot simply accept stated physical characteristics but must also inquire into the cost experience in other systems in achieving similar performance characteristics. Research on costs in direct relationship to performance characteristics is needed for this purpose. Analysts working in areas of advanced technology must devote a considerable proportion of their time to this objective.

A greater effort must be made to convey with the cost figures an understanding of their possible limitations in accuracy. This involves explicit identification by the analyst of the areas of greatest uncertainty and the exploration of ranges of possible costs for the components or other elements in such areas. Presentation to the decision-maker of ranges of costs (and their probabilities where these can be estimated) will afford him a more realistic basis for assessment.

More explicit treatment of uncertainties is particularly helpful when cost differences among alternatives are relatively small and do not really afford a reliable basis for selection.

The degree of technological risk-taking varies from time to time, in consonance with international developments and our position relative to our adversaries. In a period of high risk-taking, cost estimates will obviously be less accurate. In a period of low risk-taking there will be wider application of the program definition phase and other measures to achieve tighter control over procurement. The quality of cost analysis cannot help but reflect these changes in climate.

Military cost analysis is not living up to its potentialities if its role is merely to furnish estimates for alternatives designated by the decision-maker or the systems analyst. A more creative role is generally possible. Through intimate association with the problem and insight into the contributions to cost of the various program elements, the cost analyst is often in a position to generate additional alternatives which are worthy of consideration. A technique useful toward this end is the performance of cost-sensitivity analyses to determine the effect on overall costs of variations in key elements or parameters. Facility in making such cost-sensitivity analyses is aided by computer application.

Aimed as it is at future costs, military cost analysis tends to be largely dollar-oriented. Total program cost is regarded as the common

denominator in comparisons of programs; the real resources of manpower, materiel, and facilities are addressed primarily to get at their dollar cost implications.

In the real world, however, a dollar spent with industry may have significantly different effects than a dollar spent to maintain a man in uniform. The categorization of costs as research and development, investment and operating helps to a degree in discriminating the real resources; it is not, however, the equivalent of an explicit statement of the real resources involved.

The concern with real resources is one of the major determinants of the present appropriation and budget structure, which highlights research and development, procurement, construction, military personnel, and operation and maintenance. Not only is the overall level of the budget of concern to Congress and the Administration, but the distribution by appropriation. Certainly the number of men in uniform is subject to control along with dollars.

It is precisely in reaction to compartmentalization in these terms that military cost analysis owes its origin. Total program cost in all appropriations is the orientation of military cost analysis. It is therefore understandable that military cost analysis would have a high regard for the common denominator, the dollar.

The partiality of military cost analysis for dollar measurement may lead to an underemphasis on real resources in the process of Defense decision making.

The mix of real resources has economic and social implications to which the decision-maker cannot be insensitive. Manpower induction and civilian unemployment are very human results of defense programming and procurement practices.

Recognition of the need for a better understanding of the economic consequences of defense programs is responsible for the establishment by Defense of the Cost and Economic Information System as against simply a Cost Information System. The economic information objective is to collect and analyze employment and related economic impact data, by geographic area and industry.

Adding to the complexity is the need to consider the gold flow consequences in programs involving foreign procurements or deployments. In many cases this imposes a dual measurement requirement.

It is important that military analysis be broad enough to encompass the entire scope of resource considerations, so that the decision-maker can determine on a more objective basis the economic implications involved in the alternatives.

DISCUSSION

JAMES R. SCHLESINGER: Both our speakers have provided thoughtful surveys of two major aspects of defense economics. Praise is undeniably merited, but since the papers speak for themselves, it would be superfluous; so I shall rapidly pass on to aspects of the defense decision-making problem that the speakers have left largely unexplored.

By intent, Robert Grosse limited his discussion to analyzing the cost substructure for military decision making. It is therefore no criticism to remind ourselves that costing is no more than half the job of military choice. Moreover, it is in a sense the easier half, partly because rough quantitative accuracy is more readily obtained in measuring costs than in measuring effectiveness and partly because the most critical problems of choice lie elsewhere. Stephen Enke addressed himself more directly to the problem of selecting the appropriate mix of military capabilities, but he laid stress on the evolution of defense economics, and for this reason drew most of his illustrations exemplifying the utility of analysis from the past. For reasons that I hope to make clear, the problems of military choice in the 1950's were (or were thought to be) simpler than those with which we shall deal in the future. Past achievement in rationalizing military choices was relatively easy to come by—once bureaucratic obstacles could be overcome. There was a fertile field, since military choice previously had been based to an extent that still remains astonishing on intuitions and seat-of-the-pants judgments as structured by both the parochial ambitions and the special insights of the several services. Looking to the future, however, the limitations of cost-effectiveness analyses in making major defense decisions become more pressing—and Enke has made some provocative comments on this subject at the close of his paper. But these considerations raise doubts regarding the future relevancy of a review of past accomplishments. As the domain of systematic quantitative analysis increases and as the opportunity for major savings through the elimination of irrational forms of waste recedes, our attention will increasingly be focused on those aspects of the national security problem that quantitative analysis can in no sense "solve": the problems of objectives, uncertainties, and strategic choice.

Let me give one illustration of the way in which the intricacy of the analytical problems we face has increased, and, therefore, why it is that the kind of accomplishments drawn from the 1950's, with which Stephen Enke illustrated his discussion, are of diminishing relevance. Consider the range of problems brought to the surface by our growing sophistication regarding the character of nuclear war, and, what is to some extent independent, our altering image of our major opponent in the cold war. This has led to a gradual change in what we expect our military capabilities to achieve. Earlier I suggested that conclusions regarding optimal military resource allocation flow most readily when a single dominant objective can be specified. In the 1950's, for better or worse, we thought we could specify such an objective: to wit, the most eco-

nomical destruction, under first-strike and second-strike contingencies, of the maximum number of targets in the Soviet Union. Given such an objective, as Enke's paper illustrates, trade-offs among force components to improve efficiency could readily be specified. In the 1960's, however, I think it has become widely recognized that there is another, perhaps more important, objective for the employment of our strategic power in the event of nuclear war: to limit damage to the United States. Given the enormous Soviet capabilities for damage infliction, this implies a concern to minimize collateral damage in the Soviet Union, thereby providing what we hope is a powerful incentive to the Soviet Union not to strike at American cities in the event of nuclear war. On which objective should we place the most stress: deterring general war or minimizing its destructiveness should it come? Analysis cannot settle this question, nor can it provide precise conclusions regarding the effectiveness of military capabilities under a wide variety of possible contingencies. The whole episode illustrates the way in which strategic objectives can change. But it also suggests a danger in quantitative analysis in that in the search for firm quantitative conclusions, analysis may become frozen to a specific and unchanging objective, because it provides unambiguous results.

The function of the new analytical techniques has not been to give us the correct choice in making military decisions but rather to eliminate some very bad and wasteful choices. In our enthusiasm regarding analysis, we should not exaggerate its accomplishments, actual or potential. Some of the major choices of Secretary McNamara were made when only the scantiest cost data were available and when our understanding of our strategic objectives and of Soviet capabilities remained obscure. In the absence of an analytical framework, some very good choices were made during the Eisenhower years (along with some very wasteful ones). Even with analysis, some rather questionable choices have been made since 1961. Analysis permits us to eliminate choices which were bad in that they invest major resources in capabilities with low effectiveness, but it cannot indicate which path it is best for the nation to follow. Even here analysis is helpful. It permits us to clarify objectives, largely by revealing the costs, in terms of alternative objectives sacrificed as well as in money, of pursuing particular lines of action. But it cannot give us the decision whether to pursue as the national strategy the achievement of arms control, the development of a full first-strike capability, acquisition of balanced forces, or stress on conventional capabilities with heavy reliance upon nuclear firebreaks. These issues cannot be permanently resolved, and this question of objective is one which we should recognize will be permanently in flux. This state of uncertainty regarding our objectives degrades our capability for obtaining quantitatively precise answers in the selection of the appropriate mix of military capabilities. But, it quite appropriately degrades this capability, and we would be in error if we were unthinkingly to settle on a given objective largely because it provided us with precise quantitative estimates. The longer that we have played around with systems analysis, the more aware have we become that what was an objective a few years ago may be turned on its head and become a constraint in this year's model. Such has

been the case, most dramatically, with the transformation of the earlier wartime objective of maximum damage to the Soviet Union in the event of nuclear war into the constraint of accomplishing military objectives with minimum collateral damage to the fabric of Soviet society.

The emphasis that I have placed on the increasing complexity of the demands on the analyst in no way detracts from what the speakers on our panel have said in their papers. Their task was to discuss the techniques of analysis, and they have admirably fulfilled that commitment.

ROLF PIEKARZ: Today's economist is not so much unaware of the technique of cost analysis as he is unacquainted with both the current state of the art and the problems still to be solved.

Cost analysis has been applied to cost-effectiveness studies and program budgeting for four years in the Defense Department and even longer in the "think factories" such as IDA, RAND, and RAC, thanks in large part to both authors and the chairman of this session. Other U.S. government agencies and foreign governments, recognizing the value of these procedures as tools for allocating public funds, are attempting to apply them to problems outside defense economics. Courses, seminars, books and papers on defense economics further testify to the widespread acquaintance with the methodology and theory of these DOD procedures.

By now, discussions of cost analysis should focus on the contributions and limitations of specific techniques for policy planning. Yet the papers by Stephen Enke and Robert Grosse are broad descriptions of the history and methodology of cost-effectiveness analysis and program budgeting as applied to defense decision making.

I am disappointed that no papers have as yet appeared at these meetings to discuss the means by which existing accounting and technological data have been adapted to estimates of marginal costs and marginal products, the data-collection network which has been developed within the Defense Department to obtain the raw materials for marginal analysis, or the techniques applied to estimate costs of new technologies on the basis of experience from past technological change.

The present state of the art and application of economic analysis to defense planning represents only a promising beginning. Data deficiencies stemming from inappropriate accounting conventions (set up solely to reflect legal budget categories and individual military department needs) limit the potential accuracy of analysis, the possibilities for applying refined statistical techniques, and, on occasion, the problems which may be usefully studied. For example, the disability of having to calculate operating and maintenance expenses without regard to the weapons or forces supported still has not been overcome. In many instances, force structure and weapon cost analyses can be based on only the crudest estimates of related operations and maintenance cost. Contrary to Grosse's impression that "the application of cost factors is the less difficult part of the [costing] problem," I personally have found the

problem of estimating such factors at least as difficult as developing my analysis.

The difficulty in relating operating and maintenance expenses to force or weapon systems creates additional problems in trying to solve for minimum costs. As Enke has pointed out, weapons and support resources can be combined in variable proportions to yield a given output. But to determine the least-cost mix requires a more thorough understanding of the relationship between the two than is currently available for many force structure or weapons studies.

Nor are the uncertainties in comparative cost analysis as inevitable as Grosse seems to suggest. They can be attributed not only to such factors as the vagaries of unknown technology, doubts of future enemy capabilities, etc., but also to gaps in our knowledge of the process of technological change in military systems. Past studies, necessarily based on limited experience, have developed useful statistical estimates relating to costs, resource inputs, lead times, etc., involved in the R and D of major weapon components. Nevertheless, a great deal remains unknown about the causes of these statistical results and their sensitivity to diverse circumstances.

Furthermore, as Enke pointed out, cost-effectiveness analysis might be enriched if policy planners had feasible techniques for estimating the effect of present or projected expenditures for weapons and forces on our future military capability. The major factor here is not so much the effect of inherited facilities as the effect of skills and technological knowledge carried over to the new technology. The rapid change in weapons technology makes silos of first-generation missiles of doubtful utility in a third-generation environment. But the experience gained from a first generation of missiles contributes substantially to the performance and resource savings of succeeding generations. Indeed, the rapid technological change in weaponry to an important degree may be attributed to experience with current equipment. However, owing to the uncertain direction of future technology, it is doubtful that the present value of improvement, from current investment, in the performance of later generations of weapons can be determined within a useful range of values under reasonable constraints.

Elaboration and improvement of cost-effectiveness and force-structure analysis are not the only avenues for improving the allocation process within the Defense Department. The contractor-DOD market structure which determines prices and the internal markets and organizations which distribute goods and services among and within the military units, determine the costs of national security as clearly as do the technological relationships among materiel and manpower. The McNamara program to a large degree consists of efforts to improve these management relationships.

Still, there are internal markets where prices do not react to changing supply and demand conditions. Existing price structures which do not consider opportunity costs result in scarcities of some resources and surpluses of others. Higher prices for procurements derive from the lack of sufficient or

appropriate incentives for contractors to seek out the most efficient production techniques. Returns on some capital investments are not maximized because of the absence of appropriate planning or decision criteria.

These problems of molding internal market structures, developing techniques of economic programming, and formulating bargaining strategies are obvious occasions for exercising recent developments in economic theory. It now is up to the economist to take up the challenge.

NONMARKET DECISION MAKING

A THEORY OF BUREAUCRACY

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Introduction

It is ironic that bureaucracy is still primarily a term of scorn, even though bureaus are among the most important institutions in every nation in the world. Not only do bureaus provide employment for a very significant fraction of the world's population (probably over 18 percent of the U.S. work force, for example), but also they make critical decisions which shape the economic, political, social, and even moral lives of nearly everyone on earth. Yet economists and political scientists have largely ignored bureaucratic decision making in constructing their theories of how the world operates.

In this paper, I shall attempt to describe a theory of bureaucratic decision making aimed at achieving such predictability. My theory is based upon the fundamental hypothesis that bureaucratic officials, like all other agents in society, are motivated by their own self-interests at least part of the time. Therefore, this theory follows the tradition of economic thought from Adam Smith forward and is consistent with recent contributions to political science made by such writers as Simmel, Truman, Schattschneider, Buchanan, Tullock, Riker, Simon, and March.

Definitions

Since bureaus are a particular form of organization, the first step is to define an organization: An organization is a system of consciously coordinated activities or forces of two or more persons which has been explicitly created to achieve specific ends.

An organization is a bureau if and only if it possesses the following four primary characteristics:

1. It is large; that is, the highest ranking members know less than half of all the members personally.

2. A majority of its members are full-time workers who depend upon their employment in the organization for most of their incomes.

3. The initial hiring of personnel, their promotion within the organization, and their retention therein are at least theoretically based upon some type of assessment of the way in which they have performed or

can be expected to perform their organizational roles rather than upon either ascribed characteristics (such as religion, race, or social class) or periodic election by some outside constituency.

4. The major portion of its output is not directly or indirectly evaluated in any markets external to the organization by means of voluntary *quid pro quo* transactions.

Each of these characteristics contributes important elements to the particular nature of bureaus. Large size means that bureaus must develop relatively impersonal internal relations and are faced with substantial administrative problems. Full-time employment means that bureau members are not dilettantes but are seriously committed to their jobs. It also implies that the bureau must compete for their services in the labor market. Personnel policies based upon role performance mean bureau members are dependent upon their superiors for promotion rather than upon some outside constituency. Such policies also imply that bureau members are motivated to shape their behavior so as to seek promotions because advancement is dependent upon their performance, not upon innate characteristics like sex, religion, or social caste. Nonmarket orientation means that bureaus are unable to use the objective monetary measure of profitability to evaluate the specific activities they undertake. It also means that even very large profit-making organizations (such as General Motors) are not bureaus, though parts of such organizations (such as the public relations department of Chevrolet) can be bureaus if their specific outputs cannot be evaluated in a market. Some more typical examples of bureaus covered by the theory are the Roman Catholic Church (except for the Pope, who is elected), the University of California, the Soviet central planning agency, the U.S. State Department, the New York Port Authority, and the Chinese Communist army.

Bureaucrats are not simply people who work for bureaus. Rather, I define a bureaucrat as any person who works for a large organization; receives a money income from that organization which constitutes a major part of his total income; is hired, promoted, and retained primarily on the basis of his role performance; and produces outputs which cannot be evaluated on a market. This definition implies that a man can be a bureaucrat even if he works for a nonbureaucratic organization (such as Sears, Roebuck and Company) as long as his own output cannot be evaluated on a market (even though the value of his inputs can be so evaluated). In my analysis, the term "bureaucrat" is in no way derogatory, but because it is so universally regarded as an insult, I will use the more neutral term "official" to describe the type of person defined above.

Central Hypotheses

The theory is based upon the following major hypotheses:

1. Officials (and all other agents in the theory) seek to attain their goals rationally; that is, in the most efficient manner possible, given their limited capacities and the cost of information. This means that all agents in the theory are utility maximizers. In practical terms, this implies that whenever the cost of attaining any given goal rises in terms of time, effort, or money, they seek to attain less of that goal, *ceteris paribus;* whereas whenever the cost of attaining a goal falls, they seek to attain more of it.

2. Officials in general have a complex set of goals, including the following elements: power, income, prestige, security, convenience, loyalty (to an idea, an institution, or the nation), pride in excellent work and desire to serve the public interest (as the individual official conceives of it). However, different types of officials focus on smaller sets of these goals. In particular:

a) *Purely self-interested officials* are motivated entirely by goals which benefit themselves rather than their bureaus or society at large. There are two types of such officials: (1) *Climbers* seek to maximize their own power, income, and prestige. This can be done either by winning promotion to higher rank, increasing the status of their existing positions through aggrandizement, or "jumping" to new and better jobs elsewhere. (2) *Conservers* seek to maximize their own security and convenience. Since "security" is defined as maintenance of one's present level of power, income, and prestige, conservers favor the *status quo.* They fear change because it might reduce their present prerogatives; hence they oppose innovations and change in general.

b) *Mixed-motive officials* have goals which combine self-interest and altruistic loyalty to larger values. The main difference between the three types of mixed-motive officials is the breadth of the larger values to which they are loyal. Thus: (1) *Zealots* are loyal to relatively narrow policies or concepts, such as the development of military airplanes by Billy Mitchell. They seek power both for its own sake and so they can effectuate the sacred policies to which they are loyal. (2) *Advocates* are loyal to a broader set of policies or to a broader organization (such as naval warfare or Harvard University). They are impartial in judging the merits of various proposals within the organization to which they are loyal but highly partisan in supporting that organization in conflicts with "outsiders." The breadth of advocacy can vary widely, from a small section of a bureau (such as the economics department of a university) to a very broad bureau (such as the entire

Defense Department). (3) *Statesmen* are loyal to the nation or society as a whole; hence they resemble the "ideal" officials of public administration textbooks. However, like advocates and zealots, they seek power and prestige for personal as well as altruistic reasons, since they enjoy having an influence upon important policies.

3. The internal structure and behavior of every bureau is so closely related to its interactions with its external environment that neither can be explained without taking account of the other. Hence much of my analysis seeks to show what effects of certain types of social functions and external environments have upon bureaus' internal operations, as well as how their internal operations affect their ability to perform their social functions (the usual approach to bureaucracy).

The Environment

The world in which the officials in my theory operate is as realistic as I can make it. In this respect, it differs sharply from the "perfectly informed" world of traditional economic theory, and more closely resembles the environments assumed by most political and sociological theorists. In particular, the following conditions prevail in this environment:

1. Information is costly because it takes time, effort, and sometimes money to obtain data and comprehend their meaning.
2. Decision-makers have only limited capabilities regarding the amount of time they can spend making decisions, the number of issues they can consider simultaneously, and the amount of data they can absorb regarding any one problem.
3. Although some uncertainty can be eliminated by acquiring information, an important degree of ineradicable uncertainty is usually involved in making decisions.

Insofar as the basic institutional setting of this theoretical world is concerned, it can be either democratic, totalitarian, monarchical, traditionalist, or have any other form in which bureaus are likely to be found. As pointed out by Max Weber, bureaus probably require a money economy rather than a barter economy, but I place no other particular constraints on the type of society to which the theory applies. It is true that many of my examples are drawn from contemporary U.S. society, but that betrays only my own limitations, not those of the theory itself.

A Compendium of Hypotheses

The Nature of the Compendium. The analysis generated by this theory comprises a book of approximately 180,000 words. Unfortunately, condensing it to fit the 3,300-word limit imposed by the Ameri-

can Economic Association is beyond the present state-of-the-art of miniaturization. Since I have already used up about 1,800 words explaining the structure of the model, I have 1,500 left to set forth its applications—or less than 1 percent of the original. Therefore, I will merely present a few of the major hypotheses stemming from the model, denuded of all explanatory material. Some of these conclusions will seem obvious, since they resemble ideas already suggested by other theorists. Others will be more novel. The list will by no means include all the hypotheses arising from my theory, but will range over the various subject areas to illustrate what kinds of things the theory deals with.

"Laws" Connected with Bureaus. Some of my hypotheses appear universal enough to be classified as laws *a la* Parkinson. In order to distinguish them from other such generalizations, I have modestly included my own name in their titles. Hence Downs's Second, Third, Fourth, and Fifth Laws are as follows:

1. *Unchosen but desirable alternatives have value.* For example, many people will pay high rents in order to live near a university, so that they can attend the many cultural events given there—which they almost never actually attend.

2. *Unrestrained conflict at any given level of a hierarchical structure shifts power upwards.* This applies to local governments in metropolitan areas vis-à-vis the federal government as well as within bureaus.

3. *Any attempt to control one complex organization tends to generate another.* Examples are the Bureau of the Budget and the General Accounting Office.

4. *Requests for free services always rise to exhaust the capacity of the agency producing them.* A corollary is that the producing agency will generate "quasi-prices" as a means of reducing the number of requests. These include long delays, demands for favors in return for the "free" service, and requirements for lengthy forms in order to "qualify."

Other Hypotheses. Some less universal hypotheses are as follows:

1. Officials inevitably distort information which they relay upwards to their superiors or downwards to their subordinates. Moreover, under many frequently-encountered conditions, these distortions tend to become cumulative rather than self-correcting as the number of hierarchical levels involved rises. (*a*) If a hierarchy has many levels, the officials at the top may receive a very inaccurate picture of what is actually going on at the bottom—a picture which is overly optimistic and unduly reflects their own preconceptions and policy desires. (*b*) No one person ever knows everything that goes on in any large organization.

2. Officials also distort the orders they receive from their superiors,

interpreting them to their own benefit (or the benefit of their bureau sections or sacred policies) as they develop the implications of those orders for their subordinates. (*a*) If cumulative distortion of this type occurs in a many-level hierarchy, the behavior of low-level officials may consist largely of actions completely unrelated to the goals of top-level officials, or the formal purposes of the bureau. (*b*) No human being can control all—or even a very high percentage—of even the official behavior of a large organization. (*c*) As a result, every top-level official realizes that significant amount of the behavior within his organization is irrational in terms of the organization's formal goals. Hence he fears thorough investigations by outsiders and will go to great lengths to shape his behavior so as not to attract such investigations.

3. Redundant information channels and devices to bypass intermediate levels in the hierarchy are both ostensibly inefficient, but they are necessary so that top-level officials can check on the amount of distortion occurring in "official" channels.

4. Because of the biases of officials who formulate policy alternatives, over any given time period bureaus will tend to select policies which are simpler, more conservative, narrower in their effects, and less cognizant of uncertainty than they would be if officials were unbiased. All of these effects will be accentuated for decisions made under high time-pressure.

5. All bureaus must have hierarchical authority structures, although the "flatness" or "tallness" of such structures can vary considerably. It is therefore fruitless to try to create large organizations wholly free from the tensions which are inherent in hierarchies.

6. Because it is so expensive to change the overall behavior patterns of any large organization, inertia is a rational response to most suggestions of change made to any given bureau. The use of extensive rules is also a rational response to a bureau's problems of decision making and communication. Hence much of the behavior for which bureaucrats are usually excoriated actually represents rational responses to the incentives facing them.

7. Each bureau attempts to stake out, defend, and expand a certain "territory" of policy related to its social functions. Because of numerous technical interdependencies with other bureaus, the boundaries of each bureau's territory are both unstable and ambiguous. Hence it is continually struggling with other bureaus and nonbureau social agents to establish its sovereignty in certain overlapping policy areas. Although such struggles often appear to be irrational manifestations of petty pride and jealousy, they may be highly rational attempts by the

bureau to protect itself from excessive instability in its environment caused by uncoordinated decisions made by other agents.

The desire to avoid such territorial battles may cause bureaus to formulate policies which are socially irrational because they are too narrow in scope. On the other hand, if society does not put strong pressures upon bureaus to produce well-coordinated policies, they may avoid territorial battles by simply paying no attention to each other even though their policies are mutually interdependent.

8. The total amount of government services produced in a democracy tends to be both smaller in quantity and different in quality from what it would be if everyone were perfectly informed. However, this does not necessarily mean that the actual government budget is too small. That budget measures the amount of government inputs. It might be more than enough to produce the required expansion in outputs if people were perfectly informed, since government bureaus would perform far more efficiently under such conditions.

9. It is easier and less expensive to operate a bureau whose members have a high degree of goal-homogeneity than one whose members have highly divergent goals. Therefore, bureaus use selective recruitment, indoctrination, and ideologies to increase the degree of goal-homogeneity among their members. They prefer selective recruitment and ideologies to indoctrination, since it is both risky and expensive to alter the deep-level goals of members. In fact, indoctrination is so difficult that many bureaus avoid social functions requiring really deep-level consensus among their members. Some such functions are therefore never carried out by any bureaus (such as eliminating the "crime syndicate" from the U.S.).

10. Top-level officials of bureaus consider personal loyalty to be an important attribute among their immediate subordinates. Such loyalty is important because every top-level official is required to perform acts which would be extremely embarrassing if made public. Hence he needs subordinates whose discretion he can rely upon.

11. Bureaus have predictable life-cycles, except that once established, they rarely (within a given historical era) die. They come into being through routinization of charisma, splitting off from an existing bureau, entrepreneurial development of a new idea by zealots initially outside any bureau, or creation *ex nihilo* by powerful social agents. As they grow older, they learn to be more efficient, develop more and more extensive rules and regulations, shift their goals from performing their functions well to maintaining their organizational structures, become increasingly subject to inertia, and expand the scope of their functions. As with politicians, few die and none retire.

12. There is a close relationship between a bureau's rate of growth or decline and the quality of its personnel. A rapidly growing bureau tends to attract climbers because it offers possibilities for fast promotion. The presence of many climbers, who are innovation-oriented, increases the capabilities of the bureau; thereby accelerating its growth rate. This attracts even more climbers, which accelerates its growth still further, etc. Conversely, climbers tend to depart from a relatively slow-growing bureau. This leaves it dominated by conservers, who are opposed to change. Hence the bureau lags behind its environment; thereby reducing its capabilities, which slows down its growth even further. This drives out more climbers, reducing its capabilities still further, etc. These relationships cause the leaders of almost all organizations to value growth as a means of attracting and retaining ambitious and competent personnel.

Conclusion. My theory of bureaucratic decision making has two major purposes: (1) to enable us to better understand the operation of bureaus, and (2) to enable us to make more accurate predictions about bureau behavior. I hope the above-described hypotheses convey some idea of the way the theory attempts to achieve these goals. Perhaps it will also help rehabilitate the reputation of those vital but overmaligned pillars of society: bureaus and bureaucrats.

BARGAINING AND CONFLICT SITUATIONS IN THE LIGHT OF A NEW APPROACH TO GAME THEORY

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I

Classical economic theory is based on the assumption that in most economic situations people will act in a rational manner; that is, in accordance with certain consistency requirements called "rationality postulates"—or at least that their deviations from rationality in this sense will not be very important. Economic theory then shows that, as far as a person's actions satisfy the relevant rationality postulates, his behavior will be equivalent to maximizing his (ordinal) utility function. (The rationality postulates needed require that his choices should be transitive and connected and should satisfy an appropriate continuity requirement.)

This concept of rational behavior—assuming that it leads to realistic predictions—is an extremely powerful explanatory principle because it enables us to account for a large number of possibly quite complex empirical facts about people's behavior, in terms of a few relatively simple assumptions about their preferences or equivalently about their utility functions.

This rational behavior concept of classical economic theory, however, fails to furnish sufficiently specific predictions in situations involving risk or uncertainty, such as insurance, gambling, speculation, and in general entrepreneurial behavior; as well as in situations involving strategic interaction between two or more persons or organizations, such as individual and collective bargaining, bilateral monopoly, duopoly, oligopoly, situations involving political pressure and counterpressure, etc.

More particularly, in analyzing the concept of rational behavior, we have to distinguish the following cases:

1. Rational behavior of an isolated individual: (*a*) under certainty (where the outcome of any possible action he may take is fully known to him in advance); (*b*) under risk (where he knows at least the objective probabilities associated with alternative possible outcomes); (*c*)

under uncertainty (where even some or all of these objective probabilities are unknown to him, or where these objective probabilities are not even defined in any straightforward sense).

2. Rational behavior by two or more interacting individuals, where each individual is rationally pursuing his own personal interests (i.e., all his objectives, both selfish and unselfish, to which he assigns positive utility) against other individuals rationally pursing their own personal interests (again both selfish and unselfish)—the basic problem of game theory. For some purposes it is convenient to include also:

3. The rational pursuit of the long-run interests of the society as a whole—the basic problem of ethics,[1] and of welfare economics, which for our purposes is essentially a branch of ethics.

The rational behavior concept of classical economics corresponds to case 1*a* (certainty). It has remained for modern decision theory to develop satisfactory definitions for rational behavior in cases 1*b* and 1*c* (risk and uncertainty), by means of supplementing the rationality assumptions of classical economics by a few additional rationality postulates. The most important additional postulate needed is the Sure-Thing (or Dominance) Principle: If action X cannot yield worse results than action Y but can possibly yield better results, then a rational individual will choose X in preference to Y.

It can be shown that if a given individual acts in accordance with this enlarged set of rationality postulates, then his behavior will be equivalent to maximizing his expected utility[2]; i.e., to maximizing the mathematical expectation of his cardinal utility function. (Unlike an ordinal utility function, a person's cardinal utility function is uniquely determined once a zero point and a unit of measurement are chosen for his utility.)

More particularly, in the case of risk, where the individual concerned knows all the relevant objective probabilities, he will maximize his expected utility as defined in terms of these objective probabilities; whereas in the case of uncertainty, where he has to act without knowing all these objective probabilities, he will maximize his expected utility as defined partly or wholly in terms of his own subjective probabilities (which intuitively can be interpreted as his personal estimates of the objective probabilities unknown to him). This definition of rational behavior, making use of subjective probabilities, is often called the "Bayesian" concept of rationality.

[1] This is true at least under a utilitarian concept of ethics.

[2] For a proof of the expected-utility maximization theorem in the case of risk, see, e.g. [11]. For a proof in the case of uncertainty, see [17] or [1]. The last paper shows that even for uncertainty the assumptions needed do not go essentially beyond a certain form of the sure-thing principle.

II

We have already mentioned that classical economic theory fails to furnish determinate predictions in game situations, at least in those where there is significant strategical interdependence between the various participants' behavior. To take a very simple example, suppose a seller and a buyer try to settle the price of a house by bargaining. The seller's reservation price (supply price) is $20,000 while the buyer's demand price is $30,000. Then classical economic theory can predict only that the price will lie somewhere between $20,000 and $30,000. But it cannot predict where the price will in fact lie between these two limits—indeed it cannot even specify the variables actually determining the price within this range.

Even worse types of indeterminacy arise in slightly more complex situations, such as duopoly, where the space of possible outcomes has more than one dimension. Many eminent economists, starting with Cournot, have tried to obtain more determinate predictions for duopoly situations on the basis of various more or less *ad hoc* assumptions, but it is now generally agreed that their duopoly models were rather unsatisfactory. In particular, though all of them started out with the aim of predicting how two rational duopolists would act in duopoly situations, in fact all of them ended up with attributing some quite unplausibly irrational behavior to their two duopolists. (For example, Cournot's duopolists persist in the same mistaken expectations about each other's behavior, even though their expectations are continually disappointed by the actual events. Moreover, they make no attempt to agree on a collusive solution, even though this would make both of them much better off.)

In retrospect, the basic reason why these distinguished economists did not succeed in their efforts was the fact that they did not have any general systematic theory of rational behavior in (what we now would call) game situations with strategical interdependence between two or more rational individuals. Their example seems to show that it is virtually impossible to develop a satisfactory theory of rational behavior for some specific game situation, such as duopoly, without relying on a more general theory of rational behavior covering a much wider range of game situations.

Yet, in actual fact, the first truly systematic attempt to analyze rational behavior over a broad class of game situations, Von Neumann and Morgenstern's theory of games (1944), has not yielded determinate solutions for the game situations occurring in economics. Their theory does provide a number of very important analytical tools for the analysis of any game situation (e.g., the concepts of expected-utili-

ty maximization, strategy, mixed strategy, coalition, characteristic function, dominance, etc.). It also furnishes a very satisfactory determinate solution for two-person constant-sum games, but fails to supply determinate solutions for two-person variable-sum games and for *n*-person games—though in fact virtually all empirical social situations, including those considered by economics, belong to these two game categories.

However, the present writer has shown [4] [5] [7] [8] [9] [10] that if we add a few very natural rationality postulates to those used by Von Neumann and Morgenstern, then we obtain determinate solutions for all classes of finite games (as well as for infinite games satisfying certain regularity requirements), both two-person and *n*-person, constant-sum and variable-sum, with and without transferable utility, cooperative and noncooperative, etc.[3] The solutions of all these games become special cases of the same general theory of rational behavior in game situations. As a result, our theory provides determinate predictions for duopoly, oligopoly, bilateral monopoly, bargaining, political power situations, etc.—once we make specific assumptions about the participants' utility functions, their strategical possibilities, and the information available to them. Thus it enables us to analyze all these situations in terms of the same general principles, without any need in the various particular cases for arbitrary *ad hoc* assumptions about people's behavior.

III

The basic difficulty in defining rational behavior in game situations is the fact that in general each player's strategy will depend on his expectations about the other players' strategies. Could we assume that his expectations were given, then his problem of strategy choice would become an ordinary maximization problem: he could simply choose a strategy maximizing his own payoff on the assumption that the other players would act in accordance with his given expectations. But the point is that game theory cannot regard the players' expectations about each other's behavior as given; rather, one of the most important problems for game theory is precisely to decide what expectations intelligent players can rationally entertain about other intelligent players' behavior. This may be called the problem of mutual "rational expectations."

Technically, a given player's expectations can be represented in terms of the subjective probabilities he assigns to various possibilities. Thus the problem of rational expectations can also be regarded as a problem of rationally chosen subjective probabilities; that is, as a

[3] Our theory is largely a generalization of earlier work by [13] [14] [15] [18] [20].

problem of deciding what subjective probabilities a given player can rationally assign to different possible strategy choices by the other players, when he knows that these other players are intelligent individuals just as he himself is.

On the fundamental level of analysis, the main novelty of our own approach to game theory is the new solution we propose for this crucial problem of mutual rational expectations (or of rationally chosen subjective probabilities).

Our theory makes use of two classes of rationality postulates in the analysis of game situations. One class consists of postulates of rational behavior in a narrower sense, which deal directly with a rational player's strategy choices in game situations. Intuitively they can be regarded as formalizations of the requirement that, other things being equal, a rational player will always give preference to strategies yielding him a higher payoff, but will be indifferent (and will choose at random) between strategies yielding him the same payoff.

The other class consists of postulates of rational expectations, which deal with rational players' expectations about each other's strategies. These postulates require a rational player not to act on the expectation that another rational player will choose a strategy inconsistent with our rationality postulates. More generally, they require him not to act on the expectation that another rational player will choose a strategy which the first player himself would regard as an irrational strategy (for any reason whatever), and which he himself would therefore never use in a similar case. (In particular, in a bargaining situation a rational player cannot expect a concession from a rational opponent if he himself would refuse such a concession were their objective positions and their utility functions exactly interchanged.)

IV

For lack of space we cannot here describe our rationality postulates in greater detail, nor can we show how these postulates actually allow derivation of determinate solutions for various games. (The interested reader is referred to [3] [4] [5] [7] [8] [9] [10].) Instead, we shall only indicate some of our main results in an informal way.

We need the following definitions. Neglecting intermediate cases, we shall speak of a vocal game if the players are free to communicate with each other, and shall speak of a tacit game if no communication is allowed. A game will be called "cooperative" if the players are free to make enforceable binding agreements as well as other binding commitments; e.g., irrevocable threats. In the opposite case a game will be called "noncooperative." (Our definitions differ from the more usual ones in making the distinctions vocal versus tacit and coopera-

tive versus noncooperative mutually independent, thus permitting consideration of vocal games not allowing binding agreements, and of tacit games allowing binding agreements based on tacit understanding.)

We call a given strategy s_i of player i a best reply to the other $(n - 1)$ players' strategies $s_1, \ldots, s_{i-1}, s_{i+1}, \ldots, s_n$ if s_i maximizes player i's payoff if all other players' strategies are kept constant. We call a joint strategy $(s_1, \ldots, s_n)$ of the n players an equilibrium point if every player's strategy in it is a best reply to the other players' strategies.

We call a joint strategy stable if an agreement by the players to adopt this joint strategy would be self-enforcing, or at least would be enforced and made binding by the rules of the game. In a cooperative game every possible joint strategy will be stable (because all agreements by the players would have full binding force). In a noncooperative game only an equilibrium point can be stable (because agreements will be kept only so long as no player has an incentive to default).[4]

V

In any particular game the players usually have to solve the following problems:

1. The stability problem; i.e., the problem of identifying the set S of all stable joint strategies available to them.

2. The efficiency problem; i.e., the problem of finding the set E of efficient stable joint strategies, defined as those stable joint strategies which could not be replaced by other stable joint strategies to all players' common advantage. Any payoff vector $u = (u_1, \ldots, u_n)$ corresponding to some efficient stable joint strategy will be called an "eligible" payoff vector.

3. The bargaining problem. Different players will have opposite preferences among the various eligible payoff vectors of the game. The bargaining problem is the problem of agreeing on one unique eligible payoff vector u out of the set E^* of all eligible payoff vectors.

4. The strategy-coordination problem; i.e., the problem of agreeing on one unique joint strategy for achieving the payoff vector u already agreed upon. (This is a real problem only in tacit games.)

In cooperative games each player can strengthen his bargaining position by committing himself to use certain damaging strategies, called

[4] In actual fact, being an equilibrium point is only a necessary condition for stability but is in itself not a sufficient condition. If a joint strategy is an equilibrium point, then no player will have a positive incentive to switch over to a different strategy, but he may not have a positive incentive not to do so, either. Therefore, only equilibrium points satisfying certain additional stability conditions will be really stable [8] [9].

"threat strategies," against the other players if no agreement were reached on the payoffs of the game and a conflict situation arose. In an n-person cooperative game he can strengthen his bargaining position also by joining coalitions; i.e., by agreeing to cooperate with some players against some other players in such conflict situations. Therefore, in these games the bargaining problem gives rise to one or both of two further auxiliary problems for each player; viz., the problem of optimal threat strategies and that of optimal coalitions.

In some games there is only one stable joint strategy (e.g., in a noncooperative game containing only one stable equilibrium point). In other games, even though there may be several stable joint strategies, only one of them is efficient, or at least all efficient joint strategies yield the same unique eligible payoff vector. In such games there is no bargaining problem. In all other games the solution of the game depends primarily on the solution of the bargaining problem.

VI

The bargaining problem can be approached from two rather different standpoints. One can ask the ethical question of what particular outcome would represent a "fair" solution (in terms of some moral criteria) [1] [16]. Or, one can ask the specifically game-theoretical question of what outcome will emerge if all parties follow their own interests in a rational manner. Our own analysis tries to answer this second, specifically game-theoretical, question.

Accordingly we shall assume that one player will make concessions to another, not because he feels some moral commitment to do so, but rather because he thinks it would be too risky, from his own point of view, to refuse these concessions. (We do not necessarily assume that the players pay no attention to moral considerations. But we assume that each player's utility function will already take account of any utility he may assign to moral values of any kind.)

Our analysis of the bargaining problem primarily tries to decide under what conditions a given player can or cannot rationally expect another intelligent player to make him a concession. As an answer to this question, our rationality postulates yield a decision rule identical to a decision rule for bargaining behavior first proposed by Zeuthen [20], which we call "Zeuthen's principle." Roughly speaking, the principle asserts that the answer depends on the highest risk (i.e., the highest probability of a conflict) each player would be willing to face rather than accept his opponent's terms. (This highest risk a given player is willing to face can itself be computed on the basis of the expected-utility maximization postulate.)

More particularly, of two players the one more willing to risk a conflict (the one willing to face a higher probability of a conflict) can reasonably expect a concession from the other player, while the one less willing to risk a conflict cannot expect a concession from his opponent and has to make himself a concession if a conflict is to be avoided.

In the case of two-person cooperative games, it can be shown that Zeuthen's principle leads to the Nash solution [3] [13] [15]. As Nash has pointed out, once we have a solution for the bargaining problem, the solution for the problem of optimal threat strategies is quite immediate. Intuitively speaking, Nash's definition for optimal threat strategies represents the best compromise between trying to maximize the costs of a conflict to the opponent and trying to minimize the costs of a conflict to oneself.

As has been mentioned, classical economic theory cannot specify the variables determining the outcome of a bargaining situation. In contrast, the Nash solution predicts that the solution will depend: (1) on each player's attitudes toward risk, as expressed by his cardinal utility function, and in particular on his willingness to risk a conflict rather than accept less favorable terms; and (2) on each player's ability to cause damages to the opponent in case of a conflict, and on the costs to him of causing these damages.

The Nash-Zeuthen theory enables us to avoid various common fallacies in the analysis of bargaining situations. One is the assumption that our opponent will accept any deal so long as this deal would still be better for him than facing a conflict would be.[5] (This would mean that we could reasonably expect all concessions to come from our opponent—as if he were the only one of the two of us who had an interest in avoiding a conflict.)

Another is the assumption that a rational bargainer will never make a threat which would commit him to a conflict strategy damaging not only to the opponent but also to him himself—even though such a threat can greatly improve the terms he can extract from his opponent. (For instance, it has been argued that a union can never rationally commit itself to a strike likely to result in a net loss to the union and its members.[6])

Both fallacies represent misapplications of the utility-maximization principle, whose precise interpretation involves many pitfalls in bargaining situations.

[5] This assumption I have called the "blackmailer's fallacy" [6, p. 74].
[6] For instance [12, Chap. VII].

VII

In the case of n-person cooperative games, our theory leads to different solution concepts, depending on the communication facilities open to the players.

If the communication facilities among the players have no bias in favor of any particular group of players, then according to our theory all possible (2^n-1) coalitions will be operative simultaneously in the game; that is, the members of every possible coalition will cooperate in protecting their common interests against the rest of the players. Hence each player will be a member of a large number of mutually intersecting coalitions and will tend to side with different coalitions on different issues. Thus, our model resembles what political scientists call the "pluralistic" model of society.

Under this assumption of unbiased communication facilities, our theory leads to a solution concept which is at the same time an n-person generalization of the two-person Nash solution, and also a generalization of a modified form of the Shapley value [7] [15] [18].

In contrast, the Von Neumann-Morgenstern theory and many other game-theoretical approaches lead to solution concepts based on the assumption that the players will form two or more disjoint coalitions (each of which may itself again possibly consist of two or more disjoint subcoalitions, etc.). This means that only a small subset of all possible coalitions will be operative in any particular game. Under our theory, such coalition structures can arise only if the communication facilities among the players are biased—in particular, if some players have an opportunity to negotiate a coalition agreement before the other players could make counteroffers to them.

For instance, in a symmetric three-person game with unbiased communication facilities, all three players will obtain the same payoff. Only if these facilities are biased and permit two players to reach an agreement before the third player could intervene will the solution be based on two of the players forming a two-person coalition against the third player and obtaining a payoff advantage over him. This follows from the general principle that any asymmetry among the players in the outcome must be explained by some asymmetry in the antecedent conditions—in this case in the communication facilities open to the players.

Finally, in the case of noncooperative games, the task of the players is to agree by bargaining on one particular equilibrium point satisfying the appropriate stability and efficiency requirements. Here (even if the game is an n-person one) the coalition problem does not arise, and so

the bargaining problem can be analyzed simply in terms of Zeuthen's principle [8] [10].

VIII

We now have to say a few words about the analysis of conflict situations under our theory. In everyday language the term "conflict" may mean one of two things. It may mean a simple absence of cooperation (even in cases where no profitable cooperation is possible between the players at all): in this sense any two-person constant-sum game always represents a conflict situation.

More interesting is the other meaning. Here "conflict" refers to a situation where the players make no use, or make no full use, of some known and mutually profitable opportunities for cooperation. In this sense a conflict exists whenever the players' actual strategies represent an inefficient joint strategy (even though they know they would have some more efficient strategies).

Under our theory, a conflict in this second sense among rational players can arise only in the following cases:

1. In a cooperative game it can arise only if the players insist on mutually incompatible demands as a price of their cooperation—this again can happen only if they do not know one another's utility functions and strategical possibilities (since otherwise they could always avoid making mutually incompatible demands simply by asking for no more than the payoff the solution of the game assigns to each of them).

2. In a noncooperative game the players may have to forego the use of efficient joint strategies also because these strategies may be unstable; in other words, because they may have good reasons not to trust one another to abide by any agreement involving the use of these strategies. This is often called the "Prisoner's Dilemma" case.

3. The players may use what an observer considers to be an inefficient joint strategy; yet from the players' own point of view this may not be an inefficient strategy at all, because their real payoff targets may be different from what the observer thinks them to be. For instance, in a two-person game the observer may think that both players want to maximize their money gains while in actual fact each of them may want to maximize the difference between his own gain and that of his opponent. Thus what may look like inefficient strategies in a non-zero-sum game may be in fact very efficient strategies in the zero-sum game which the players are actually playing.[7]

[7] Though in this case the players' strategies will not represent a conflict in the second sense of the word, they will correspond to a conflict in the first sense. I owe the example to Professor Lawrence Fouraker.

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ENTRY BARRIERS IN POLITICS

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The question, "What is the difference between a political party and a department store?" sounds like a riddle, but I suggest it as a serious subject for inquiry. The two types of organization clearly are very different, and specifying the differences is a good exercise for an economist who is interested in applying the tools of his trade to the study of politics. The subject of this paper came to me while I was engaged in just this exercise (which may or may not be regarded as a recommendation). One of the conspicuous differences is that the department store will normally own its own plant, while the political party does not. The political party may, of course, own some office furniture, but this is only a tiny part of the capital that it will use to serve its customers if it is successful in the competitive struggle. This difference between the political party and the department store is such an obvious one that, like the purloined letter, it is almost invisible. We are also so accustomed to it, that a question as to why it should exist may seem eccentric, but, as I hope to demonstrate, it has an economically interesting answer. This is one field where an essentially political problem can be treated entirely as an economic problem, specifically as an example of the problem of controlling natural monopolies. Looked at in this way, we will see that there is a method of controlling natural monopolies which has not been much discussed by economists but which has been in use by practical men for many years.

Three techniques are normally suggested for dealing with natural monopolies: we can leave them alone, letting their managers do as they wish, we can subject them to regulation, or they can be publicly owned and operated. If you will bear with me in considering the government itself as a natural monopoly, it will be obvious that none of these techniques are suitable for dealing with it. Leaving the monopoly alone, which if we are talking about government means a despotic state, is not at all impossible. In fact, this has been by far the commonest way of handling the problem if we consider the whole of man's history. I suspect, however, that there is no one in this room, no matter how devoted to laissez faire, who will advocate this solution for this type of natural monopoly. In a sense the whole point of democracy is to prevent this sort of "free enterprise."

If the extreme libertarians will not favor free enterprise, it is also an area where devotees of *dirigismé* will not favor controls. It is, of course, a little hard to see who would do the controlling in this case, and who would control the (presumed) monopoly of controlling would be even harder, but I doubt that any democrat, no matter how devoted to planning and controls, would favor controls even if these technical problems could be solved. This is one area where we all favor unrestrained consumer sovereignty. The preoccupation which economists have had since Bentham with improving the information of the parties to a transaction would not be out of place in this area. Efforts to prevent direct fraud would also seem to be sensible, although experience seems to indicate that little can be done on these lines. But these are the same sort of things which we hope for in purely competitive industries, not measures to control a monopoly. The third alternative—public ownership and operation—raises difficult problems of definition. It is not at all clear what a proposal to subject a successful political party to public ownership and operation would mean. The government surely is publicly owned already, and whether it is publicly operated depends upon the definition of public in this particular usage. I am inclined to think that our present set of institutions could best be described as public ownership with private operation.

One of the handy definitions of government is "the monopoly of force." Those of us who have been engaged in what we call synergetics—or the present invasion of political theory by economists—have begun to wonder whether the force really must be monopolized. There seem to be a surprising number of private policemen in New York, and a good deal of our national defense establishment is handled by private companies under contract. It may be that some governmental activities are not so necessarily monopolistic as has been generally thought. Leaving this problem aside, however, clearly a good many of the activities undertaken by governments are natural monopolies. Further, under democratic procedures, the elected members of the government always exercise a sort of monopoly due to the simple fact that there is only one set of them elected. Judge Smith has a monopoly representing Charlottesville, Virginia, in the House of Representatives. A small group of rather estimable gentlemen have a monopoly of governing the city of Charlottesville, and Mr. Johnson has a monopoly of a whole set of governmental activities. The situation is, perhaps, clearer in a parliamentary government where a single party or coalition has complete control over all governmental activities. The natural monopoly here comes from a technological consideration which amounts to a very strong economy of scale: only one majority can exist at a time.

Let us now consider this problem simply as one in economics.[1] Suppose we have an industry, say cement manufacturing, in a small, isolated country in which economies of scale are strong enough so that a company having more than half of the market could operate on a lower cost level than any smaller competitor. Further, to make our analogy complete, assume that the cost advantage would continue up to full market control by one company. If the industry is a vital one where we fear the results of simply leaving it alone, if regulation is ruled out, and if public control is impossible, what can we do?

The problem, of course, is simply an extreme entry barrier. Presumably a competitor could come in and drive the present occupant of the market into bankruptcy by violent competition, but it would be an expensive and risky thing to do. The competitor would have to build a new plant and invest a good deal of funds in the staying alive through the cutthroat competition phase, and he would have no assurance of winning out, let alone making enough from the ensuing monopoly to pay him back for his expenditure. Clearly the company in occupation of the natural monopoly would not be completely free from restraints, but equally clearly only really monumental inefficiency would really much endanger its position.[2]

Looked at from the standpoint of the public of the small island country, this situation would clearly be unsatisfactory, but we have barred them from the traditional remedies. There remains an alternative. They could build or buy a cement plant and periodically put its operation up to auction. In its simplest form this auction could require as a bid simply the price which the entrepreneurial company would charge for cement during the coming year. The lowest bidder would get the right to operate the plant. This, of course, would leave open to the operator letting the plant run down through undermaintenance. We might deal with this problem by having the small country specify the amount and type of maintenance which would have to be undertaken during the year before the bidding. This, however, would involve partial management of the plant by the country, and in any event it is not

[1] Given the title of my paper and the nature of this panel, it would have been impossible for me to conceal the political basis of the problem. In private conversations with several economists I have presented the problem in much the same terms as the next few pages but without giving them any idea of the "industry" under consideration. They have normally accepted the argument and the solution I propose on purely economic grounds. Thus it is experimentally established that the problem can be solved in economic terms even though it arises in political systems.

[2] The analogous problem in economics has received considerable attention recently. Perhaps the best discussion is in Joe S. Bain, *Barriers to New Competition* (Harvard, 1959). More recent work has included William H. Martin, "Potential Competition and the United States Chlorine-Alkali Industry," *J. Ind. Econ.*, July, 1961; Elizabeth Brunner, "A Note on Potential Competition," *J. Ind. Econ.*, July, 1961; Franco Modigliani, "New Developments on the Oligopoly Front," *J.P.E.*, June, 1958.

much like what goes on in the political sphere. Another procedure would be to have the bid include not only the price at which the cement was to be sold but also statements about maintenance, introduction of improvements, etc. The entrepreneurial group who would be permitted to operate the plant would be selected on the basis of a judgment as to which of these rather involved bids was, in all-around terms, the best. Naturally this complicated type of "bidding" raises difficult problems of judgment, but in the governmental case it is hard to see any alternative which is superior. This is, in fact, the type of complex judgment which we normally make in market transactions. In buying a car I cannot simply choose the cheapest; I must balance one package of attributes including a price against another. The choice of the best "bidder" would be similar. The principle difference would be the unenforceable nature of the promises made by the political "entrepreneurs."

Putting the whole transaction in strictly economic terms, we have an industry with an extremely high entry barrier. Public provision of the capital plant artificially reduces that barrier. With the new lower barrier, competition and potential competition will enter much more strongly into the calculations of the present management than it would without this reduction. The small island community could depend upon the cost of its cement being considerably lower than under laissez faire. The problems that would arise would be, essentially, problems of consumer judgment for a complex product and problems of aggregating preferences. The situation would be one of monopoly only insofar as there was a remaining entry barrier even when the plant was provided. Ignoring the latter class of problems for the moment, decisions as to which of the "bids" was best would be similar to ordinary consumer choice in principle, but more complex in practice. A genuine technical judgment on the desirability of the replacement of some given machine in this period or the next would be necessary in some cases. Whether replacement of present equipment by new would be worthwhile would unfortunately be a decision that the "consumer" would have to make. In the recent campaign the desirability of certain capital investments for our military machine was an issue. Further, these decisions have to be made prospectively, not retrospectively. If I am considering buying a Chrysler with a turbine engine, I can at least look at and try an existing car just like the one I would buy. This would not be so with our cement plant if new equipment were proposed. Thus we could and should anticipate less successful judgment of the bids than we find in more normal market situations. This is, of course, realistic. It is true that most people's judgment on whether the

Republicans or the Democrats will serve them best is less accurate than their judgment on the same question about a car they are thinking of buying.

The problems of aggregating preferences simply complicate the situation further. They make the judgment of the consumers even less skillful. The fact that this judgment is a relatively poor one raises some special problems. It is sensible for the entrepreneurs who are bidding for the use of the plant to take the relatively uninformed state of the people who will judge the bids into account. Deception and distortion will be easier under these circumstances than in the marketplace. As one example, let us consider the so-called "going concern" value of the plant. If a new group takes over the plant, it will normally be more efficient for them to retain most of the labor force and lower management rather than hire and train new personnel. If the bids were submitted to highly qualified personnel for judgment, no doubt they would be able to require performance which could only be reached by highly efficient operations, and hence the workers and lower management would be taken over by the new group of entrepreneurs. If, however, the people making the judgment as to which bid was best were not highly qualified—and the voters certainly are not—then various rather inefficient provisions might be inserted into the bid. The successful entrepreneurs might, for example, specify that they would work for a fixed fee instead of for a residual.

This procedure might seem silly, but it is a fact that most elected officials do work for fixed fees. This may be simply due to the difficulty of computing anything comparable to a profit on the government's operations, but it may also be because the voters somehow feel that this is a better system. In many types of government contracts where profit as a residual would seem the obvious choice a fixed fee or contract renegotiation has in fact been adopted, apparently to please the voters. But regardless of the reasons for it, this fixed fee system together with a system of judging bids which is not highly skilled gives the political entrepreneur a motive for behavior which, on the surface, would appear to be simply inefficient. Putting people on the payroll not in terms of their contribution to the enterprise but in terms of their contribution to the entrepreneur would be a quite sensible way of capturing profits not available under the fixed fee system.

The obvious answer to this problem is simply improving the standards by which the bids are judged. If the various entrepreneurs must submit their bids to the scrutiny of real experts (although whether experts are actually ever this good is questionable), they would have to plan on the most efficient methods of management in order to beat out their competitors. Ruling this method out as impractical in a democra-

cy,[3] the only two expedients that seem to remain are simply to let the entrepreneurs do this if it pays them or give the workers and lower management some sort of civil service status. Both expedients have severe drawbacks. Since the disadvantages of a civil service system are, for some reason, normally not discussed, a few words on them might be helpful. The higher management, being unable to discharge the workers without some sort of elaborate procedure, will be less effective. If the civil service system amounts to permanent tenure, as it does in the United States, then the real control that the higher management has over the lower is apt to be much less than optimum. It may, of course, still be better than simply ignoring the problem. The purchase of goods for government use raises somewhat the same problem, although simply requiring bidding may provide a suitable solution.

But to return to the subject of entry barriers and the economics of monopoly, the company now operating the cement plant would have one advantage over any group of entrepreneurs contemplating bidding against them for the next period. They already have a management in existence which has some "going concern" value of its own. The potential competitors will have to establish such an organization simply for the purpose of making their bid. Thus the potential competitor will need to invest some resources into preparing for the competition which will not be required of the present operators. Although the entry barrier has been lowered by the public ownership of the plant, it has not been reduced to zero. This means that the existing management—and the competitors, for that matter—can afford to put their bid somewhat above the "pure competition" level because of this impediment to competition. Indeed, if the profits from managing the enterprise are small, which would be so either if the bid system worked very well or if payment was by a relatively small fixed fee and side-payments were prohibited, then even a very low entry barrier might be sufficient to keep the effective level of potential competition low. In most modern democracies the legally available rewards of office are fairly low, considering the size of the enterprise. This prevents large monopoly profits but also eliminates the normal incentives for efficiency. Under these circumstances competition is needed, not to prevent exploitation, but to keep the management on its toes. Thus we might find these entry barriers still too high and look for an expedient to lower them still further.

Perhaps, however, it might be wise to remind ourselves of the exact

[3] It is not, of course, necessarily impractical for small parts of the government. The government itself must be chosen by the voters, but it might contract for the performance of various services by the method we are here discussing and obtain qualified experts to judge the bids.

problem we are examining. Many governmental services are natural monopolies, and economists are naturally apt to think of them when we discuss problems in this general area. Our monopoly, however, is the government itself, not its constituent services. It is a monopoly simply because we can have only one cabinet, governor, mayor, president, or majority in a legislature. The scale advantage which acts as a barrier to entry is the majority voting rule[4] which provides that the "entrepreneurial group" which obtains half the customers can drive the other entrepreneurial group or groups out of the market. This is the basic reason we must depend upon potential competition rather than upon real competition. This is also the reason why there may well be only two competitors—the present occupant and a potential replacement—instead of many competing "firms." The problem of public policy we are discussing in this essay is insuring that the entry barrier is low enough so that this potential competition is a real restriction on the activities of the present occupant of the monopoly.

The problem is that there may not be any significant potential competition. This fact is not very obvious because we normally think of national politics, and during our lifetime there has always been an active opposition party.[5] If we turn to local politics, however, we will frequently observe a complete absence of organized opposition. Charlottesville, for example, has normally only one serious set of candidates for the city council. The job of city councilman, of course, is unsalaried; so the only attractions are the nonpecuniary rewards of office. I suspect that the pecuniary and nonpecuniary pains of running for office and being beaten are, in net, much greater than the nonpecuniary gains of being a councilman. It is also quite hard for a potential opposition individual or slate to even get his name and platform before the voters, let alone persuading them that it is sensible to vote for him or them.[6] Under the circumstances it is not surprising that there are few opposition candidates.

The obvious way of insuring potential competition in an organized form would be to raise the rewards of office to a level where they would be worth more than the organizational costs of an opposition

[4] If another voting rule is actually used, then analogous problems arise.

[5] The United States has had one of the parties disintegrate, with a temporary cessation of organized potential competition three times: the "era of good feeling," the period just before the foundation of the Republican Party, and 1872 when the Democrats did not nominate an independent presidential candidate.

[6] It might be argued that the communities—and there are many of them—which are governed by such a system are not really democratic. The council is not really subject to any check unless it does something really awful. The councilmen's principle satisfaction comes from "doing their duty." If this theory is true, they would be compelled to give good government, according to their own lights, in order to get the satisfaction for which they hold the office. The system would really be aristocratic government with *noblesse oblige* as the operative motive.

group properly discounted by the risks involved. This might be quite expensive, and democracies have characteristically not followed this course of action. Compensation for electoral success—legal compensation, anyway—has normally been quite modest. Another technique would be to pay a potential opposition group. In the very direct form of the official salary for the leader of the opposition in Britain, this procedure has been rare, but in a more indirect way it is the common system. If quite a number of offices are put up for vote, it is fairly certain, human nature being what it is, that one organized group will not get all of them. The occupants of the other offices will receive compensation[7] and will also be in positions where they can fairly easily make their counterproposals to the government's policies known to the voters. Their actual existence and the practice of public debates in legislative bodies makes it unlikely that the present occupants of the monopoly will behave as though they had a highly secure position. Whether this is a cheaper way of getting a given quality of entrepreneurial ability than offering higher rewards to those in power I cannot say, but it certainly works.

Looked at from this standpoint, the purpose of providing offices for the opposition is to effectively lower the entry barrier by paying part of the organization costs of the potential entrant. Parliamentary debates have the function of simply keeping the existence of a well-organized potential competitor continually before the mind of the government and of providing publicity for the offers of the opposition and government for the next decision period. The system is neat in that no device need be introduced to select the potential competitors who will be supported as opposed to those who will not. The elections select both the government and the opposition. People who do not get elected at all are not supported. Further, the opposition is not motivated to simply settle down to professional opposition because the rewards of office are greater than those of opposition while the costs of trying to replace the present occupants of the monopoly are not significantly greater than the costs of simply holding on.

It is possible to argue that this system would be more efficient if there were more than two competitors. The European systems with many parties give the government to a coalition of parties, and each present member of the coalition competes with each party out of office because it is always possible to make up a new coalition. Whether this really leads to more effective competition, given the fact that the coalitions themselves are the governments while the voters vote for the parties, is not at all certain. In any event, the various systems all do pro-

[7] In Switzerland and a few other places this compensation is purely nonpecuniary.

vide for public support for the organizational costs of a potential competitor and hence keep the entry barrier low. Here is one place where potential competition is obviously highly effective, but only because of intelligent public policy.

This paper has, in a sense, been an exercise in applying economic analysis to a completely noneconomic area. In our universities as they are now organized the subject matter of my paper, political organization, is taught in one department and the techniques of analysis I have employed are taught in another. It is probably for this reason that the rather simple and straightforward line of reasoning I have presented has never before been brought out. In this area, as in so many others, practical men have solved problems that the theorists have not even thought of. It seems to me that this paper is a simple but plain demonstration of the need of some change of our present ways of organizing knowledge. Economists who are interested in politics and political theorists who have a command of economic tools can perform research which is impossible for the more traditionally trained man. This thesis will not be new to the members of this panel, nor, I think, to many of the listeners in this room. I am, in a sense, preaching to the converted, but I am asking you to go out and preach the gospel to all the nations.

DISCUSSION

ROBERT L. BISHOP: In the brief time available, Professor Harsanyi has been able to give us only a general summary of the philosophy that underlies his approach to game theory, together with an outline of the broadened set of rationality postulates on which he relies for determinate solutions to many classes of games that otherwise defy solution. Standing behind this summary treatment, however, are many previous articles and a book-length discussion that is in process. Having read much of Harsanyi's work, I can testify admiringly to the great logical ingenuity that he consistently displays, even though I am far from being a convert to his gospel. Of all game theorists, he seems to be clearly the leading believer in the existence of unique solutions as based on an allegedly unique and compelling conception of rational behavior. By contrast, I must identify myself as at least a skeptic, perhaps even an agnostic.

Harsanyi's approach seems to me to be based on a preconception—really a kind of implicit axiom—that any given game has a unique solution. Then, if it is not solvable with rationality postulates already at hand, new ones are added until they are sufficient for the game in question. This resolution of the problem is then rendered definitive by a kind of terminal axiom that says, in effect, that all other considerations (or variables) are irrelevant. In this manner, what were hitherto just sufficient conditions for a solution become necessary as well; and the possible claims of alternative conceptions of "rationality" are thus effectively dismissed. In my own view, Harsanyi rationality is not so unique or compelling as to be clearly superior to some alternatives, especially in games involving bargaining or "strategic interaction," where the rationality must be mutual.

Only a severely selective sampling of my misgivings about some of Harsanyi's conclusions can be mentioned or illustrated here. Those misgivings are pervasive indeed; and they even include heresy as to that major article of faith among orthodox game theorists, the overriding relevance of Von Neumann-Morgenstern utilities. Thus I would not castigate as irrational a person who deliberately refuses to maximize expected utility, as may well be the case if he finds a utility or disutility in the mere sensation of risk, apart from the utility of the objective outcome of the gamble. To the lottery player or social gambler, for example, there may be utility in the dream of "what might be," even when it does not come true; or, after the fact, there may be a disutility in the regret over "what might have been," which is quite capable of interfering with expected utility maximization before the fact. Even more doubtful, to me, are some of the ways that utility properties are supposed to affect the outcome of Harsanyi-rational bargaining. For example, if two bargainers agree to divide $10 equally, despite an asymmetry of their utility functions, that would violate Harsanyi's canons but not mine. In general, it would not violate my own rather relaxed criteria of passably rational behavior if bargainers seemed to be invoking notions of "fairness" in their negotiations, or

even some improvised notions as to an interpersonal comparison of their utilities (which must be firmly excluded according to Harsanyi).

In noncooperative, variable-sum games there is sometimes a unique, mixed-strategy equilibrium point where, with each person playing his minimax strategy, the payoff of each is the same as that yielded by his maximin strategy. In such cases, Harsanyi prescribes maximin strategies as the rational ones, even though each player would have a better strategy in response to the other's maximin. This seems to me very questionable.

Harsanyi also has a distinctive notion that any n-person game, in which alternative coalitions are relevant, has a uniquely rational solution subject to a specification that all possible coalitions are "operative simultaneously." This is perhaps his most venturesome (and most debatable) gambit in his determined search for a unique solution, no matter what the character of the game. It is interesting to note Professor Tullock's rather casually expressed opinion to the contrary: "Only one majority can exist at a time."

I concluded my discussion in Chicago with a small experiment testing the Harsanyi rationality of some twenty-odd members of the audience. The following double matrix of payoffs in a two-person bargaining game was put on the blackboard:

R_3	5, 2	0, 0	0, 0
R_2	4, 2	4, 4	0, 0
R_1	3, 2	3, 4	3, 6
	C_1	C_2	C_3

Of each pair of payoffs, the first is Row's and the second is Column's. It was suggested that the numbers might be visualized as dollars, but preferably also as utilities (as they could justifiably be interpreted to be if the amounts of money are small enough so that any curvilinearity of the players' utility schedules can be ignored). It was emphasized that Row's three strategies, R_1 to R_3, range from cautious to bold—as do Column's strategies, C_1 to C_3.

Persons in the first few rows on one side of the room were asked to assume the role of Row, and a comparable number on the other side were similarly assigned the role of Column. Although the game was to be played only once as a tacit, two-person affair, it was specified that each person's hypothetical payoff was to be calculated as if he were playing the game simultaneously with each of the players on the opposite side of the aisle.

The reader is invited, as the actual players were, to decide now which R or C strategy he would select. Clearly, each of the three outcomes (5,2), (4,4), and (3,6) is an equilibrium point; and each is also Pareto-optimal. The out-

come (4,4), besides implying equal payoffs, has the "prominence" of a central position in the payoff matrix; and it also yields a payoff for each player midway between the maximum and minimum that he would receive at the other equilibrium points. These considerations happen to qualify (4,4) as the solution under two "arbitration rules" that Raiffa has proposed. On the other hand, the Nash-Zeuthen-Harsanyi solution is (3,6), because the product of those payoffs is maximized. Those payoffs also happen to add up to the largest sum, though this would be irrelevant as far as the latter theorists are concerned.

In point of fact, the subjects at our meeting plumped overwhelmingly for the (4,4) solution. The frequencies of the Row and Column strategies, together with the implied total and average payoffs for each player for each possible strategy, were:

Strategies	R_1	R_2	R_3	C_1	C_2	C_3
Frequencies	0	10	3	0	13	1
Total payoffs	42	52	0	26	40	0
Average payoffs	3	3.71	0	2	3.08	0

I must confess that these results were even more one-sided than I had anticipated. Only one player out of the twenty-seven made the "rational" choice according to the Nash-Zeuthen-Harsanyi theory; and he, choosing C_3, got a zero payoff because not even one Row player chose R_1.

JESSE W. MARKHAM: The three papers before us for discussion range in subject matter from the definition of a bureaucrat to the specification of variables determining the outcome of bargaining and conflict situations. Nevertheless, out of this heterogeneity emerges a single intent on the part of the authors to apply the analytical constructs of the market economy to nonmarket decision making. Harsanyi's effort might be a slight variation on this general theme: game theory was originally applied to nonmarket game situations, later to oligopoly, which I assume qualifies as a market situation, and for some time now has been applied with equal alacrity and agility to such wide ranging social problems as "prisoners' dilemmas" and the design of an optimum strategy for husbands to use against their wives in cases of forgotten birthdays and wedding anniversaries. My comments, addressed to all three papers, are directed much less to what was said than to what was left unsaid.

Since I have already referred to Harsanyi's provocative paper and reside in the undisputed intellectual cradle and possibly still the capital of game theory, I shall begin here in spite of my tremendous handicap of knowing virtually nothing about the subject. Harsanyi asserts that classical economists failed to furnish determinate solutions in game situations and were especially wide of the mark in dealing with the complex problem of duopoly. He charges

Cournot with having purchased determinacy at the high price of attributing implausibly irrational behavior to his duopolists: (1) persistence in the same mistaken expectations about each other's behavior in the face of contrary actual events, and (2) no attempt at collusion when such a course would obviously have made each duopolist better off. Harsanyi overlooked, or at least failed to mention, the Chamberlinian small-number oligopoly model which was successfully designed to overcome both of these weaknesses, although it may have contained others.

But the principal problem I have with Harsanyi's paper is the apparent wide difference between the theoretical potentialities and the demonstrated capabilities of the many variants of game theory to handle market, bargaining, and conflict situations. I shall confine myself to his one practical illustration. Given his situation where the seller's reservation price is $20,000 while the buyer's highest demand price is $30,000, classical economic theory, Harsanyi concludes, can predict only that the price will lie between $20,000 and $30,000. In contrast, game theory provides determinate predictions, and all one has to do is make specific assumptions about (1) the participants' utility functions, (2) their strategical possibilities, which in turn involve assumptions underlying each player's expectations concerning the other players' strategies (which may be represented by rationally chosen subjective probabilities), and (3) the information available to them. Clearly, a game theory model's capacity to predict some unique price between $20,000 and $30,000 depends on the assumptions we make about a wide variety of factors, and it is not obvious that those we make apply equally to all players randomly selected; e.g., their willingness to risk conflict. It is therefore not entirely obvious that game theory permits analysis of all the duopoly, oligopoly, bilateral monopoly, bargaining, and political power situations Harsanyi lists without making certain arbitrary assumptions about people's behavior, the deficiency he ascribes to classical models.

Tullock's main point that the economic concept of barriers to entry can profitably be applied to political situations could, I believe, have profited from the use of some of Harsanyi's analytical tools. His point is that in holding down entry barriers confronting other parties we (the public) secure for ourselves responsible government by the "monopolists" (party in power) through the beneficial pressures of potential competition. Since democratic systems provide for public support of political parties, we have an intelligent public policy that keeps entry barriers low. But does this necessarily follow in cases of two (or more) party systems where barriers may be relative rather than absolute? If voters (or, more correctly, contributors to political parties) reason that contributions to the "in" party yield a bigger payoff (a higher expected utility) than if made to the "out" or "potential" party, the "war chests" of the "in" party may become insuperable barriers whereas a public policy levying a special tax on party contributions may simply make entry difficult but relatively easier.

Downs's paper unfortunately lists without developing the major and minor hypotheses of his theory of bureaucracy, and sets forth the five "Downs's Laws" without deriving their respective proofs. In their abbreviated form

they fall short of attaining their purposes of enabling us better to understand the operation of bureaus and make more accurate predictions about their behavior.

A final comment on the more general problem to which the three papers are addressed. In all societies a choice must be made as to which activities will be assigned to the market for solution and which will be solved by nonmarket criteria. Market solutions are typically imperfect, but when we observe the frightening possible consequences of nonmarket decision making, we are likely to accept the solutions of reasonably competitive markets with renewed faith, and urge, as it seems to me Tullock does, that they be used more extensively and for a wider range of social problems.

ECONOMIC THEORY AND NONPROFIT ENTERPRISE

SOME PROBLEMS IN THE ECONOMICS OF HOSPITALS

By M. W. Reder
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Because I possess neither the factual knowledge nor the expository skill requisite to encompass the subject in the 3,300 words I have been allotted, I shall consider but one of its aspects: the behavioral and policy implications of the fact that hospitals are for the most part nonprofit institutions. Hospitals differ in ownership, degree of specialization, type of care furnished, etc. While my remarks have relevance to other types of hospitals, their immediate reference is to the voluntary, short-term general care hospital. In restricting reference to hospitals of this kind I exclude the government-owned and the proprietary (and presumably profit seeking) institutions; hospitals providing long-term invalid care or care for the mentally ill are excluded also.

In short, the ideal type "hospital" of which I speak is a private nonprofit public service corporation aiming both to be solvent and to promote public health. The hospital attempts to raise capital for expansion and improvement of services mainly from private benefactions and/or governmental contributions of one kind or another; to only a limited extent is it able to earn and retain profits from the sale of its services. In this paper, I shall not presume to recommend specific measures to promote the more efficient utilization of resources by hospitals. Rather, I shall confine my remarks to indicating some of the obstacles to efficient use that are associated with their institutional structure.

The Socially Optimal Stock of Hospital Facilities

Obviously we must attempt to define "social optimum." To the extent that investment in hospital facilities is private there might appear to be no special problem; i.e., endowing hospitals is a form of personal consumption and no more subject to social criticism than any other form. However, public authorities wish to know how may beds are "needed" and what part of this need they "should" supply, and private donors appear to obtain satisfaction by behaving as though they were using their resources so as to maximize some index of public health.

While it would not be easy to specify a public health index and re-

late it to investment in hospital facilities, that task would be far simpler than the one required by the question under discussion (to determine the socially optimal stock of hospitals). This is because it is generally agreed in the literature that the pattern of utilization of hospital facilities reflects a wide variety of influences other than those normally subsumed under the term "medical necessity." It seems quite clear that whether a person is hospitalized depends upon his economic situation, the type of care he can obtain outside the hospital, and the availability of a hospital bed, as well as upon the diagnosis. This is not necessarily a bad state of affairs; it reflects the desire of many doctors to consider the welfare of the "whole patient" and not merely his medical needs—if indeed the two can be effectively separated. But it does create serious actuarial problems for purveyors of hospital insurance and definitional ones for planners seeking a measure of the need for hospital facilities.

As so often happens in matters of serious practical import, lack of a definition has not precluded measurement. A number of estimates of need for hospital facilities has been made; the most commonly suggested target for supply of general beds relative to the general population is 4.5 to 5.0 general beds per thousand population.[1] A more sophisticated approach to the problem of measuring bed needs is taken by Rosenthal who has estimated a demand function for beds from cross-sectional regressions of (several aspects of) bed usage among states upon corresponding state levels of hospital room rates, hospital insurance coverage, income, age composition, sex composition, education, percentage of single persons, etc.[2] From this demand function and a need criterion (i.e., bed supply is adequate only if the probability of all beds being filled on a given day—and presumably of some patients being forced to queue—does not exceed .01) he deduces an estimate of bed demand for each state.[3] Then, by assigning "optimal" values to those variables (e.g., hospital room rates and lack of insurance coverage) that are indicators of economic disincentive to use hospital facilities, he computes from his demand function and need criterion the effective demand for beds that would arise if all states had such low hospital charges and extensive insurance coverage as the most fortunate among them.[4]

Although Rosenthal's work represents an important advance over

[1] G. D. Rosenthal, "The Demand for General Hospital Facilities," Hospital Monograph Series No. 14 (American Hospital Asso., Chicago, 1964), pp. 12-13. Also J. Palmer, *Measuring Bed Needs for General Hospitals: Historial Review of Opinions with Annotated Bibliography* (U.S. Public Health Service, 1956).

[2] Rosenthal, *op. cit.*, Chap. 4 and 5.

[3] *Ibid.*, Chap. 6.

[4] *Ibid.*, Chap. 9.

previous studies, it still does not provide us with an acceptable estimate of the need for hospital beds. The main reason for this is the interrelation of demand and supply for beds in given communities. It is frequently stated, with considerable supporting evidence, that when available hospital beds increase, *ceteris paribus,* doctors tend both to hospitalize patients more readily and also to keep them in for longer periods.[5] In its extreme form, this argument amounts to a Say's Law for hospital beds; i.e., supply creates its own demand. Rosenthal successfully refutes this version of the argument by showing that when the variables reflecting demand are held constant, a pressure index (on hospital facilities) varies inversely with supply (as indicated by beds per 1,000 population).[6] That is, *ceteris paribus,* an increase in supply does not create such an increase in demand as to leave the "pressure index" unchanged. However, this does not cast doubt on the proposition that an exogenous increase in supply would lead to some increase in demand for beds;[7] therefore, we do not have a way of deriving the "medical need" for hospital facilities from observations on their use.

The ambiguity of "need for hospital facilities" stems from the interrelation of demand for hospital facilities and other goods. Under certain circumstances, doctors will prescribe hospital care more readily for poor patients than for rich. One important reason for this is that medical considerations require a higher rate of real per capita consumption by the sick than society is prepared to guarantee to the economically inept when well. To increase the rate of transfer payments to the sick poor, it is frequently necessary to hospitalize them. The extent to which doctors can indulge this proclivity to stimulate income transfers is the greater, the greater the availability of beds for nonacute conditions; hence the intercorrelation of demand and supply of beds.

The Optimal Use of Facilities

The impact of nonmedical factors upon rates of hospitalization obviously may generate overutilization by some patients and underutili-

[5] A good introduction to the literature on hospital use is "Conference on Research in Hospital Use," Jan. 22-23, 1963, Chicago (U.S. Dept. of Health, Education and Welfare, Public Health Service, 1963). Also, see, "Research in Hospital Use: Progress and Problems" (U.S. Dept. of Health, Education and Welfare, Public Health Service, 1962); H. E. Klarman, "The Increased Cost of Hospital Care," in *The Economics of Health and Medical Care* (Ann Arbor, Mich., 1964), pp. 227-54, and M. I. Roemer and M. Shain, "Hospital Utilization Under Insurance," Hospital Monograph Series No. 6, (American Hospital Asso., Chicago, 1957). These sources make extensive reference to the burgeoning literature of the field. Also, "Hospital Utilization Studies: Selected References Annotated" (U.S. Dept. of Health, Education and Welfare, 1962).

[6] *Op. cit.,* Chap. 7.

[7] Rosenthal himself points out (footnote to p. 61) that supply and demand for beds remain substantially intercorrelated after allowing for the effect of all the demand variables.

zation by others. Deciding on what constitutes over and/or under, as distinguished from appropriate, utilization is a difficult matter requiring medical decisions as to the boundaries between appropriate and inappropriate use. Despite the difficulties, a number of studies have been made whose general conclusion is that from 10 to 20 percent of the hospital patients on a given day did not need to be there for medical reasons.[8]

The relevant percentages varied from one study to another for a variety of reasons. In addition to the possibility of medical incompetence and patient chiseling, there is the important element of difference in physician-view as to proper length of stay; this appears to vary considerably from one region of the country to another, as well as between countries, for a given diagnosis. There is also reported a tendency for hospitals with empty beds to pressure their staffs to get them more business.[9] Another cause of overuse is the tendency for partial shutdown of laboratory and surgical facilities over the weekend so that persons entering on Friday sometimes are hospitalized for two unnecessary days.[10] Conversely, patients are occasionally retained until the weekend so that there will be someone to care for them when they get home. However, there is evidence of underuse, also. In the Michigan study of hospital use it was found that while 9.6 percent of the discharges studied were adjudged to represent overstays, 6.8 percent represented understays.[11]

One of the important reasons for the overuse of facilities is the absence of financial incentive to avoid it. When all or most of the patient's hospital bill is paid for by a third party (insurer or otherwise), his own resistance to hospitalization is greatly reduced while the doctor's incentive to hospitalize remains unabated.[12] These incentives are greatly strengthened where a medical plan pays all or part of the physician's fee in the event of hospitalization, but not otherwise. Consistent with these expectations, it has been found in several studies that utilization rates tend to be higher, *ceteris paribus*, where the costs of

[8] See "Research in Hospital Use," *op. cit.*, pp. 5-23, for a brief overview of these studies and Appendix A, pp. 43-47, for a bibliography. Also, "Conference on Research in Hospital Use," *op. cit.* pp. 46-71.

[9] See the remarks of Dr. R. E. Trussel, Commissioner of Hospitals of New York, in "Conference on Research in Hospital Use," *op. cit.*, pp. 70-71.

[10] *Ibid.*, p. 61 and p. 71. Dr. Trussel says, p. 61, on the basis of a statewide survey in New York that "in large hospitals which handle a very large volume of Blue Cross work, the average length of stay in the hospital will vary anywhere from 2 to 4 days longer among people admitted on Friday than for people admitted on Tuesday. The reasons . . . have to do with the general shut-down of activity of the hospital over the weekend."

[11] W. J. McNerney *et al.*, *Hospital and Medical Economics*, Vol. I (Hospital Research and Educational Trust, Chicago, 1962), p. 474.

[12] E.g., often better care can be given in a hospital and also hospital calls take up much less of a doctor's time (per patient) than house calls.

hospitalization are prepaid and also where the fee of the physician is paid by an insurer only if the patient is hospitalized.[13]

The party with the incentive to economize the use of hospital resources is the insurer who bears the burden of overuse. And insurers are extremely alert to the dangers of overuse. On the question of rates, they negotiate quite aggressively with hospitals (see below), but policing overuse raises some very delicate issues concerning physician autonomy. As matters stand, the judgment of a qualified physician in deciding to hospitalize a patient is subject to question only by his peers within the hospital who are extremely reluctant to second-guess one another's diagnoses or procedures. Consequently, hospital administrators cannot bring pressure to bear upon admission policy and, accordingly, third party insurers are unable to press administrators to curb overuse, except in flagrant cases.

Achieving optimal use of hospitals is greatly complicated by the possibilities of substitution among inputs and outputs. For example, medically optimal results might be achieved with less use of hospital facilities if outpatient care were more highly developed, if diagnostic workups could be performed at the clinic or office rather than in the hospital, if nursing homes were more adequate, and if patients' families were more knowledgeable, than otherwise. Modes of treatment that economize on hospital care are usually less expensive than alternatives, but are not extensively developed in many places.[14] Moreover, they place more responsibility on the patient and his family than would be involved if more intensive use were made of hospital facilities, implying greater medical risks.[15] Medical optimality probably implies avoiding the risks and letting the pecuniary costs fall where they may, and it is difficult for the layman—or other physicians—to tell a given doctor how to value life expectancy in pecuniary terms.

Yet another problem of achieving optimal use arises from the fact that the demand for hospital facilities is, in part, a random variable. Therefore the stock of facilities must serve as an inventory, implying that on some occasions there will be idle facilities and on others, queues. The optimum stock must balance the marginal expected social loss

[13] See the remarks of P. M. Densen "Conference on Research," *op. cit.*, pp. 55-59; also McNerney *et al.*, *op. cit.*, Chap. 23. However, there is some evidence that is inconsistent with this hypothesis. H. E. Klarman, "Effect of Prepaid Group Practice on Hospital Use," Public Health Reports, Nov., 1963, pp. 955-65, gives an excellent summary of existing studies in the course of which he points out that two of the most recent fail to bear out earlier findings that prepaid group practice plans have lower hospitalization rates.

[14] Roemer and Shain, *op. cit.*, pp. 20-24. The situation may have improved somewhat since the studies cited by these authors, but I believe the remarks in the text still hold good.

[15] For a specific example, see the remarks of Dr. R. E. Trussel in "Conference on Research," *op. cit.*, p. 44.

from unsatisfied patients against the value of the resources saved by embodying a marginally smaller quantity of resources in hospital facilities. Striking this balance is complicated by the fact that demand is not entirely random but has some price elasticity.

Because of this price elasticity, some allocative improvements might be achieved by charging higher prices in peak-load periods (e.g., midweek) than in others or charging lower prices for persons who will (and can) "stand-by" until space is available. This, in turn, would affect hospital insurance rates, etc., and possibly doctors fees as well. However, the incentive effect upon hospitals of being able to charge higher rates for peak-load periods would be to encourage the creation of "swing" or convertible facilities which would increase the ease of shifting space and related facilities from one use to another, and thereby diminish the probability of bottlenecks.[16]

Efficiency and Cost Differentials

In the profit-seeking sector, economy of resource utilization is achieved through the elimination of high cost producers by those more efficient. Among hospitals this competitive process operates under very severe restraints, if at all. Hospitals cannot be selected on the basis of cost by either patient or doctor; a patient must be hospitalized where his doctor is affiliated. While the elasticity of derived demand may have some effect in leading doctors to affiliate with the optimal hospital (from the viewpoint of their prospective patients), I doubt that it would be seriously contended that this possibility has had much effect upon hospital costs. As a first approximation, I venture the guess that demand for beds in a given hospital is independent of their prices relative to those of other hospitals in the same area.[17]

Since a large part of the nation's hospital bill is paid through Blue Cross and other insurers,[18] the reimbursement schedules of these agencies have considerable impact upon what hospitals can charge (and collect) and indirectly upon costs. Therefore these insurers could, conceivably, bring pressure upon hospitals to lower costs; e.g. reimburse hospitals upon a standard cost rather than an actual cost

[16] On the question of "peak-load" pricing, see P. J. Feldstein, "An Investigation of the Marginal Costs of Hospital Services," Graduate Program in Hospital Administration, University of Chicago, Chicago, Ill., 1961, pp. 65-74. The potential savings from increased use of "swing beds" are discussed by M. F. Long, "Efficient Use of Hospitals," in *The Economics of Medical Care*, *op. cit.*, pp. 211-26.

[17] This statement assumes, of course, that relative prices remain within conventional limits. Obviously for some sets of relative prices, relative demand will respond to relative price.

[18] On July 1, 1963, 78 percent of the U.S. population had some form of hospital insurance.

basis. And in some, perhaps many, cases Blue Cross has attempted to curb cost increases.[19]

But cost differentials are not an unambiguous indicator of differences in efficiency. Although there is some evidence for the presence of *ceteris paribus* economies of scale,[20] it is the larger hospitals that have the widest variety of equipment and services and also the highest costs per patient day.[21] To impose a single rate reimbursement schedule upon all hospitals in a given community or area would, under present circumstances, punish the leaders in medical research and treatment. Of course, this would not be the case if each service were costed separately and priced so as to cover its prime cost. But this is a very big "if." Hospital rate schedules are shot through with intended price discrimination, and eliminating such practices will take considerable time.[22]

Still further complications exist: hospitals produce not only current treatment but also train personnel for the production of future treatment. The costs and benefits of this training to the hospitals providing it are not well known. But irrespective of whether the current costs of training exceed current benefits (to the hospital concerned), they must be met, which introduces an important element of cost differentiation between teaching and nonteaching hospitals. However, it is widely believed that for many types of diagnoses teaching hospitals furnish better care than others; i.e., the higher costs may be justified by the superior treatment.

But who bears the burden of the differential costs of the teaching hospitals, under present hospital insurance plans? Essentially the subscribers who go to "inferior" hospitals but pay the same insurance rates as others. Moreover, the teaching hospitals disproportionately benefit from the services of interns and residents whose wages are far

[19] For example, see H. E. Klarman, *Hosptial Care in New York City* (Columbia Univ. Press, 1963), pp. 426-29; McNerney *et al., op. cit.*, Vol. 2, Chap. 55; H. M. and A. R. Somers, *Doctors, Patients and Health Insurance* (Brookings Institution, 1961), pp. 413-21.

[20] For example, see McNerney *et al., op. cit.*, pp. 785-821; also see the remarks of John Thompson in "Conference on Research in Hospital Use," *op. cit.*, pp. 78-79.

[21] See McNerney *et al., op. cit.*, pp. 785-821. However, P. J. Feldstein, *op. cit.*, Appendix B, especially pp. 62-64, found in a sample of sixty hospitals of varying sizes that average costs fall with size (and marginal costs are constant) without correcting for size correlated variations in output mix. Since such variations almost surely raise the marginal cost of large hospitals relative to small ones, Feldstein concludes (p. 64) that marginal costs would fall with size, "product quality" constant.

[22] It is often alleged that hospitals practice price discrimination, "undercharging" for room and board and overcharging for use of complicated medical equipment. (For example, McNerney *et al., op. cit.*, pp. 921-24.) However, the allegation will not be proved until marginal as well as average costs are compared with prices of various services; i.e., it is not clear that the relatively high priced services are not also those for which relative demand is strongest so that relatively high prices may parallel relatively high ratios of marginal to average costs.

below the market value of their services. Indirectly, and in the long run, this works via the supply of doctors to raise everyone's medical bills. If "exploitation" of medical personnel in training were to cease, the adverse cost differential of teaching hospitals would be even greater, but, in the long run, the cost of physician's services would be lower than at present.

However, the patient himself is an input in the training process. Difficulties have already arisen about the growing shortage of ward patients (the raw material of medical teaching) as a result of the spread of hospitalization insurance. If the patient, too, is to be rewarded in accordance with market criteria, some allowance must be made for the value of his (passive) services in assessing hospital charges.

In short, the ratios of the prices of various hospital services and their marginal costs of production are affected by a number of disturbing forces, and it is not easy to say which services and which hospitals are relatively overpriced by a marginal cost criterion. However, if prices of hospital services are proportional to marginal costs, it is more by chance than intent.

Duplication of Facilities and Efficiency

Because doctors can admit patients into only one or two hospitals, they have an incentive to become affiliated with hospitals that are as fully equipped as possible, so that they may treat hospitalized patients for as wide a range of ailments as their competence (as they judge it) permits. This is especially important for general practitioners, but as equipment becomes increasingly specialized, it may concern specialists as well. Hospitals that wish to attract men of outstanding qualifications to their staffs are therefore impelled to expand the inventory of their equipment and the range of services they are able to offer. This serves to reinforce the usual prestige motives for expansion and improvement inherent in any organization.

These motives operate with special force in the case of hospitals, partly because of the predominance of prestige considerations in the minds of sponsors and partly because of the opportunities and incentives that bear upon hospital managements. A hospital administrator can gain no kudos from paying large dividends; if he succeeds in earning a "profit"—and for him this is not a very important measure of success—all he can do is plow it back. Moreover, his prestige and salary are related to the size, completeness, and modernity (from the medical point of view) of his establishment rather than to any measure of profitability. Hence, the desire to retain "good management" would prompt hospital boards toward expansion if nothing else did. In short,

hospitals of the kind under discussion tend to be run as though their objective was to maximize the weighted number of patients treated (per time period), the "weights" being the professional prestige of the doctors attending them.[23]

Under the circumstances, there is likely to be a strong tendency toward duplication of facilities and generation of excess capacity. This tendency has been recognized and has prompted suggestions for overall regional planning of hospitals. Obviously this raises yet another set of economic problems, but lack of space prevents us from considering them.

[23] This is perhaps a special case of the Baumol sales maximization hypothesis.

ECONOMICS OF THE UNIVERSITY

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There are few other commodities more eagerly sought, more haphazardly selected, more irrationally priced, more undervalued in the act consuming it, more misleadingly represented by some producers, a more enjoyed by the labor force engaged in its production, than higher education. In this economic Wonderland it is not surprising that theory of the university has not yet emerged, for things are only rare what they seem and even the most selective colleges or universities] wail their limitations in detecting skim milk masquerading as cream. is also not surprising that a paper should have been invited on t theme for this particular meeting, for few other economists since V len's delightful polemic on *The Higher Learning in America* ha been more outspoken critics of educational folklore than the dist guished President-elect of the Association. Having had the sig fortune of being his colleague for a decade (but the more dubi fortune of being on the receiving end as one of his deans for the l four of those years), I am perhaps more acutely aware of the pitfa of supposing to be a rational man in an intentionally irrational u verse.

Some of our academic colleagues would deny the relevance of e nomic rationality to such a serious matter as education—economics for the world of wheat, automation, and stock markets, they wo argue, while higher education is the world of humane learning, schol ly inquiry, and freedom of the spirit. The "economics of the unive ty," *they would feel, is in the same category as positing the econom* of the Church. As one of my theological friends once noted: *"After* Judas was the economist among the disciples, and *look how* th turned out!"

Somewhat surprisingly, until very recently economists have al treated the university as sacrosanct and have spent their energies lo ing out through its windows at the rest of the world instead of view their own natural habitat.[1] Perhaps fearing that a closer look wo reveal an Augean Stable, they have spent thousands of man-years a

[1] By contrast the sociologists have been less reticent, beginning with Logan Wils *The Academic Man* in 1942. Among the more interesting studies of the intellectual social environment of academia are Paul Lazarsfeld's *The Academic Mind* (1958), T. (low and J. McGee, *Academic Marketplace* (1958), and David Riesman, *Constraint Variety in American Education* (1958).

lyzing the behavior of business firms, financial entities, governments, households, trade-unions, entrepreneurs, and most any other variety of institution and have scarcely given a thought to that one with which they are most closely and dependently connected. In the light of developments of the last several years, one qualification should be added: we are at last developing a macroeconomic view of education,[2] but we have not even begun to develop a counterpart view of education analogous to the theory of the firm.

Before examining what it is that an economist might say about his own institution, let me suggest why so little has been done in the past. First, as academicians I think we have always felt that the university was misunderstood and undervalued by the world at large, and consequently that an uninformed public was safer than an informed one. We have carried this distrust of knowledge and distaste for public debate so far that we are content to be uninformed about our own institutions. Some professors even today believe that A.A.U.P. Committee Z has been too zealous (a few presidents think that is what the "Z" stands for) and has somehow committed a breach of good manners by discussing mundane salary matters in public.

Second, I believe many of us—perhaps less so economists than philosophers or classicists—fear that to study the university as an economic institution may ultimately result in the wholesale application of business procedures and principles to education. In an age of mass education and the burgeoning "multi-versity" there is a real danger of developing inflexible bureaucratic rules and formulae; and the scholar has some justification in holding to "a little rationality is a dangerous thing" philosophy.

Third, most university administrators in the seventy-five years ending with the recent war—and many even today—feel that matters of university finance, salaries, and even teaching loads are none of the faculty's business. This play-your-cards-close-to-your-chest attitude may arise for one of two reasons: because the president views himself in the role of employer and the faculty as employees, or because the president sees himself in the role of mediator, and to reveal too much to the faculty will commit him to a similar clean breast with the board of control. The latter view is certainly preferable to the former, although neither fosters informative research on the institution. It should be added, however, that the president may in effect be right (even if for the wrong reason), for too much information can fan the rivalries and

[2] I refer here to the seminal work of Schultz, Harris, and Machlup among the more senior members of the profession, and of Becker, Bowen, and Weisbrod among the younger members.

petty jealousies which seem to breed particularly on college and small university campuses.

Finally, for a variety of reasons, existing data on educational institutions, even were it all to be freely available, is extremely poor. University accounting procedures are devised, not to show the health of an institution, but to satisfy trustees and government auditors. Because universities rely on the largesse of philanthropy or state legislatures for capital needs, not only accounting provisions for replacement needs but the whole concept of depreciation and obsolescence tend to be absent. One ironic result is that despite fifteen years of persuasive argument the universities have never been able to convince the Congress that 20 percent overhead on grants and contracts is not pure profit, while an identical project with a business firm would safely allow 30 percent to 50 percent. The Office of Education has collected educational statistics for a century but cannot even provide information on the number of full-time-student equivalents in higher education today, much less the kind of cost data which would be of primary use to economists. Despite the rapid and continuing expansion of college enrollments, no one has done a real study of incremental costs, comparing, say, expansion of existing institutions versus the founding of new ones. One other example suggests the shocking state of the arts in analyzing the market for academic personnel. Most everyone has been making dire predictions about the shortage of faculty trained at the doctorate level. The N.E.A. studies since 1953 have indicated a sharp reduction in the percentage of new faculty with the Ph.D., and the Office of Education, the Fund for the Advancement of Education, the graduate deans, and some of the President's most important advisory groups have estimated the large cumulative deficits in the production of Ph.D.'s. Recent studies now suggest that over the last decade the percentage of teaching faculty holding the doctorate has actually risen in the four-year colleges and universities from approximately 44 percent to 51 percent. There is much in our own house to put in order, and we may be in the embarrassing position of having cried wolf when it was only a tabby cat, and thus being friendless if a real wolf appears.

Turning to the individual institution, the closest analogy one can draw between a university and a business firm is to picture the former as a firm producing multiple products. This "firm" operates under conditions of variable proportions, with cost interdependencies producing a strong incentive for vertical integration, particularly in a backward direction. That is to say, once one is committed to the production of Ph.D.'s, cost factors necessarily impose the adoption of undergraduate programs producing baccalaureates. The reverse has not been true

in the past, for liberal arts colleges have not been driven by economic factors to integrate forward into university status. Prestige factors have occasionally brought this about, and judging from the postwar success of major universities in raising private endowments and attracting massive federal support, there may be emerging long-run economic factors which will entice more frequent attempts at forward integration. The movement of such colleges as Wesleyan, Dartmouth, Lawrence, and Reed towards full university status may indicate a new trend.

The evidence of the pressure for vertical integration is that no major graduate school today is without an undergraduate base with the single exception of the Rockefeller Institute (which is removed from financial constraints with an endowment equal to approximately $1 million per student). Clark, Johns Hopkins, and Chicago at one time or another attempted to be purely graduate institutions, but without success. It is self-evident that the additional investment and incremental costs of adding undergraduates to a graduate establishment are relatively low. Only minor additions are needed to library and laboratory facilities, dormitories are largely self-financing and not even necessary in an urban setting, and courses can be predominantly taught by advanced graduate students and junior nontenure faculty. To use an example on the last point, in 1962-63 instruction in the Faculty of Arts and Sciences at Harvard was provided by 1,103 individuals, 27 percent of whom were tenured faculty members, 21 percent were instructors and assistant professors, and 52 percent were part-time instructors. (These are numbers of persons, not full-time equivalents.)

The incremental cost of adding graduate programs to an existing undergraduate college are extremely high—or stated more correctly, are extremely high if quality programs are to be offered. Judging from the rash of institutions now beginning doctoral work, this is not self-evident, and forward integration is proceeding on some campuses despite the economics of the case. It is one thing for Michigan State, Duke, or Brandeis to become full-fledged universities overnight with fully adequate resources at hand; it is quite another for Melrose College and Omaha Normal (I hope these are fictitious names) to initiate doctoral programs willynilly. To cite one example which should give pause for thought, there are today at least fifteen or twenty universities offering the Ph.D. in a variety of fields whose library holdings are less than 150,000 volumes—barely a creditable collection for a good small liberal arts college.

I once attempted a rough cost estimate for a private university which indicated yearly costs of approximately $900 for the first two undergraduate years, $1,300 for the upper-class years, and $2,700 for

the graduate school. This report is probably still gathering dust in the files of some academic vice-president, for his reaction was: "Most interesting, but for Heaven's sake don't show it to the President or the Board!" Cost figures for a major state system a year or two ago were, if I remember correctly, $32 per credit hour for freshmen and sophomores, $45 for juniors and seniors, and $162 for graduate education. These are at least suggestive of differential cost levels and should raise two other warning flags. If we did, indeed, have accurate cost studies, I doubt we should any longer be able to tell the private university undergraduate that he pays only 35-40 percent of his educational costs. Tuition may be that proportion of total university expenditure, but I suspect the undergraduate comes close to paying the entire educational and general expense attributable to undergraduate education. And second, if the university costed out the graduate school of arts and sciences as it frequently does its schools of medicine and law, the deficit might be a source of extreme delight to our medical and legal brethren. If my suspicions are correct, the president who jealously protects the innocence of both his faculty and his board of control by permitting only fuzzy consolidated financial reports to see the light of day begins to take on the characteristics of wisdom and sanctity.

The economist is unlikely to make much headway in cost studies of individual institutions without the full support of the administration. Can he nevertheless make a contribution in studying such problems as optimum size, the relationship between cost and quality, the market for academic personnel, etc.? There are many fascinating questions in this realm, and issues of public policy demand answers even if imprecise ones. There are currently a number of centers where economists are engaged in research into various aspects of the economics of education: Schultz and Bowman at Chicago; Weisbrod and Hansen at Wisconsin; Bowen and Machlup at Princeton; Fein and Rivlin at Brookings; Hirsch and other at U.C.L.A.; Becker at Columbia; Harris at San Diego; and others too numerous to name. In the four short years since Schultz's presidential address to the A.E.A. meeting in St. Louis, the economics of education has become a popular enough subject to run the danger now of becoming fashionable.

To add one more center, such an interest has developed even in the citadel of college and university presidents—the American Council on Education. Our work to date has focused chiefly on the university sector, with particular attention to graduate education. Some preliminary research results may be of interest, since they bear closely on some of the above questions.

Members of the audience may have participated in a major survey conducted last spring evaluating quality in graduate education. Profes-

sors in thirty separate disciplines in the 106 largest universities were asked to rate the quality of graduate faculty in their respective fields in each institution, and also to judge the effectiveness of doctoral programs. We were fortunate in receiving nearly an 80 percent response to the 5,400 questionnaires. Although detailed results will be published next summer, a summary of findings for the most prestigious economics departments is given in Table 1.

TABLE 1

RANKING OF QUALITY OF FACULTY FOR LEADING ECONOMICS DEPARTMENTS, SPRING 1964

RANK	INSTITUTION	Ranking by: Department Chairmen	Junior Scholars	Senior Scholars	East	Midwest	South	West	Expert Panel
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1	Harvard	1	1	1	1	1	2	1	3
2	M.I.T.	1	1	2	3	2	1	2	1
3	Chicago	5	3	3	4	5	3	4	5
4	Yale	3	3	5	2	4	5	5	1
5	California (Berkeley)	7	5	4	5	3	4	3	6
6	Stanford	4	6	6	6	6	7	6	4
7	Princeton	6	7	7	7	7	6	7	7
8	Michigan	9	8	9	9	9	9	8	8
9	Columbia	9	9	8	8	12	8	11	11
10	Wisconsin	8	9	10	10	8	13	9	13
11	Minnesota	13	11	11	13	11	11	12	9
12	Northwestern	11	13	12	11	13	12	10	11
13	Carnegie Tech	12	12	14	11	10	10	19	9
14	Pennsylvania	14	15	13	14	15	16	13	13
15	Johns Hopkins	15	14	15	15	14	14	14	15
16	U.C.L.A.	19	16	16	18	16	15	15	15
17	Cornell	16	18	17	16	21	17	16	17
18	Duke	17	20	18	17	20	18	17	18
19	Purdue	17	19	19	18	17	26	17	20
20	Michigan State	21	23	22	24	19	25	20	24
173	Number of respondents in each category	41	59	73	65	49	35	24	13

One hundred seventy-three out of 219 economists receiving the questionnaire returned usable replies. Table 1 lists the first twenty departments in order by quality of faculty. Columns 3, 4, and 5 indicate the rank resulting from the judgments of department chairmen, distinguished senior scholars (who have never served as chairmen except in a temporary acting capacity), and knowledgeable junior scholars not more than ten years beyond the receipt of the Ph.D. Columns 6 through 9 show rankings as seen by respondents currently employed in four regions. Column 10 indicates the judgment of a small select panel of experts, including some past and present officers of this Association,

editors of major economic journals, and others recognized for their scholarly attainments.

I am aware of the limitations of such subjective evaluations—that they are based only partly on first-hand knowledge, are influenced by hearsay, incorporate halo effects, are subject to time lags, etc. As one of our respondents quoted Dr. Johnson, "A compendium of gossip is still gossip." Despite some very real limitations, however, I believe such evaluations more clearly reflect reputation and accomplishment than do such "objective" measures as number of books in the library, Nobel laureates on the faculty, Woodrow Wilson fellows enrolled, or dollars of research grants from government or foundations. The present study was designed to answer some of the criticisms of Keniston's study in 1957 and Hughes's studies in 1924 and 1934. Keniston used only chairmen, and Hughes used small select panels; the reader of the A.C.E. study will be able to give any weighting he wishes to the eight various subsamples.

Among the more interesting conclusions is the close correlation of ratings among the three ranks (particularly of the junior and senior raters), and the variance with the fifteen-member experts' panel. More detailed analysis of absolute scores than shown here would characterize the easterners as being uncharitable to everyone, including many of their own institutions. The westerners and southerners are most charitable to everyone, although most noticeably to themselves. The midwesterners play out their role of middlemen, showing the least regional bias. Most respondents also leaned over backward in rating their present university attachment and their doctoral institution, rating their current department on the average 17 percent higher than outsiders did, and rating their doctoral alma mater 9 percent above the evaluation of non-alumni. If one were to assume that these differences reflected bias, not better informed judgments, and corrected for them, the top of the list would read M.I.T., Harvard, Yale, Chicago, etc.

Table 2 summarizes the results of a second question on the effectiveness of the doctoral program, where the respondent was asked to take into consideration the accessibility of the faculty, library and research facilities, quality of other students, the intellectual climate, curriculum, etc. In effect this asked where the informed respondent, knowing what he now knows, would go to study economics if he were starting over again. The good small departments in private universities generally moved up the scale and the larger departments slipped. Chicago and Columbia show the most marked differences, for reasons which I shall let others interpret.

Table 3 compares the 1964 findings on faculty quality (combined scores) with the earlier Keniston and Hughes rankings. Whether or

TABLE 2

TOP RANKING ECONOMICS DEPARTMENTS

Institution	Rank According to "Effectiveness of Doctoral Program"	Rank According to "Quality of Graduate Faculty"
M.I.T.	1	2
Harvard	2	1
Yale	3	4
Stanford	4	6
California (Berkeley)	5	5
Princeton	6	7
Chicago	7	3
Wisconsin	8	10
Michigan	9	8
Northwestern	10	12
Carnegie Tech	11	13
Minnesota	12	11
Johns Hopkins	13	15
Columbia	14	9
Pennsylvania	15	14
Duke	16	18
Cornell	17	17
Purdue	18	19
U.C.L.A.	19	16
North Carolina (Chapel Hill)	20	21

TABLE 3

LEADING ECONOMICS DEPARTMENTS IN 1924, 1957, AND 1964

	1924*	1957†	1964‡
1.	Harvard	Harvard	Harvard
2.	Columbia	Chicago	M.I.T.
3.	Chicago	Yale	Chicago
4.	Wisconsin	Columbia	Yale
5.	Yale	California	California
6.	Johns Hopkins	Stanford	Stanford
7.	Michigan	Princeton	Princeton
8.	Pennsylvania	Johns Hopkins	Michigan
9.	Illinois	Michigan	Columbia
10.	Cornell	Minnesota	Wisconsin
11.	Princeton	Northwestern	Minnesota
12.	California	Duke	Northwestern
13.	Minnesota	Wisconsin	Carnegie Tech.
14.	Northwestern	Pennsylvania	Pennsylvania
15.	Stanford	Cornell	Johns Hopkins
16.			U.C.L.A.
17.			Cornell
18.			Duke
19.			Purdue
20.			Michigan State

(Lines indicate a shift of more than two positions in rank order.)

* From Raymond M. Hughes Report to the Association of American Colleges, Jan., 1925. The results are summarized in *American Universities and Colleges*, 1st ed. (American Council on Education, 1928), and in the Keniston report.

† From Hayward Keniston, *Graduate Study and Research in the Arts and Sciences at the University of Pennsylvania* (Univ. of Pennsylvania Press, 1959), Appendix.

‡ From Table 1 above.

not one agrees with the precise rankings, the noticeable movements appear to be in keeping with what one knows of changes over time. Hopkins has declined relatively, particularly since 1957. M.I.T. has soared to the top. Columbia has gradually lost its commanding position of the 1920's, and Wisconsin is completing a cycle by climbing again after a sharp drop in the postwar period. (Yale can be thankful that no rating was done in the 1940's.) Most interesting is the relative stability of the top departments—a phenomenon peculiar to economics alone of the major disciplines studied. Between 1924 and 1957, Illinois dropped out of the first fifteen and Duke was added. In 1964, two institutions not included in previous studies, M.I.T. and Carnegie Tech, placed high on the list, but the same fourteen survivors of the "Class of '24" are still among the first seventeen (U.C.L.A. has bumped Duke for inclusion). By contrast, in a field such as botany only seven of the original fifteen are still among the leaders today.

How can we make such quality ratings more than an interesting compendium of gossip? An economist might ask: What is the cost of quality? What are the economic constraints on improving quality? What degree of concentration exists in output, research support, fellowship funds, etc.? What is the relationship between quality and size? Is the concept of optimum size relevant to higher education? One brief paper cannot answer adequately these and the many other interesting related questions, but a few tentative conclusions will be suggested, and others will be included in the later publication of the study results.

First, the subjective ratings indicated above are closely correlated with the quantity of scholarly publications originating in the various departments. Figure 1 compares for economics the index of quality with a publications index. The latter is based on a count of all articles, communications, and book reviews appearing in six major nonspecialized economics journals and all books reviewed or noted in the *American Economic Review* during the period January, 1961, through June, 1964. Weights were assigned to various types of publications, counting a substantive book as 10, 3.3 for a textbook, 2.5 for an edited volume, 2.5 for substantive articles, 1 for notes and communications, and .5 for book reviews. In the case of joint authorship these scores were divided evenly among authors. Scholarly output, as measured by "article equivalents" per year in this index, is highly concentrated in a few institutions; the ten most productive departments accounted for 56 percent and the top twenty-five departments for nearly 90 percent of all publications reviewed. At the other extreme, about twenty-five of the seventy-two Ph.D. granting departments placed no more than a single article in these journals, and nearly a dozen departments accounted for not more than one book review in nearly four years. Even
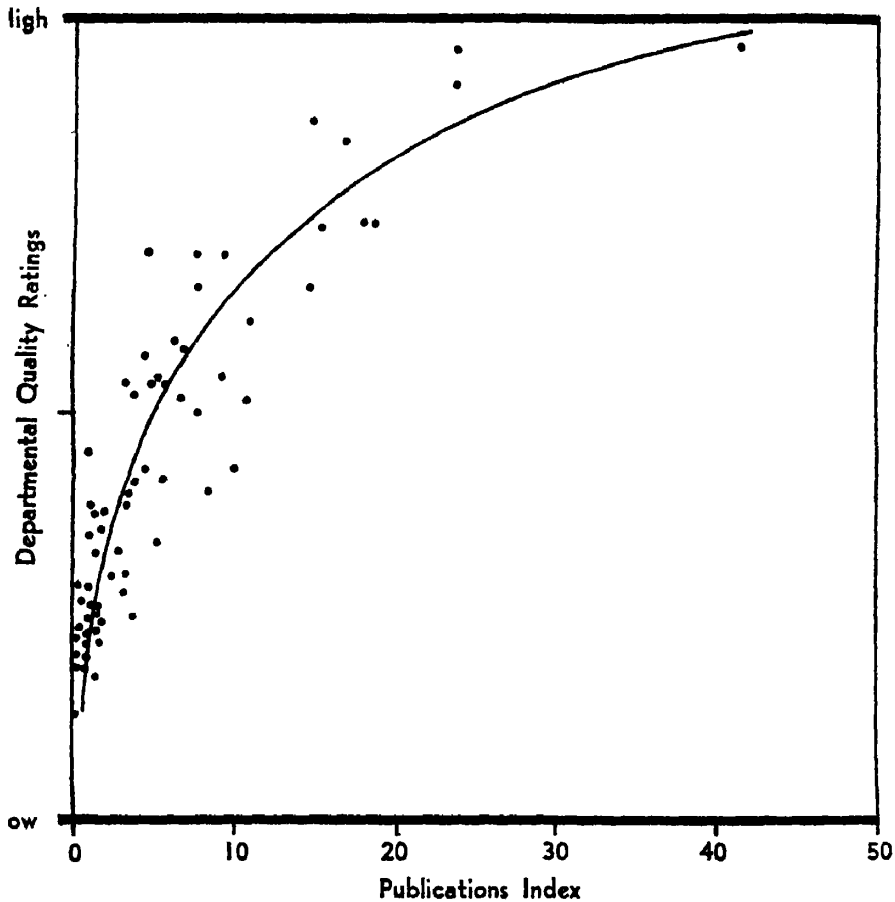
FIGURE 1
PUBLICATIONS INDEX AND QUALITY OF GRADUATE FACULTY FOR
72 LEADING ECONOMICS DEPARTMENTS, 1964

llowing for the fact that it is easier to place an article in a reputable ournal if one's address is Cambridge or Berkeley, the poorer depart- ients were so unproductive that the President-elect of the Association, ere he to be treated as a department all by himself, would have ranked hirty-seventh.

This evidence may lend support to Stigler's Law, which maintains iat there are not more than fourteen really first-class scholars in any eld of study.[8] The market being what it is, no single department can fford to hire them all, just as no single museum can afford to purchase ll the Rembrandts. A department building around scholar number fteen cannot hope to achieve distinction, says George Stigler, and the

[8] George J. Stigler, *The Intellectual and the Marketplace* (Free Press of Glencoe, 1963).
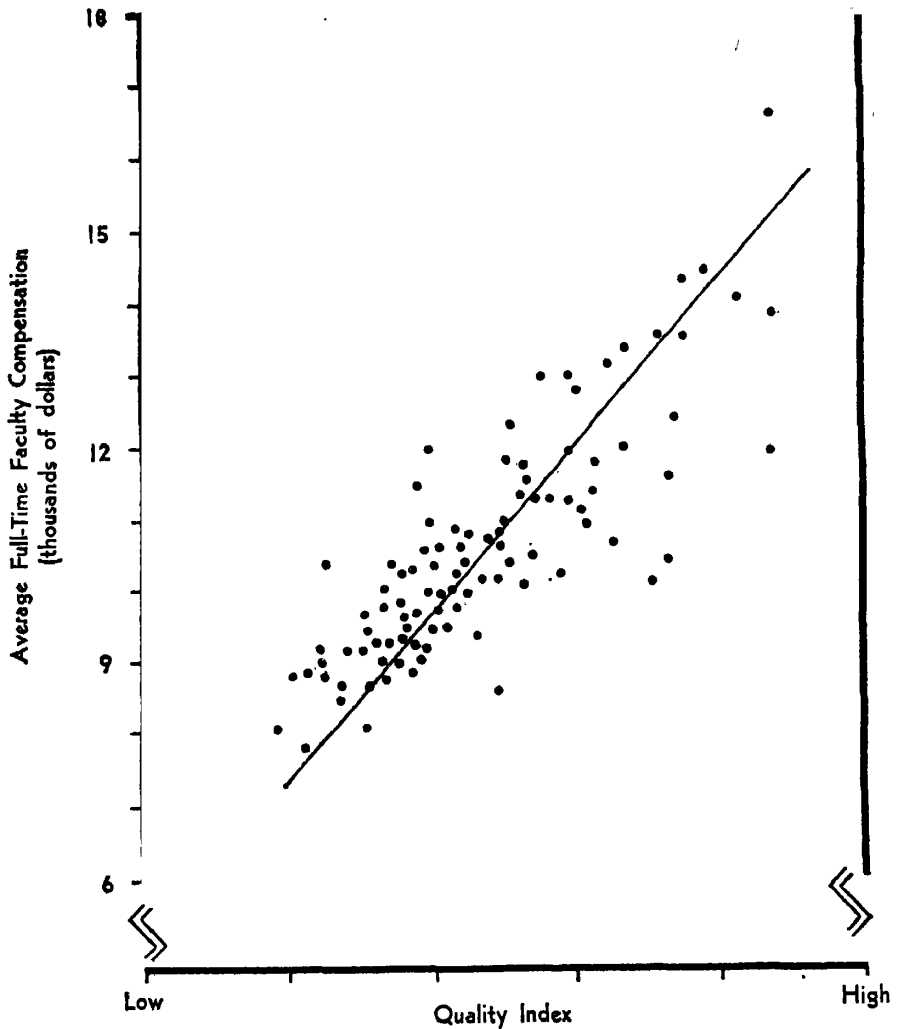
FIGURE 2
AVERAGE FACULTY COMPENSATION (A.A.U.P.) AND QUALITY OF GRADUATE FACULTY OF 97 LEADING UNIVERSITIES, SPRING, 1964

first fourteen men distribute themselves so that there are normally only a half dozen or so really outstanding departments. Fortunately, in our study Chicago emerged third in faculty quality, thus lending comfort to Stigler's plea for "selective eminence," and credence to the objectivity of economists (most particularly since only 7 percent of those participating in the ratings are present faculty or former Ph.D.'s from that eminent institution).

A second conclusion I would offer is that quality is expensive, and that it is most closely correlated with the level of faculty salaries. Fig-

ure 2 illustrates this relationship by comparing average quality ratings for ninety-seven universities reporting A.A.U.P. faculty salary data. The coefficient of correlation is .872, and if we were to measure the elasticity of quality in response to salary level, the scatter would suggest a moderately high elasticity at low salary levels, diminishing as both salaries and quality rise.

Third, although there are obvious quality advantages associated with size, the actual correlation between the two is rather poor. Two scatter diagrams indicate this, Figure 3 plotting annual doctorates awarded in economics during 1953-62 against quality rating, and Figure 4 plotting overall quality with total doctorates awarded. Economics is a particularly interesting case, since six institutions among the top half of the quality list produced less than ten Ph.D.'s in the last decade, while nine departments with twenty-five or more doctorates awarded in the decade were on the bottom half of the list.

Fourth, and perhaps most interesting, Figure 5 relates educational and general income per full-time student "unit" to overall size of institution.[4] For the top twenty-five universities in terms of quality there appear to be marked economies of scale; the scatter for all other universities in the study exhibits an almost precisely horizontal line of best fit. The latter supports the hypothesis that higher education, for the typical institution, tends to exhibit constancy of costs over a wide range of scale. The interesting study made by Russell and Reeves thirty years ago[5]—one of the few early educational studies which asked the kinds of questions an economist would like to pose—is probably still valid today, indicating that liberal arts colleges experience significant economies of scale up to an enrollment of about 1,000 to 1,500 and thereafter costs remain almost exactly constant per unit for any particular level of educational quality.

The theory of the firm is traditionally two dimensional, dealing with quantity and cost (or price) assuming either a standardized product or a clearly differentiated one with a separate identifiable demand. Higher education pretends to produce certain standardized commodities (e.g., B.A.'s M.A.'s Ph.D.'s) but in fact qualitatively exhibits wide range and variety. Qualitative measures are becoming more common today in higher education, as studies of student test scores, fellowship data, institutional finance, and subjective ratings proliferate. These open new and interesting avenues for the economist in the study of higher educa-

[4] Full-time undergraduate students are the unit of measure. Graduate and professional students were counted as three units, and part-time students were converted to full-time equivalents by multiplying by .4.

[5] John Dale Russell and Floyd W. Reeves, *The Evaluation of Higher Institutions*, Vol. VII, *Finance* (Univ. of Chicago Press, 1935).
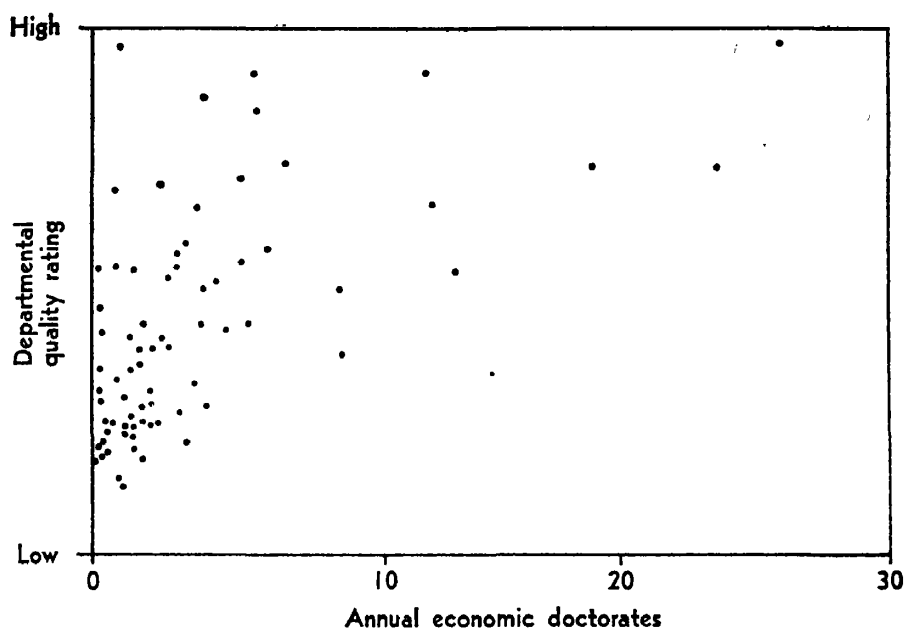
FIGURE 3
AVERAGE ANNUAL DOCTORATES AWARDED IN ECONOMICS (1953-62) AND SUBJECTIVE QUALITY RATINGS (1963-64) OF 70 LEADING GRADUATE ECONOMICS DEPARTMENTS
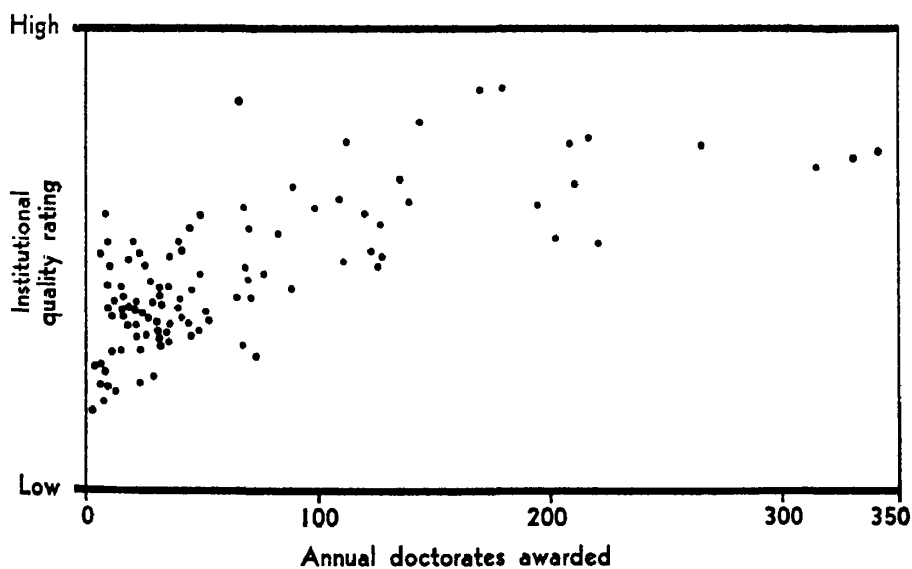
FIGURE 4
AVERAGE ANNUAL DOCTORATES AWARDED (1953-62) AND SUBJECTIVE QUALITY RATINGS (1964) OF 103 LEADING GRADUATE INSTITUTIONS
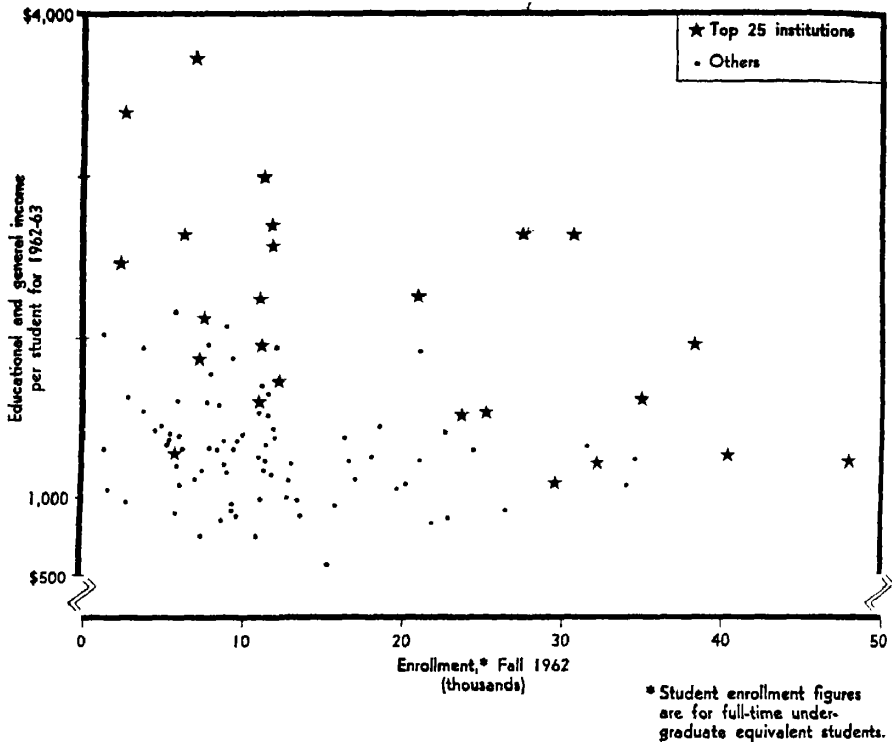
FIGURE 5
EDUCATIONAL AND GENERAL INCOME PER STUDENT BY SIZE OF ENROLLMENT

tion and may indeed help us to develop a more sophisticated economics of educational institutions. Howard Bowen's Committee of this Association looked at cost and size a few years ago.[6] I have concentrated on the relationship of quality to some cost factors (i.e., salaries) and to size as represented by degree production. Perhaps the study I have reported on above will be a useful tool in developing a more relevant three-dimensional model.

The present paper has hardly more than scratched the surface, but I hope it will encourage others to undertake research on that overlooked but fascinating institution—their own university.

[6] Report of the A.E.A. Committee on Graduate Education in Economics, *A.E.R.*, Sept., Pt. 2, 1953.

ON THE PERFORMING ARTS: THE ANATOMY OF THEIR ECONOMIC PROBLEMS*

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I. *The Setting*

Romanticism long ago fixed in our minds the idea that there is something inevitable about the association between artistic achievement and poverty. The starving artist has become a stereotype among whose overtones is the notion that squalor and misery are noble and inspiring. It is one of the happier attributes of our time that we have generally been disabused of this type of absurdity. We readily recognize that poverty is demeaning rather than inspiring—that instead of stimulating the artist it deprives him of the energy, time, or even the equipment with which to create or perform.

While we have come to accept the idea that artists are often impecunious, even a cursory encounter with the facts of the matter usually proves surprising. One may or may not see something shocking in the fact that the median total income in 1959 of males classified by the census as actors was $5,640; that for musicians and music teachers the comparable figure was $4,757; and that for dancers and dancing teachers, $3,483.[1] But one must recognize that these figures include income from all sources, some of them (e.g., truck driving, lobster fishing, waiting on tables) rather unrelated to the performer's art.[2]

A detailed and specific investigation of economic conditions in the performing arts was conducted by Senate and House Committees in 1961 and 1962, and the volumes of *Hearings* which resulted are very

* This paper is based on a study being prepared by the authors for the Twentieth Century Fund, through the administrative channel of Mathematica. The study is still in progress, and this paper is nothing more than a brief introduction and a statement of certain theoretical ideas. The Fund has facilitated our work, not only by making generous financial provision for the extensive job of data collection and analysis which has been necessary, but also by helping to secure the cooperation of organizations and individuals and by allowing us full freedom to proceed as we wish. In the volume which will emerge from this study, we shall acknowledge our debt to the many people whose patient assistance has been essential to our work.

[1] U.S. Bureau of the Census, U.S. Census of Population: 1960, Subject Reports, *Occupational Characteristics,* Final Report PC(2)—7A, Table 25.

[2] As the Department of Labor's career guidance publication stresses: "Many performers . . . supplement their incomes by teaching, and thousands of others have to work much of the time in other occupations." (U.S. Dept. of Labor, Bureau of Labor Statistics, "Employment Outlook in the Performing Arts," Bulletin No. 1300-65, 1961, p. 214.) The BLS goes on to warn: ". . . the difficulty of earning a living as a performer is one of the facts young people should bear in mind in considering an artistic career" (*loc. cit.*).

revealing.[3] At that time the minimum weekly salary for Off-Broadway actors was $45 per week (it is currently $60); and what makes this figure significant is that most Off-Broadway actors are at the minimum. In such circumstances it is not difficult to see why Joseph Papp, producer of the New York Shakespeare Festival, was able to report that "banks and landlords consider him [the actor] a credit risk without visible means of support."[4]

Mrs. Helen Thompson, of the American Symphony Orchestra League, presented figures indicating that in 1961-62 the average salary paid to musicians in the twenty-six major orchestras in this country was $4,512; if the four highest-paying orchestras are excluded, the average for the remaining twenty-two major orchestras (again as of 1961-62) falls to $3,500.[5]

Dancers are in even worse financial circumstances, as illustrated by the case of a leading modern dance company whose members normally receive $25 after a trip which frequently includes four days of travel, a day of rehearsals, and a public performance.

In the main, performing artists are employed by organizations—by orchestras, opera and dance companies, producers and impresarios, resident theater companies—and the underlying economic pressures which manifest themselves in low performer salaries are transmitted through these organizations. Inadequate financial flows to these groups can threaten not only the welfare of individual performers but also the very existence of the institutions serving the entrepreneurial and managerial functions in the field of the performing arts. And, notwithstanding the publicity that has been given to the alleged "cultural boom" in America, we continue to hear frequently of theatrical groups which collapse, of opera houses whose seasons are in danger, and performing arts organizations of all kinds for whom financial emergency seems to have become a way of life. It is this situation and the threat that it poses for the cultural prospects of our society which constitutes the setting for the study we have undertaken.

The first objective of our study is to explain the strained economic circumstances which beset performing companies, to determine whether they are attributable mainly to fortuitous historical circumstances, to mismanagement or poor institutional arrangements, or whether there is something fundamental in the economic order which accounts

[3] U. S. House of Representatives, *Hearings Before the Select Subcommittee on Education of the Committee on Education and Labor,* "Economic Conditions in the Performing Arts," 87th Cong., 1st and 2nd Sess., 1962 (cited hereafter as "House Hearings"). U. S. Senate, *Hearings Before a Special Sub-Committee of the Committee on Labor and Public Welfare,* "Government and the Arts," 87th Cong., 2nd Sess., 1962.

[4] House *Hearings,* p. 111.

[5] House *Hearings,* p. 47.

for these difficulties. On the basis of our analysis we hope to produce some conditional forecasts of the financial future of the performing arts, the prospective costs, the operating revenues likely to be associated with various levels of activity, and the proportion of the resultant financial gaps which one can expect to be met from current sources of contributed income.

This session is intended to deal with theoretical matters and, while much of our work has been empirical, we welcome this opportunity to try to describe the basic economic relationships which seem to us to underlie the financial problems of the performing arts.

II. *Basic Economic Characteristics of Nonprofit Organizations*

Before we turn to the special economic properties of the performing arts, it is useful to devote some discussion to the economics of nonprofit-making organizations in general, for only in this way can the difficulties which beset the performing arts be seen in perspective.

Nonprofit organizations as a group share at least two characteristics: (1) they earn no pecuniary return on invested capital and (2) they claim to fulfill some social purpose. These two features are not wholly independent. Any group which sought to fulfill no social purpose and earned no financial return would presumably disappear from the landscape. Moreover, its goals themselves often help explain why no money is earned by such an organization. While an automobile producer may take pride in the quality of his cars, he is much less likely to regard product quality per se as an ultimate objective of the enterprise than is the head of a nonprofit organization. Nor is the auto producer likely to be nearly as concerned about the social composition of his clientele.

The significant point is that the objectives of the typical nonprofit organization are by their very naturé designed to keep it constantly on the brink of financial catastrophe, for to such a group the quality of the services which it provides becomes an end in itself. Better research, more adequate hospital facilities, more generous rehearsal time, better training for those engaged in these activities—all these are not merely incidental desiderata. They are fundamental goals in themselves, and with objectives such as these, the likelihood of surplus funds is slim indeed. These goals constitute bottomless receptacles into which limitless funds can be poured. As soon as more money becomes available to a nonprofit organization, corresponding new uses can easily be found, and still other uses for which no financing has been provided will inevitably arise to take their place. Any lively nonprofit organization always has a group of projects which it cannot afford to undertake and for whose realization it looks hopefully to the future. Once

this fundamental fact is grasped, it is hardly surprising that such groups feel themselves constantly strapped. It becomes clear that they are simply built that way.[6]

Nor is it just through its quality aspirations that the social goals of the nonprofit enterprise contribute to its financial difficulties. The concern of the typical nonprofit organization for the size and composition of its clientele often causes operating revenue to be lower than would be the case if services were priced to satisfy a simple profit-maximization goal. Since such a group normally considers itself to be a supplier of virtue, it is natural that it should seek to distribute its bounty as widely and as equitably as possible. The group is usually determined to prevent income and wealth alone from deciding who is to have priority in the consumption of its services. It wishes to offer its products to the needy and the deserving—to students, to the impecunious, to those initially not interested in consuming them, and to a variety of others to whom high prices would serve as an effective deterrent to consumption. In short, a low price for the product of a nonprofit group is normally an inevitable consequence of its objectives, and indeed sometimes becomes an article of faith. The ancient doctrine of "just price" is imbedded in the operations of these groups and carries with it all the difficulties which inevitably accompany an attempt to put it into practice.

The desire to provide a product of as high a quality as possible and to distribute the product in a manner other than that which maximizes revenue combine to produce a situation which is unusual in yet another respect. For such an enterprise a substantial increase in the demand for its product may well worsen the organization's financial health! Marginal costs may well exceed marginal revenues over the relevant interval. An increased number of student applications, an increased number of hospital patients, an increased number of orchestral performances may well increase the size of the contributions required for solvency. More generally, it follows that, contrary to widespread impressions, the much publicized cultural and educational "booms," whatever their composition, may in many cases prove a very mixed financial blessing.

Yet even in such circumstances the organizations cannot simply refuse to expand their activities in response to an increase in demand. By such a refusal the organization would renege on its fundamental

[6] The fact that any nonprofit organization can always find uses for a temporary excess of funds—and indeed may be embarrassed to report to its contributors that it has some money left at the end of the year—makes it very difficult to determine its cost functions. If an auto producer finds that a sudden increase in demand has swollen his receipts, he is only too happy to report higher profits; a nonprofit enterprise, however, may well use the extra revenue in a way which, in effect, deliberately raises it costs.

objectives, and, incidentally, it might even produce a loss in private and community support.

III. *The Performing Arts in Particular*

It is apparent that all of the standard problems of nonprofit organizations which have just been discussed beset the performing arts. It is not surprising, therefore, that the survival of the great majority of its organizations requires a constant flow of contributions. We can then easily understand why the arts find themselves in their present unhappy financial circumstances. But, up to this point, our discussion has offered no portents for the future. Here we don the inherited mantle of the dismal scientist and argue that one can read the prospects of the arts tomorrow in the economic structure which characterizes them today. The evidence will suggest that the prospects offer no grounds for complacency—that there are fundamental reasons to expect the financial strains which beset the performing arts organizations to increase, chronically, with the passage of time.

To understand the prospective developments on the cost side, it is necessary to digress briefly and consider in general terms the implications of differential rates of growth in productivity within the economy for the relative costs of its various outputs.[7] Let us think of an economy divided into two sectors: one in which productivity is rising and another where productivity is stable. As an illustration, let us suppose that where technological improvements are possible they lead to an increase in output per man-hour of 4 percent per annum, but that output per man-hour remains absolutely constant in the stable productivity sector. If these sectors are assigned equal weights in the construction of an economy-wide productivity index, the aggregate rate of increase in output per man-hour will be 2 percent per annum. For the moment let us assume that there is only one grade of labor, that labor is free to move back and forth between sectors, and that the real wage rate rises *pari passu* with the aggregate rate of change of productivity, at 2 percent per annum. Finally, let us suppose that the money supply and the level of aggregate demand are controlled in such a way that the price level is kept stable. Assuming that there are no changes in the shares of capital and labor, this means that money wages will also increase at the rate of 2 percent a year.

The implications of this simple model for costs in the two sectors are straightforward. In the rising productivity sector, output per man-

[7] There is, of course, nothing new in the following observations on the effects of differential rates of productivity change on costs and prices. See, e g., Tibor and Ann Scitovsky, "What Price Economic Progress?" *Yale Rev.*, Autumn, 1959. Only its application to the state of the arts is novel.

hour increases more rapidly than the money wage rate and labor costs per unit must therefore decline. However, in the sector where productivity is stable, there is no offsetting improvement in output per man-hour, and so every increase in money wages is translated automatically into an equivalent increase in unit labor costs—2 percent per annum in our example. It should be noted that the extent of the increase in costs in the stable productivity sector varies directly with the economy-wide rate of increase in output per man-hour. The faster the general pace of technological advance, the greater will be the increase in the overall wage level and the greater the upward pressure on costs in those industries which do not enjoy increased productivity. Faster technological progress is no blessing for the laggards, at least as far as their costs are concerned.

It is apparent that the live performing arts belong to the stable productivity sector of our economy. The legitimate theater, the symphony orchestra, the chamber group, the opera, the dance—all can serve as textbook illustrations of activities offering little opportunity for major technological change. The output per man-hour of the violinist playing a Schubert quartet in a standard concert hall is relatively fixed, and it is fairly difficult to reduce the number of actors necessary for a performance of *Henry IV*, Part II.

Moreover, from the standpoint of long-term developments, the essence of the matter is not absolute or relative levels of productivity at a given date but the rates of change in productivity over time. This means that even if the arts could somehow manage to effect technological economies, they would not solve their long-term cost problem if such savings were once-and-for-all in nature. In order to join the ranks of the rising productivity industries, the arts would somehow have to learn not only to increase output per man-hour but to continue to do so into the indefinite future. Otherwise, they must at some juncture fall behind the technologically progressive industries and experience increases in costs which stem not from their own decisions but from the inexorable march of technological change in other parts of the economy.

True, some inefficiencies of operation are to be found in the field, and their elimination can help matters somewhat. Moreover, performing arts organizations can reduce the rate of increase in their unit costs by permitting some deterioration in the quality of their product—by fewer rehearsals, the use of more poorly trained performers, shoddy costumes and scenery. But such a course is never popular with organizations dedicated to quality, and, furthermore, it may lead to loss of audience and community support. Nevertheless, it is not an uncommon

"temporary" expedient, imposed by the realization that the cutting of corners may be the only alternative to abandonment of the enterprise.

There is one other important avenue for cost saving open to the performing arts which has so far not been considered. We refer to wages paid performers. In the simple model sketched above, we postulated a situation in which a single, market-clearing wage was paid to all persons regardless of the industry in which they were employed. In actual fact, the live performing arts constitute a rather special labor market—a market in which the need for great native ability and extensive training limits the supply, but in which the psychic returns to those who meet these tests often offers a very substantial inducement to remain in the field. For these reasons, the performing arts are relatively insensitive to general wage trends, especially in the short run. It is largely for this reason that performing arts organizations in financial difficulty have often managed to shift part of their financial burden back to the performers—and to the managements, who also are generally very poorly paid by commercial standards. The level of the incomes in this general field must be considered remarkably low by any standards, and particularly so in light of the heavy investment that has often been made by the artists in their education, training, and equipment. And it is surely explained at least in part by the willingness of those who work in these fields to sacrifice money income for the less material pleasures of their participation in the arts.

However, there are limits to the financial sacrifices society can extract from the performers in exchange for psychic returns. One may reasonably expect that rising incomes in other sectors will ultimately produce untoward effects on the supply of talent. At what point this will occur depends partly on the income elasticity of the demand for psychic income. As the general level of real income rises, it may well be possible to persuade performers to accept a lower relative position in the income hierarchy. However, there are symptoms which suggest that, in some specialized areas, effects involving both quantity and quality are already being felt, though, overall, excess supply continues to be one of the market's most notable characteristics.

In sum, the cost structure of the performing arts organizations promises them no easier future. One might anticipate, therefore, that this structural problem would produce discernible effects on pricing policy. Certainly, in most of the industries in which productivity is stable, we would expect the price of the product or service to rise relative to the general price level. And there is a widespread impression that the arts have indeed behaved in accord with this anticipation—that ticket prices have been soaring. Yet our preliminary evidence suggests

strongly that this view is incorrect and is largely a product of money illusion. Indeed, our preliminary data indicate that the rate of increase of ticket prices has barely managed to keep up with the price level and has lagged substantially behind increases in costs.

One might undertake to account for the surprisingly modest rate of increase in ticket prices in terms of a revenue maximization model—on the hypothesis that arts organizations believe the demand for their product to be highly elastic. We suspect, however, that a more valid explanation is the role of a doctrine of just price in the objectives of these organizations.

The tendency for increases in prices to lag behind increases in costs means simply that arts organizations have had to raise larger and larger sums from their contributors—and our analysis leads us to expect this trend to continue. Thus our analysis has offered us not only an explanation for the current state of affairs; it has also provided us with a basis for speculation about the future. What it has shown will not, we are afraid, be reassuring to those to whom ready availability of the arts constitutes an important objective of society. If our model is valid, and if, as may be suspected, there are limits to the amounts that can be obtained from private contributors, increased support from other sources will have to be found if the performing arts are to continue their present role in the cultural life of this country and especially if it is intended that they will expand their role and flourish.

DISCUSSION

MANUEL GOTTLIEB: Dr. Reder has shrewdly analyzed many of the "obstacles to efficient use" of resources administered by the quasi-public nonprofit, voluntary short-term, general-care hospital. These obstacles are reviewed under four broad headings: (1) tendencies to overutilization of hospitals by physicians seeking to indulge clients or to facilitate their private practice at the cost of public funds administered by insurance agencies with little or no control over utilization; (2) tendencies to inefficient administration of resources within hospitals due to prevailing hospital-sponsored schemes for prepayment with cost-plus pricing at "usual billings"; (3) excessive concentration on high-cost inputs and outputs because few hospitals provide—and few insurance schemes cover—a broad spectrum of medical care including facilities for outpatient care or diagnosis or specialized convalescence facilities; and (4) lack of proper investment and size criteria for individual hospitals with tendencies toward overcapacity and duplication of expensive facilities acquired with little regard to overall community-wide benefit-cost study.

If time had permitted, other inefficiencies could have been added to this damning indictment of the central institution of the American health care scene: the general private hospital. Medical staff specialists rendering anesthesia and pathological and radiological services have been given a free hand to financially exploit hospital patients and medical-care funds by their use of hospital facilities and staff with the privilege of private billing. And hospital administration has been torn apart and disrupted at its source by the dual control stemming, on the one hand, from the so-called "medical staff," made up in large part of high-status private practitioners oriented to their private practice, and the salaried hospital staff which administer nursing and housekeeping and other functions. A subordinate line of dual control stems from the division of the medical staff into teaching interns and residents oriented to their work of apprenticeship and the outside practitioners. Effective teamwork, unified administration and good work morale are essential preconditions of efficient economic organization and may outrank in importance brick and mortar investment and elaborate equipment.

These striking inefficiencies of resource use and organization by no means follow, as Reder alleges at the outset of his paper, as "implications" of the nonprofit character of the organization in question. The inefficiencies are for the most part by-products of the current institutional situation in medical care in which essentially public functions are being discharged by private or quasi-public groups with abundant opportunities for gratification of private group interests and with weak mechanisms for coordination and planning. At the core of this situation is the organized medical profession, dedicated to private solo fee-for-service practice and autonomous hospitals drawing large support from public funds but with weak boards made up of community notables. This institutional situation contrasts strongly with medical care facilities in other countries of the Western world or with a considerable number of

group-care schemes in which practitioner, insurance agency, and hospital are integrated under a single control. What is needed is a systematic comparison of hospital administration and resource-use in these various kinds of institutional schemes.

If the Reder paper did not adequately develop the implications of the nonprofit character of the American general hospital, neither did the Baumol-Bowen paper adequately develop the "basic economic relationships which seem . . . to underlie the financial problems of the performing arts." Surely, the absence of "pecuniary returns on invested capital" is not a sure criterion or a self-evident fact since most of the privately organized performing groups on a full-time basis are operated for gainful purposes. That some social purpose is objectively fulfilled by these groups is not unique; all occupational groups fulfill "some social purpose," be it merely that of maintaining our plumbing systems or cutting our hair. Nor is pride in quality a distinctive characteristic of the performing arts. Most successful producers of complicated products or services who cater to informed and interested buyers become quality conscious, and many of these producers earn a deserved reputation for dedication to quality as a matter of merchandising reputation. Nor is the necessity of choice among abounding investment projects designed to improve quality unique to the performing arts; all quality-minded producers wrestle with the same dilemmas of rationing under a budget or price restraint. So likewise it is to be doubted that the services of performing artists are disposed of on terms which do not effectively exploit the income and wealth potential of customers. Even if the artists concerned should be a bit naïve, their managers and business organizers are shrewd and resourceful in designing schemes for price discrimination and customer segregation on terms which are designed to maximize revenues.

The basic economic relationships which seem to underlie the financial problems of the performing arts are found, I would submit, in other directions faintly indicated in the Baumol-Bowen account: (1) performing art organizations are a surviving species of craft enterprise with its characteristic small market, small organization, and operation on the basis of craft skills and talents; (2) though all craft and professional workers derive considerable psychic returns from exercise of their skills and talents in a chosen vocation, the tendency for utilization of work as a mode of self-expression is carried farthest in this calling; (3) though the performing arts have undergone little or no technical improvements, the artists involved have been able to reach much broader audiences through the revolution in means of transmission yielding the new artistic forms and media of the movie, television show, hi-fi phonographic reproduction. Because of the latter technological revolution, the performing arts suffer from real technological unemployment, and the low levels of mean earnings adduced by Baumol-Bowen, while partly depressed because they include many part-time workers, also mask, I would guess, an unusually wide spectrum of dispersion among celebrities who have broken through the mass media and who serve millions and down-and-outers waiting their turn on the "extra" line.

The case for public support spelled out at the close of the paper can be strengthened by appeal to quite other considerations. Such support constitutes in part a phase of public education, for it develops valuable capacities and talents which otherwise would lie dormant. Second, the services of the performing arts are enhanced by sharing with others and up to points of congestion marginal costs are zero for short-run operations. Third, tastes for the performing arts must be cultivated and public expenditures for this purpose foster "infant industries" which must be renewed with every generation.

SIMEON E. LELAND: This is one of the most interesting sessions of the American Economic Association that I have attended. The three excellent papers covered the gamut of the economic status of those engaged in higher education: from the poorly paid artists (whether painters or dancers) who stand at the bottom of university salary lists to the financially elite doctors and the equally elite practicing members of this Association.

Cartter is right. The university is an economic anomaly. Under Baumol's classification it is probably the prime not-for-profit organization, which large group consists of a lot of cats and dogs, as any annual state corporation commissioner's report will show. It may be superfluous to add, though this reminder is often pertinent, that a university does not exist primarily for research or the economic gain of professors, but rather for the training and educating of students. Nor are hospitals run for doctors or theaters for actors, though often the patients and the students tend to be (temporarily) forgotten. In the theater, however, the audience is always foremost.

A university is like a firm, as Cartter suggests, yet the differences outnumber the similarities. A diversified output is its goal or the result of its production. The "pieces" are not standardized from one plant to another, nor from the same plant in any period of time. Unlike the firm, the university does not sell its product at cost or recoup its costs (if it knows them) over the long run. When proposals are made to charge full cost to students (or parents), the changes are resisted and even deferral of a part of the costs to later years is also vigorously opposed. The higher the education the greater the unwillingness of the individual to assume the costs. Graduate students in recent years have succeeded in pushing larger and larger portions of the cost of their education on to others—donors of fellowships rather than parents—until they, like the institutions that train them, acquire their capital assets via gifts or state appropriations. What has become of the old-fashioned notion that students are making a capital investment in themselves when they pay for their graduate education? Moreover, there is little willingness on the part of the students to borrow to finance their education. Nor are the state universities willing to charge out-of-state residents the cost of their education. The rule is to sell education below cost and depend on subsidies, gifts, donations, and gratuities to keep going—true for both individuals and institutions.

It should be emphasized, too, as Cartter has pointed out, that a large share of the cost of graduate education has been borne by undergraduates. In general, undergraduates pay higher tuition than graduate students. They are

taught in larger classes by lower-cost instructors, and the savings are diverted to upper-class and graduate instruction. Nor do undergraduates need as expensive library facilities, computer centers, or laboratory equipment as their more advanced companions. It is no accident that the eminent graduate institutions have grown up leaning on their colleges; and this has been the typical evolution rather than the other way around. Even the undergraduates have contributed more liberally (I believe though I haven't verified this statistically) to university fund solicitations than their brothers and sisters with Ph.D.'s or other higher degrees.

It is easy to corroborate, too, how little the universities know about themselves, how sparse are the factual data needed for efficient management, and how little use they make of the brains on their staffs (so effectively used by outside firms at high consulting fees) to give them competent advice on operations, investments, purchasing, advertising—and so on, almost without limit. Bigness to some means more than quality, tradition more than efficiency, deficits more than savings—many of which could be avoided if their institutions researched themselves as well as everything else. Educational managements do not seem to care about determining "breaking points" or optimum size or the number of branch plants they can efficiently operate. Higher education seems to have expanded without regard to cost or quality of output. Before some universities can get their new branches into operation they are planning even newer ones! Perhaps we should ask, "What can man afford?" But that is not as important as one fact from Cartter's evaluation survey, to wit: there is "rather poor" correlation between the size of an institution and the quality of its departments. Today the economics departments were the object of correlation. Quality and size do not go together! There is reason to believe that most of those who strive for size do not care about quality in higher education. This may explain the low correlation.

Quality is expensive, so of course are numbers, and well may the choice be made. But quality is expensive regardless of fields, and there is little difference, at least in my shop, in top rates of professional pay even among noncompeting groups. In my experience, top-notch professors of English, history, biology, biochemistry, chemistry, political science, or physics are as well paid, in general, as top-notch economists. On each salary peak there are several roosts for a variety of birds.

The institutional rating survey of which Cartter gave us a glimpse is undoubtedly the best to date, but though the total sample is large, the sample of economists seems small when spread over twenty departments. The corroboration of ratings by even smaller groups was illuminating, but even this does not disclose the strengths and weaknesses in specific fields within our discipline. Here is where graduate students need advice, particularly since they either tend to aim for the "top three or four" universities, regardless of weakness in the area of their concern, or else they tend to accept the largest stipend offered by anyone. The survey of thirty departments common to our universities should help correct this. Perhaps graduate students would be better advised to learn of the weaknesses of a university than of its strengths.

What Cartter says about the correlation between quality ratings and staff

productivity is heartening. It provides evidence to support many of our biases, as well as promotion and salary practices. Good departments produce; good economists become known to their professional colleagues through what they write. Good economists get in good departments—just as good farmers till good land. Although production is important, professors can be expected to be good teachers.

The changes in departmental reputations over time, which Cartter points out in tables and charts, are most interesting. In spite of constancy at the top, these data illustrate the old adage about "shirt sleeves to shirt sleeves." One of my administrative colleagues from Wisconsin says that it takes about a decade for professional groups to recognize what has happened to institutions regardless of the direction of their movement up or down the scale. The tables do show what has happened in several departments, perhaps not in all.

Other changes may yet have to be recognized. This prompts me to ask some of you, "Where were you, Charlie, when some of these things were happening?" My guess, Charlie, is that you were on leave—too long.

We are in great debt to our colleagues today for the wealth of material and ideas they have presented to us. Let us continue this discussion at future meetings.

MARTIN BRONFENBRENNER: Professors Baumol and Bowen have given us not so much a finished paper as a promising progress report of their current research on the economic status of the performing arts. Their completed book will not only treat their problem under more heads than this paper but will include a good deal of the evidence to which they have only referred here. Under these circumstances, my role is primarily to suggest a few items for the expansion process, which they may have considered or even rejected already. There is only one caveat or emendation I should like to offer, relating to technological change (point 3 below).

1. Baumol and Bowen mention "fair price" attitudes among performing artists and impresarios as interfering with optimal pricing from the viewpoint of artists' income. I should go somewhat further, to explore a similar attitude among the public which buys their services. The consequences of this attitude are, I think, amenable to treatment by the apparatus Samuelson and Musgrave developed a few years ago in connection with public goods.

Let me condense the Samuelson-Musgrave argument: As taxpayers, consumers of public goods have an economic motive to understate their demands for these goods and shift their costs to other taxpayers. But since "other taxpayers" do the same thing, there is a more-or-less universal understatement of demand for public goods. Therefore, too few of them tend to be provided relative to private goods, from the viewpoint not only of Pareto optimality but of more general varieties of welfare theory. The true marginal utilities of private goods tend to be systematically lower (per dollar's worth) than those of public goods, to nearly all consumers of both.

Something similar may also be true for a class of goods, often private in our economy, which we may call "philanthropic goods." These include not only the theater and the concert hall but also the hospital, the university, the

library, the museum, the zoo, and similar cultural institutions. When a "fair price"—in some cases zero—will not provide the services of cultural institutions, the man in the street expects someone else to make up the difference—Washington, City Hall, Santa Claus, Lady Bountiful, or "dedicated" geniuses in garrets. While waiting for these people to come to his aid, the man in the street again is tempted to understate his demands by "resenting" higher prices, and the cultural services are again underprovided. (Sometimes they are not provided at all.)

An alternative form of this notion is that fair prices are somehow included in consumers' utility functions and that their importance is greater for cultural and other philanthropic goods than for most other goods. This usually means that, when the price of a cultural service rises, the fair price remaining the same, the amount of the service demanded falls by more than it would if the fair price were not involved in the consumers' functions.

2. In this progress report, Baumol and Bowen say little about the supply of performing artists, who choose to offer their services for less than they could earn in more popular or lower-grade alternative occupations. On this subject, I should like to offer a guess or conjecture for consideration in their coming book. This conjecture is that a disproportionately large fraction of this supply comes from a few segments of the population. Five such segments are:

a) Scions or proteges of wealthy families. The violinist Albert Spalding exemplified the first, his contemporary Mischa Elman exemplified the second.

b) Members of minority races, for whom the arts offer (almost) unique methods of rising from ghettos to the cultured world. Examples are plentiful from the Jews of Russia and Poland; the American Negroes are providing their share as well.

c) Physically attractive persons, especially women, for whom the arts can serve two purposes in one: professional careers and avenues for marriage into social classes higher than their parents'.

d) Specialized castes or guilds, sometimes limited to a few families, who pass an art down from generation to generation and include outsiders mainly by adoption. The *kabuki* actors of Japan are cases in point, but the American stage has also had its "royal families."

e) A few "Renaissance men" with more conventional skills to fall back on for bread and butter had their artistic reach exceeded their grasp. Joseph Hofmann was an inventor as well as a pianist, Fritz Kreisler a surgeon as well as a violinist.

A possible consequence of this conjecture about the supply of professional performing artists, if the conjecture is correct, is that with the decline of racial, religious, sex, and caste barriers to conventional careers, we can expect this supply to dry up in the future unless the economic status of these artists rises. (Automation may prove me wrong, by giving us all twenty or thirty extra hours a week to develop our latent talents to the professional artist level.)

3. Baumol and Bowen base a considerable amount of their pessimism upon the insulation of the performing arts from technological improvements. I won-

der whether they may not have exaggerated this point somewhat. Many, probably most, performing artists have secondary occupations, most of which are basically complementary with their artistic development—unlike Baumol and Bowen's "truck driving, lobster fishing, [and] waiting on tables." If some of the most important complementary secondary occupations are amenable to technical progress, the insulation of the artistic performance itself is less important than our paper maintains.

Perhaps a few examples will make the point clearer. As we know, many performing artists also teach. When technical progress permits pupils to be met five, ten, or twenty rather than one at a time, or when it lessens the time the teacher must waste running about between pupils' homes, there is an improvement in the teacher's productivity. (I hope improvement stops short of the thundering herds of sophomores some of us face in Memorial Auditorium for Economics 101.) Artists also make recordings, which apparently generate increased net demand for live performances as well. The recording industry, surely, is not immune from technical advance. What is true of records seems to apply also for films, slides, tapes, television, and the various media for reproducing works of graphic art. Baumol and Bowen suggest, and I agree, that technical improvements redound in part to workers' income. What should be added to their account is that technical improvements in performing artists' complementary secondary occupations, such as teaching, recordings, and television, add to the artists' real incomes in much the same way as would improvements in the performing arts themselves.

4. For all its pessimism, I missed in the Baumol-Bowen paper any sense of urgency. Urgency may be uncalled for, but we should recall that a branch of art can disappear in fairly short order—or, at least, can collapse into a kind of museum piece, preserved in suspended animation and trotted out for visiting specialists as a living genuine antique. Within the present century, something of the sort has happened to the theatrical *smorgasbord* called vaudeville. Since 1945, something of the sort has accelerated dangerously for almost all the traditional Japanese dramatic and musical arts, despite the great economic upsurges since the Korean war. I do not myself believe that it is later than Baumol and Bowen think it is—but what if both I and they are wrong?

THE ECONOMICS OF POVERTY

INVESTING IN POOR PEOPLE: AN ECONOMIST'S VIEW*

By THEODORE W. SCHULTZ
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Economists have made room for economic stability and for growth. But poverty for want of a theory is lost in economics notwithstanding all of the statistics that show the size distribution of personal income and the age, sex, and family composition of people with low income and with consumption below some standard. A vast catalogue of the attributes of poor people is at hand. But for all that, there is no integrated body of economic knowledge and no agenda of economic hypotheses to get at important economic questions about poverty. Although there are valuable empirical studies, there is no integrated analytical approach to determine the factors that account for the distribution of personal income and wealth. It would be a mistaken view, however, to blame those who have produced these statistics; for economists have not formulated a theory to guide the organization and analysis of facts pertaining to poverty. Although it is obvious that without theory statisticians founder, if blame we must, we should fault economists for not bringing poverty into the realm of economics.

Nor was it always thus. Malthus theorized about the causes of poverty and from the type of population growth that he considered normal he derived his well-known dismal consequences.[1] Marx built a system which gave the fruits of progress to the owners of capital. Marshall's conception of enterprise implied progress with poverty diminishing; Pigou then complemented this with welfare programs. But modern economists have not found poverty to their analytical taste. For a time there was a concern about the personal distribution of income but it is now outmoded. More recently the elegant community welfare function was fashionable; but its purpose is not to analyze poverty and it is ob-

* I am indebted to Harry G. Johnson, Herman P. Miller, and Eugene Smolensky for pointing out a number of omissions and ambiguities. Each has made available to me a recent paper he had prepared on which I have drawn with profit.

[1] The Malthusian conception of the growth in population is by no means obsolete when one examines the recent upsurge in population in relation to the supply of food in many poor countries. See my, "Economic Growth from Traditional Agriculture," appearing in *Agricultural Sciences for the Developing Nations* (Pub. No. 76, American Association for the Advancement of Science, 1964). The monumental study by B. H. Slicher Van Bath, *The Agrarian History of Western Europe A.D. 500 to 1850* (St Martin's Press, 1963), leaves little room for doubt that for ages past the growth in population and the expansion of agriculture were broadly related in the way that Malthus represented that relationship.

viously empty in specifying and identifying poverty. The conventional thing to do, of course, is to believe in a natural law which will cause poverty to disappear. What remains in the affluent society are pockets of people who because of preferences or circumstances have not been cleansed by progress! Little wonder then that poverty has no room in the house modern economists have built.

Meanwhile poverty has been placed on the political agenda, oddly enough in spite of the fact that poor people are poor voters. To add to the perplexity, although the legislative approach is cast in terms of economic opportunity which should appeal to economists, few have responded to this appeal. Data showing the personal distribution of income were never more abundant and as good as they are presently, and yet anyone who examines them critically is convinced that they are far from satisfactory in identifying the people who are really poor. Who are the poor? Why are they poor? What can best be done to improve their lot? These are questions that would seem to require economic analysis. But economic theory seems to be of little avail. It could of course be true that they are not economic questions, or that the economic components in poverty are trivial and of no importance. I am under no illusions that anyone can in one fell swoop formulate the required theory, much less that I as a novice in this field can perform this task. There are undoubtedly many building blocks of which I have little or no knowledge. Yet, so it seems to me, parts of the core of economics can be used and the contributions that they can make may not be trivial in adding to our knowledge of poverty.

My plan is, first, to consider two applications of "consumer" demand, then to present three hypotheses with respect to changes in the sources of income and the effects of these changes upon poverty, and lastly to call attention to some of the implications of this approach.

I. *Two Applications of Demand Theory*

In applying the concept of demand, I turn to the effects of rising per capita income upon demand in an attempt to determine why the so-called "poverty line" is rising, and also why more young people and more of the aged become consumer units with earnings that place them in the low-income class. Although it is obvious that the standard by which society judges the welfare of the poor has been rising over time, it is not obvious that this rising standard represents an increase in the demand for welfare services for the poor, that this increase in demand as it is revealed by the social-political process is a function of the rise in per capita income which can be treated as an income elasticity.[2]

[2] To visualize this process, it is helpful to use a conventional demand and supply dia-

The underlying observed behavior appears to be consistent with an income elasticity substantially less than unity. During the period since the mid-thirties real income per family virtually doubled and the poverty line, measured in constant dollars, may have risen about one half.[3] I venture that the relevant income elasticity here, although it is gradually becoming less elastic as per capita income rises, is sufficiently stable to make useful and dependable projections.

The rise in per capita income also increases the demand for the convenience of smaller households where young people can move from their parental household and set up separate households of their own and older people can maintain their status as separate families longer than formerly.[4] The demand here also can be treated as a function of the rise in income, hence as an income elasticity. The economic logic here presumes that families prefer and can afford as their incomes rise to have their young people break away from the parental unit and establish their own homes at an earlier age than formerly. It also pre-

gram. Place the price of a standard unit of welfare service on the vertical scale and the quantity of such welfare services that will be provided per family for poor people on the horizonal scale. Assume for convenience that the supply curve is horizonal in the sense .hat the supply price remains constant. The demand curve of the usual slope is then lrawn. The intercept will indicate the quantity of such welfare services per poor family .hat the social-political process will "vote for" at the price indicated by the intercept. Suppose now that the per capita income of the community rises and that the income elasticity of the demand for these welfare services is positive, the demand curve will as a consequence shift to the right and this means that the social-political process will vote for . larger quantity of these welfare services per poor family than formerly and that the quantity will be indicated by the new intercept. Meanwhile, of course, the development hat produces the rise in per family income generally may increase the income of many amilies who were formerly poor, so much that even by the new higher standard they are no longer among the poor.

[3] Eugene Smolensky, in "The Past and Present Poor," develops a concept of poverty which shows that when one takes the contemporary poverty line to be $3,000, the poverty ine in 1935 was in the neighborhood of $1,950, both expressed in 1959 dollars, thus by his measure the poverty line has risen 55 percent since 1935. Real income per family between 1935-56 and 1963 rose from $3,343 to $6,613 in 1954 dollars; see *Survey of Current Business*, Apr., 1964, Table 13. Smolensky places 37 percent of the families in poverty s of 1935 and 23 percent so situated in 1959. In the text of his paper he refers to onehird and one-fifth as the appropriate estimates for the mid-thirties and currently. Herman '. Miller in "Measurements for Alternative Concepts of Poverty," American Statistical ssociation, Chicago, Illinois, Dec., 1964, presents cogent reasons for the rise in the overty line and data to show the extent of the rise. He cites estimates of the cost of ubsistence budgets made by Ruth Mack. According to these estimates, the percent of amilies with income below this budget are as follows:

	Contemporary Definition of Subsistence	1960 Definition of Subsistence
935	28	47
941	17	31
950	13	26
960	10	10

[4] The studies by Dorothy S. Brady are especially relevant in this connection.

sumes that families prefer and can afford to maintain their old people as separate units longer than formerly before these aged members become an integral part of the household of their offsprings. It has been widely observed that this process of subdivision creates and maintains additional consumer units which functionally earn relatively small incomes.[5] But what then is the welfare implication of the fact that some of these additional consumer units show statistically as poor? When treated as an increase in the demand resulting from the rise in income, the implication is that the subdivision which occurs for this reason represents a superior welfare position, notwithstanding the fact that we observe as a consequence more young and old people who are poor statistically in terms of earnings.[6]

But these two applications of the theory of demand will not tell us much about poverty. They call for no investigation of the sources of the rise in income, whereas these sources are, so it seems to me, the key to the economics of poverty. The history of poverty since the industrial revolution can usefully be divided into two parts: economic growth with no appreciable rise in per capita income and, then, with substantial increases in per capita income.[7] The latter is a comparatively recent development and it is for this type that we lack a theoretical scaffold. Classical theory meanwhile continues to be relevant in investigating poverty in an economy in which there is growth but no rise in per capita income. The magnificent dynamics of Malthus, Ricardo, James Mill, McCulloch, and Senior—the leading classical economists—are based on a model in which earnings per laborer do not rise.[8]

[5] The trend of family income of heads of families, ages 14-24 and 65 and more has been declining relative to the income of all families. The following estimates of the median income of all families and the two age classes mentioned show this trend.

MEDIAN INCOME

	1947		1960	
	In 1959 dollars	Relative	In 1959 dollars	Relative
All families	3,957	100	5,547	100
Age 14–24	3,075	78	3,965	71
Age 65 and more	2,398	61	2,862	52

SOURCE: H. P. Miller, *Trends in Incomes of Families in the United States: 1947 to 1960* (Technical Paper No. 8, Bureau of Census, Washington, D. C., 1963).

[6] The consumption picture is of course substantially modified by income transfers, not only through public measures, but importantly by private income transfers to young people from their parents and to older people from their established offsprings.

[7] For differences between these two parts and the movement from the first to the second in terms of sociological-economic evidence and in terms of agricultural-economic evidence, respectively, see Jean Fourastié, *The Causes of Wealth* (Free Press of Glencoe, 1960). Translated by Theodore Caplow; and Slicher Van Bath, *op. cit.*

[8] See, William J. Baumol, *Economic Dynamics* (Macmillan Co., 1951), Part I.

Our economy obviously is not of this type. The plain fact is that the earnings of labor are rising, that these additional earnings account for virtually all of the rise in per capita income, and that they are the primary factor in reducing poverty over time. Why are these earnings increasing? Why is labor's functional share of national income becoming larger? These questions as already noted have become relevant rather recently in our history. How then are we to approach the dynamics of our economy with a view of settling these questions?

Before turning to them let me mention some well-established differences among the sources of income and some specialized chains of economic logic to guide particular types of analysis in this field. We are accustomed to classifying the functional sources to show whether they are from wages and salaries, from property, or from self-employment which is as a rule a combination of the two. To these sources income transfers are then added in classifying the sources of personal income. Nor is all of it monetary income; for a part of it is in kind and in services that are as a rule consumed directly by those who have such sources. But these differences are less important for our purpose than are the differences with respect to price, marketability, and the time it takes to develop the various sources and to obtain income from them. With regard to economic logic, the movements of the different income streams, i.e., those from profits, property, and labor, associated with the business cycle, undoubtedly have their own internal logic.[9] So do the effects of the process of economic growth upon the income of different regions, industries, and occupations.[10] Then, too, we now have a theory with respect to consumer behavior for assaying the transitory and permanent components of different classes of income.[11] Without detracting from these achievements, they do not tell us why earnings are rising relative to other income and rising per worker. To cope adequately with these questions, we await a general theory that will integrate these specialized chains of economic logic and provide us with a comprehensive and consistent analytical framework for deter-

[9] The income effects of changes in rate of unemployment have been receiving much deserved attention. See, for example, Harry G. Johnson, "Poverty and Unemployment" (Nov., 1964, unpublished). In explaining the decline in poverty historically, this paper attributes more importance to this factor than it would appear to warrant. See, also, Eugene Smolensky, "The Past and Present Poor," already cited. Robert J. Lampman's study, "The Low Income Population and Economic Growth," Joint Economic Committee, 86th Cong., 1st Sess., Nov. 16, 1959, also treats unemployment. There are, of course, many more treatments.

[10] My first attempt to treat poverty belongs here. "Reflections on Poverty within Agriculture," *J.P.E.*, Feb., 1950; and then on "A Policy to Redistribute the Losses from Economic Progress," *J. of Farm Econ.*, Aug., 1961. It also appears in *Labor Mobility and Population in Agriculture* (Iowa State Univ. Press, 1961).

[11] Milton Friedman, *A Theory of the Consumption Function* (Princeton Univ. Press, 1957).

mining the functional and final distribution of personal income. As a first step, I shall present the following hypotheses.

II. *Three Hypotheses*

My purpose here is to classify and organize the different sources of income streams and then to show how they are related to the observed changes in poverty. I propose to use the concepts of supply and demand to determine the changes in the prices of the different sources of these income streams. Income streams can be given quantitative dimensions per unit of time; i.e., a one-dollar-per-year income stream. Except for income transfers, to obtain possession of an income stream it is necessary to acquire the source of that stream. These sources are valuable, and each income stream in this sense has a price. The price may be low or high. If a source of a one-dollar-per-year income stream can be acquired for $10, it presumably would be cheap; if it cost $25, it would be dear. Thus what we should do is to identify the different sources and then proceed to ascertain the price at which each of the respective sources can be acquired. The central economic problem then becomes one of explaining what determines the price of these income streams. In this approach, then, it is meaningful to apply the concepts of supply and demand.[12]

The underlying assumptions, which are quite conventional, are as follows: the sources of income streams are acquired at particular prices; these prices change over time, and people respond to changes in these prices subject to the restraints of the capital market, their preferences and capacity to save, the effects of taxes and subsidies, and of discrimination with respect to employment and investment. We can then postulate a dynamic process from which we can derive the following complementary hypotheses that pertain to the type of economic growth that we have had during recent decades:

1. The price of the sources of income streams that represent the acquired human capabilities of value in economic endeavor declined during this period relative to the price of material forms.

2. In responding to this change in the relative prices of these two sources of income the rate of investment in human sources rose during this period relative to that in material sources.

3. The increase in the investment in human sources relative to the investment in nonhuman sources has increased earnings relative to property income and the more equal distribution of investment in men has tended to equalize earnings among human agents.

These are testable hypotheses. They appear to win support from a

[12] In this paragraph I follow closely my *Transforming Traditional Agriculture* (Yale Univ. Press, 1964), Chap. 5, beginning with p. 74.

number of new studies. The private rates of return to schooling support the first. My attempt to test the second, admittedly a very rough approximation of the increases in the latter two of these stocks, indicates for the period between 1929 and 1957 that the stock of reproducible tangible wealth increased at an annual rate of about 2 percent while that of education in the labor force rose at a rate of 4 percent, and that of on-the-job training of males in the labor force at over 5 percent.[13] The marked increase in the proportion of the labor force that has attended high school and college is one of the developments in support of the third hypothesis.[14]

But this is not the occasion to enter upon a survey of the studies that provide estimates to test these hypotheses. Instead, I want to explore the developments that have been altering the underlying supply and demand of the major sources of these income streams.

During a normal business cycle the supply does not change substantially. The demand, however, shifts back and forth considerably during recessions and booms, and as a consequence the income from corporate and from some other forms of property fluctuates more over the cycle than national income. The fluctuations in income from wages and salaries are largest for unskilled labor, for workers who are least specific in their training in terms of the labor requirement of employers, and for workers who have the least seniority, with the result that the inequality of the personal distribution of income decreases in years of prosperity and increases in years of depression.[15]

Over the long run, both the demand and supply are subject to shifts some of which are accumulative and become large over time. Among the factors that shift the demand for the sources of income streams in this context, three are of major importance:

1. The aggregate demand for goods and service. Theory here is better than the art of fiscal-monetary policy. The aggregate demand was obviously far from sufficient during the massive unemployment of the early 1930's. It was more nearly enough during the high employment of the mid-fifties. Since then there has been much slack. Idle plants and idle men reduce the demand for the sources of income. Clearly

[13] From my "Reflections on Investment in Man," *J.P.E.*, Sup., Oct., 1962, under the title, "Investment in Human Beings," Table 1, p. 6.

[14] Between 1940 and 1959, for the labor force 18 to 64 years old, both sexes, the number that had completed four years of high school rose from 20 to 32 percent, and that had attended college rose from 13 to 19 percent of the total labor force. *Statistical Abstract of the United States 1960*, Table 139, p. 109.

[15] Entrepreneurial income aside, in a free enterprise economy there are two sources of income fluctuations that are very burdensome for many poor people. One is a consequence of the low price elasticity of the demand for farm products where there are wide year-to-year variations in production because of weather. The other arises from short-run inelastic supply of unskilled labor where the shifts in demand for such labor are large over the business cycle.

poor people have much at stake in government policy that will maintain full employment.

2. The demand effects of the advance in knowledge. These effects are commonly concealed under "technological change."[16] New knowledge that is useful in economic endeavor requires either new forms of material capital or new skills on the part of labor, or, what is true in general, both are required. This factor, so it appears, has increased the demand for high skills relative to low skills and for the productive services of labor (numbers and quality combined) relative to the productive services of old and new material things.

3. The demand effects and changes in restrictions "on the opportunity for individuals to participate in the productive process to the full extent of their potential."[17] What matters here is discrimination against Negroes with respect to jobs, against the aged poor who still are willing and able to do productive work but who are required to quit working or work only part time to be eligible for retirement and survivor payments, and against the participation of women in the labor force.

Turning next briefly to the long-run changes in the supply of the sources of income streams, these changes may be explored either in terms of adjustments to shifts in the demand or in terms of factors which play a fairly independent role. The adjustment process in which the demand and supply interact is the core of the economic behavior underlying the formulation of the second hypothesis here advanced. The major "independent" factors affecting the supply are as follows: research and development activities and dissemination of the resulting useful knowledge from these activities, the mobility (immobility) of particular sources, predominantly labor, in leaving declining industries and occupations, the amount and distribution of public investment in schooling, and, closely related, the discrimination against Negroes, rural farm children, and others with respect to schooling.

Thus, the analytical task at hand might be to account for the observed decline in poverty, or alternatively to account for the poverty that remains. Although the latter has its appeal, for it is more direct, it may be less efficient because it is undoubtedly true that the first task is a prerequisite to doing the second. I shall therefore continue to concentrate on the first task.

Income from Property. By all accounts the functional share of income from property has been declining. The stock of tangible reproducible wealth has not increased at as high a rate as the acquired abilities of workers. Differences in the rates of return have favored invest-

[16] See my *Transforming Traditional Agriculture*, Chap. 9, *op. cit.*
[17] I follow closely here Harry G. Johnson, "Poverty and Unemployment," already cited.

ment in human capital. True, the relative decline in income from material wealth would undoubtedly have been somewhat less during the recent past had the tax on corporate income remained at the prewar level. Meanwhile, what has been happening to the personal distribution of wealth holdings? It is hard to believe that poor people have been acquiring a substantially large share of this wealth and that it is the source of the income that has brought about the observed decline in poverty. The stock of wealth represented by houses may be an exception, in the sense that it has been an attractive investment for many low-income families while the economy has been adjusting to the favorable tax treatment that home ownership has been receiving. But homes owned by families with less than $3,000 of income in 1962 had a mean value of only $3,750.[18] Any plausible increase in the net worth of low-income consumers since the mid-thirties could account for only a very small part of the decline in poverty.[19]

Income from Labor. Meanwhile, labor's functional share of national income has been rising. The demand for workers with high skills has been increasing at a higher rate than that for low skills. Thus the incentive to increase skills has been strong and the supply of skills has been responding; for people have been investing much more than formerly to increase their skills. But why has the demand for skills been shifting upward in this manner? In my judgment it has come about mainly as a consequence of the dynamic process in which skills along with new useful knowledge gradually have been increasing national income, and at the same time the resulting rise in per capita income of consumers has altered the mix of products and services demanded in such a way that the products and services requiring high skills have increased at a higher rate than that of those requiring low skills. Another factor of some importance in this process has undoubtedly been the increase in the demand for producer durables and services by the military establishment, which also has been increasing the demand for high skills.

III. *Implications*

The first and most general implication is that the observed large decline in poverty is primarily a consequence of increases in income from labor. It is not to be attributed to income from property. The real earnings of workers have been rising because the demand for high

[18] See, "Survey of Financial Characteristics of Consumers," *Fed. Res. Bul.*, Mar., 1964, Supp. Table 2B.

[19] The families with less than $3,000 of income in 1962 in the Federal Reserve Survey just referred to had a mean net worth of $8,875. Even if one were to assume that this net worth had doubled over recent decades for those at this real income level and one were to allow a 10 percent rate of return, it would account for only $444 of additional income.

skills has been increasing relative to that for low skills and because workers have been acquiring the more valuable skills.

Another implication is that a substantial part of the remaining poverty is a consequence of a number of disequilibria. Although workers have been responding to the changes in the market for skills, the economy in this respect has been in substantial disequilibrium at many points. The reasons why this is so are fairly obvious; namely, unemployment, the adverse incidences of economic growth on some sectors, inadequate information, and a lack of opportunity to invest in acquiring the more valuable skills because of discrimination and the restraints on the capital market in providing funds for this purpose. Let me call attention to three of these disequilibria.

1. The market for the skills that are required in agriculture has been long depressed. Although the labor force devoted to farming has declined by one-half since 1940, the market for these skills is still in serious disequilibrium. Older members of this labor force have had no real alternative but to settle for the depressed, salvage value of the skills they possess. In many farm areas the quality of elementary and secondary schooling has been and continues to be far below par[20] and thus the oncoming generation from these areas is ill-prepared to take advantage of the strong market in other parts of the economy for high skills. It should also be said that the vast expenditures by the federal government on behalf of agriculture have not been used to raise the level of these skills; on the contrary, they have been used in ways that enhance the income from some classes of property and that worsen the personal distribution of income among farm families. Thus it should not come as a surprise that although farm families are presently a very small faction of all U.S. families, they account for much of the observed poverty[21] and that many of the families in urban areas who are below the poverty line have recently come from our farms.

2. The market for the skills of Negroes has also been long depressed and the poverty component here is large. This market has been intertwined with that of agriculture; and both on our farms and in our

[20] See my "Underinvestment in the Quality of Schooling: The Rural Farm Areas," in *Increasing Understanding of Public Problems and Policies 1964*, Proceedings of the 14th National Agricultural Policy Conference, College Station, Texas, Sept. 15-17, 1964 (Jan., 1965, Farm Found., Chicago).

[21] It should be noted, however, that the precise amount of poverty attributed to farm families is not as large as the widely used statistics appear to show. Wealth holdings are large relative to measured income. My estimate of average net asset position of farmers who are actually farming (3.48 million in 1963) was approximately $35,800 per farmer in 1963 (from my "Our Welfare State and the Welfare of Farm People," *Soc. Serv. Rev.*, June, 1964, pp. 125-26). In the "Survey of Financial Characteristics of Consumers," *Fed. Res. Bul., Mar.*, 1964, Table 2, p. 293, the average net worth of the 2 million farm operators in this sample came to $43,973 on Dec. 31, 1962. My colleague, Margaret Reid, has done yeoman work in directing attention to components of real income of farm operator families that are still not measured.

cities, there has been and continues to be much job discrimination. More important still is the low level of skills of Negroes, which is mainly a consequence of the history of discrimination against Negroes in schooling. Not only have Negroes obtained fewer years of schooling but the schooling has been of very low quality; it was especially so for the older Negroes in the labor force.

3. The South is burdened with much more poverty than other regions basically for three reasons: (*a*) it is more dependent upon agriculture than the rest of the United States (it accounted for over 45 percent of all U.S. farms at the time the 1959 census of agriculture was taken); (*b*) the labor force in the South is more largely Negro than in the North and West and in terms of marketable skills the Negroes in the South are even worse off than the Negroes in other regions; and (*c*) relatively more of the whites in the labor force in the South have low skills than whites in other regions. In short, the South has been lagging seriously in providing people the opportunities to invest in acquiring the high skills for which the demand has been increasing at so rapid a rate, predominantly because of social, political, and economic discrimination adverse to poor people.

In conclusion, this paper is a modest proposal to provide a small room for poverty in the house that economists have built. To furnish it there are two handy demand pieces. Both go back to the preferences of people underlying their demand: one is for welfare which marks the poverty line, and the other is for consumer units which divide families. They give us two more attractive income elasticities to estimate and to converse about. A more useful piece is designed to explain the large decline in poverty. The concepts of supply and demand can be applied to determine the price of the different sources of income streams. Instructions are included for setting up the economic logic of our development during the recent past and for deriving from this logic three testable hypotheses. Available tests are comforting. Thus it would appear that there have been strong economic incentives to invest in man, and such investments will explain most of the observed decline in poverty. The poverty that remains is to a large extent the result of such investment opportunities having been thwarted.

APPROACHES TO THE REDUCTION OF POVERTY*

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The greatest accomplishment of modern economies has been the raising of living standards of the common man and the reduction of the share of the population in poverty. Contrary to the gloomy predictions of Malthus, production has increased faster than population and, unlike the expectations of Marx, inequality of income has not steadily increased. The growth in value of product per person is generally understood to arise out of more capital, economies of scale and specialization, better management and organization, innovation with regard both to end products and techniques of production, greater mobility of factors, and improved quality of labor. All of these in turn yield additional income which, in a benign spiral, makes possible more and higher quality inputs for further growth.

The process of growth has not meant simply higher property incomes. As a matter of fact, income from property has fallen as a share of national income.[1] Neither has growth meant a widening of differential for skill in labor incomes. Rates of pay for the most menial of tasks have tended to rise with average productivity. Social policies in fields such as labor and education aimed at assuring opportunities for all have narrowed initial advantages of the more fortunate. Such policies, along with taxation, social insurance, and public assistance measures which redistribute income toward the poor have tended to stabilize if not reduce the degree of income inequality.

A growth in productivity of 2 percent per person per year and a relatively fixed pattern of income inequality probably have combined to yield a net reduction in poverty in most decades of American history. However, the rate of reduction has undoubtedly varied with changes in the growth rate, shifts from prosperity to depression, changes in immigration, in age composition, and in differential family size by income level.

The Poverty Rate and the Poverty Income Gap

Using a present-day standard for poverty and even without recognizing the relativity of poverty over long periods, we would estimate

* My colleagues Martin H. David, Harold M. Groves, and Burton A. Weisbrod have been helpful to me in criticizing an earlier version of this paper.

[1] Irving Kravis, *The Structure of Income* (1962), pp. 127-42.

that poverty had become a condition that afflicted only a minority of Americans by the second decade of this century. This situation was upset by the Great Depression of the 1930's but later restored by the booming economy of World War II. The postwar period has yielded a somewhat above average rate of growth in productivity and a reduction in poverty which probably is at least average for recent decades. The number of families in poverty (as marked off from nonpoverty by a $3,000 income at 1962 prices) fell from 12 million in 1947 to 9 million in 1963. This was a drop from 32 percent to 19 percent of families.

The rate of reduction one records or predicts will vary somewhat with the definition of poverty which he adopts. The Council of Economic Advisers adopted an income cut-off of $3,000 of total money income for families and $1,500 for unrelated individuals.[2] It is not inconsistent with those guidelines to make further modification for family size, using $3,000 as the mark for an urban family of four persons with variations of $500 per person and to set a lower mark for rural families. Such a procedure yields a slightly lower rate of reduction in the percentage of all persons in poverty than is suggested by the 32 to 19 drop shown above.[3] This discrepancy is due to a shift in family size and the rural to urban migration during the postwar years.

It is possible that consideration of personal income as opposed to total money income, of average rather than one year's income, of assets and extraordinary needs as well as income, and of related matters would alter our understanding of how poverty has been reduced. It is clear that some of these considerations affect the number and the composition of the population counted as poor; and it is obvious that the rate of reduction would vary if we varied the poverty line over time.

These matters of definition are important to a refinement of the generalized goal of elimination of poverty to which President Johnson has called us. Economists can assist in reaching a national consensus on the specific nature of the goal, of ways to measure the distance from and rate of movement toward the goal. Currently we are in the stage of goal-setting with poverty that occurred in 1946 with unemployment and that we have experienced with respect to other national goals such as price stability and economic growth. Hopefully, out of current controversy there will emerge a refined and only infrequently changed measurement of poverty reduction which will take its place along with the unemployment rate and the growth rate and the consumer price index as guides to appraisal of the performance of the economy. In the

[2] *Annual Report* (1964), Chap. 2.

[3] This is roughly the procedure followed by my *Low Income Population and Economic Growth*, Study Paper No. 12, Joint Economic Committee, 86th Cong., 1st Sess., 1959.

meantime, we can carry on our discussion of ways and means to achieve the general goal with the rough and ready measures that are at hand.

At this point in time, poverty is clearly a condition which afflicts only a minority—a dwindling minority—of Americans. The recent average rate of change, namely, a fall in the percentage of families in poverty by one percentage point per year, suggests that the poverty problem is about twenty years from solution. This rate of reduction may be difficult to maintain as we get down to a hard core of poverty and a situation in which further growth will not contribute to the reduction of the poverty rate. My own view is that this rate is still highly responsive to changes in the growth rate and that it will continue to be so for some time ahead. The relationship between the two rates is a complex one and is influenced by such things as demographic change, changes in labor force participation, occupational shifts in demands for labor, and derived changes in property incomes and social security benefits. Some groups—notably the aged, the disabled, and the broken families—have poverty rates that appear to be relatively immune to growth in average income. One powerful drag on the responsiveness of the poverty rate to growth, which has now about run its course and will shortly reverse, is the aging and reduction in labor force participation of family heads.[4]

While the size of the poverty population is dwindling, the size of what can be called the "poverty income gap" is diminishing. This gap—the aggregate amount by which the present poor population's income falls short of $3,000 per family or $1,500 per unrelated individual—is now about $12 billion, or 2 percent of GNP. As time goes on this gap will assuredly be less, both because of economic growth and because of scheduled increases in social insurance benefits. (Transfers now make up about $10 billion of the $25 billion income of the poor.) Projecting recent rates of change suggests that by 1975 the poor will be no more than 12 percent of the population and the poverty income gap will be as little as 1 percent of that year's GNP.

As I see it, the goal of eliminating poverty needs to have a time dimension and intermediate targets. I assume we want a rate of progress at least as fast as that of recent years. Further, it helps to think of the goal in two parts: the reduction of the poverty rate and the reduction of the poverty income gap. This means we want to work from the top down and from the bottom up, so to speak. The aim of policy should

[4] These questions are pursued in the form of an exchange with John K. Galbraith in *ibid.*, pp. 13-28. For a different approach and different conclusions, see W. H. Locke Anderson, "Trickling Down: The Relationship Between Economic Growth and the Extent of Poverty Among American Families," *Q.J.E.*, Nov., 1964, pp. 511-24.

be to do each type of reduction without slowing the other and to do both with the least possible sacrifice of and the greatest possible contribution to other important goals.

Why Poverty Persists

As background to such strategic decisions, it is useful to categorize the causes of poverty in today's economy. But perhaps it is necessary first to brush aside the idea that there has to be some given amount of poverty. Most economists have long since given up the idea that a progressive society needs the threat of poverty to induce work and sobriety in the lower classes. Similarly, one can consign to folklore the ideas that some are rich only because others are poor and exploited, that if none were poor then necessary but unpleasant jobs would go undone, that the middle class has a psychological need to exclude a minority from above-poverty living standards, and that poverty is a necessary concomitant of the unemployment which necessarily accompanies economic growth.

Why, then, is it that there remains a minority of persons who are involuntarily poor in this affluent society? How does our system select the particular members for this minority? To the latter question we offer a three-part answer: (1) Events external to individuals select a number to be poor. (2) Social barriers of caste, class, and custom denominate persons with certain characteristics to run a high risk of being poor. (3) The market assigns a high risk of being poor to those with limited ability or motivations.

One cannot look at the data on who are the poor without sensing that many are poor because of events beyond their control. Over a third of the 35 million poor are children whose misfortune arises out of the chance assignment to poor parents. In some cases this poverty comes out of being members of unusually large families. Among the poor adults, about a third have either suffered a disability, premature death of the family breadwinner, or family dissolution. A considerable number have confronted a declining demand for services in their chosen occupation, industry, or place of residence. Some have outlived their savings or have lost them due to inflation or bank failure. For many persons who are otherwise "normal" poverty may be said to arise out of one or a combination of such happenings.

A second factor that operates in the selection of persons to be poor is the maintenance of social barriers in the form of caste, class, and custom. The clearest example of this, of course, is racial discrimination with regard to opportunities to qualify for and to obtain work. (It is perhaps worth emphasizing here that only a fifth of the present poor are nonwhite, and that only a minority of the nonwhites are presently

poor.) Similar types of arbitrary barriers or market imperfections are observable in the case of sex, age, residence, religion, education, and seniority. They are formalized in employer hiring procedures, in the rules of unions and professional and trade associations, in governmental regulations concerning housing and welfare and other programs, and are informally expressed in customer preferences. Barriers, once established, tend to be reinforced from the poverty side by the alienated themselves. The poor tend to be cut off from not only opportunity but even from information about opportunity. A poverty subculture develops which sustains attitudes and values that are hostile to escape from poverty. These barriers combine to make events nonrandom; e.g., unemployment is slanted away from those inside the feudalistic walls of collective bargaining, disability more commonly occurs in jobs reserved for those outside the barriers, the subculture of poverty invites or is prone to self-realizing forecasts of disaster.

The third factor involved in selecting persons out of the affluent society to be poor is limited ability or motivation of persons to earn and to protect themselves against events and to fight their way over the barriers.[5] To the extent that the market is perfect one can rationalize the selection for poverty (insofar as earnings alone are considered) on the basis of the abilities and skills needed by the market and the distribution of those abilities and skills in the population. But we note that ability is to some extent acquired or environmentally determined and that poverty tends to create personalities who will be de-selected by the market as inadequate on the basis of ability or motivation.

Countering "Events"

Approaches to the reduction of poverty can be seen as parallel to the causes or bases for selection recounted above. The first approach, then, is to prevent or counter the events or happenings which select some persons for poverty status. The poverty rate could be lessened by any reduction in early death, disability, family desertion, what Galbraith referred to as excessive procreation by the poor, or by containment of inflation and other hazards to financial security. Among the important events in this context the one most relevant to public policy consideration at this time is excessive unemployment. It would appear that if the recent level of over 5 percent unemployment could be reduced to 4 percent, the poverty rate would drop by about one percentage point.[6]

[5] For an insight into the relative importance of this factor see James M. Morgan, Martin H. David, Wilbur J. Cohen, and Harvey E. Brazer, *Income and Welfare in the U.S.* (1962), pp. 196-98.

[6] Unemployment is not strikingly different among the poor than the nonpoor. Nonparticipation in the labor force is more markedly associated with poverty than is unemployment. However, it seems that about 1 million poor family heads experience unem-

Further fall in the poverty rate would follow if—by retraining and relocation of some workers—long-term unemployment could be cut or if unemployment could be more widely shared with the nonpoor.

To the extent that events are beyond prevention, some, e.g., disability, can be countered by remedial measures. Where neither the preventive nor the remedial approach is suitable, only the alleviative measures of social insurance and public assistance remain. And the sufficiency of these measures will help determine the poverty rate and the size of the poverty income gap. It is interesting to note that our system of public income maintenance, which now pays out $35 billion in benefits per year, is aimed more at the problem of income insecurity of the middle class and at blocking returns to poverty than in facilitating exits from poverty for those who have never been out of poverty. The nonpoor have the major claim to social insurance benefits, the levels of which in most cases are not adequate in themselves to keep a family out of poverty. Assistance payments of $4 billion now go to 8 million persons, all of whom are in the ranks of the poor, but about half of the 35 million poor receive neither assistance nor social insurance payments. One important step in the campaign against poverty would be to reexamine our insurance and assistance programs to discover ways in which they could be more effective in helping people to get out of poverty. Among the ideas to be considered along this line are easier eligibility for benefits, higher minimum benefits, incentives to earn while receiving benefits, ways to combine work-relief, retraining, rehabilitation, and relocation with receipt of benefits.

Among the several events that select people for poverty, the ones about which we have done the least by social policy are family break-up by other than death and the event of being born poor. Both of these could be alleviated by a family allowance system, which the U.S., almost alone among Western nations, lacks. We do, of course, have arrangements in the federal individual income tax for personal deductions and exemptions whereby families of different size and composition are ranked for the imposition of progressive rates. However, it is a major irony of this system that it does not extend the full force of its allowances for children to the really poor. In order to do so, the tax system could be converted to have negative as well as positive rates, paying out grants as well as forgiving taxes on the basis of already adopted exemptions and rates. At present there are almost $20 billion

ployment during the year. (*Census Population Reports,* P-60, No. 39, Feb. 28, 1963, Tables 15 and 16.) If half of this group were moved out of poverty by more nearly full employment, then the poverty rate would be one percentage point lower. Another way to estimate this is as follows. The national income would be $30 billion higher than it is if we had full employment. And a $30 billion increase in recent years has generally meant a full percentage point drop in the percent of families in poverty.

of unused exemptions and deductions, most of which relate to families with children. Restricting the plan to such families and applying a negative tax rate of, say, 20 percent, to this amount would "yield" an allowance total of almost $4 billion. This would not in itself take many people out of poverty, but it would go a considerable distance toward closing the poverty income gap, which now aggregates about $12 billion.

It would, of course, be possible to go considerably further by this device without significantly impairing incentive to work and save. First, however, let me reject as unworkable any simple plan to assure a minimum income of $3,000. To make such an assurance would induce many now earning less than and even some earning slightly more than $3,000 to forego earnings opportunities and to accept the grant. Hence the poverty income gap of $12 billion would far understate the cost of such a minimum income plan. However, it would be practicable to enact a system of progressive rates articulated with the present income tax schedule.[7] The present rates fall from 70 percent at the top to 14 percent at income just above $3,700 for a family of five, to zero percent for income below $3,700. The average negative tax rates could move, then, from zero percent to minus 14 percent for, say, the unused exemptions that total $500, to 20 percent for those that total $1,000 and 40 percent for those that total $3,700. This would amount to a minimum income of $1,480 for a family of five; it would retain positive incentives through a set of grants that would gradually diminish as earned income rose.

The total amount to be paid out (interestingly, this would be shown in the federal budget as a net reduction in tax collections) under such a program would obviously depend upon the particular rates selected, the definition of income used, the types of income-receiving units declared eligible, and the offsets made in public assistance payments. But it clearly could be more than the $4 billion mentioned in connection with the more limited plan of a standard 20 percent negative tax rate. At the outset it might involve half the poverty income gap and total about $6 billion. This amount is approximately equal to the total federal, state, and local taxes now paid by the poor. Hence it would amount to a remission of taxes paid. As the number in poverty fell, the amount paid out under this plan would in turn diminish.

Breaking Down Barriers

The approaches discussed thus far are consistent with the view that poverty is the result of events which happen to people. But there are

[7] Cf. Milton Friedman, *Capitalism and Freedom* (1962), pp. 192-93.

other approaches, including those aimed at removing barriers which keep people in poverty. Legislation and private, volunteer efforts to assure equal educational and employment opportunities can make a contribution in this direction. Efforts to randomize unemployment by area redevelopment and relocation can in some cases work to break down "islands of poverty." Public policy can prevent or modify the forming of a poverty subculture by city zoning laws, by public housing and by regulations of private housing, by school redistricting, by recreational, cultural, and public health programs. It is curious that medieval cities built walls to keep poverty outside. Present arrangements often work to bottle it up inside cities or parts of cities and thereby encourage poverty to function as its own cause.

Improving Abilities and Motivations

The third broad approach to accelerated reduction of poverty relates to the basis for selection referred to above as limited ability or motivation. The process of economic growth works the poverty line progressively deeper into the ranks of people who are below average in ability or motivation, but meantime it should be possible to raise the ability and motivation levels of the lowest. It is interesting that few children, even those of below average ability, who are not born and raised in poverty, actually end up in poverty as adults. This suggests that poverty is to some extent an inherited disease. But it also suggests that if poor children had the same opportunities, including preschool training and remedial health care, as the nonpoor (even assuming no great breakthroughs of scientific understanding), the rate of escape from poverty would be higher. Even more fundamentally, we know that mental retardation as well as infant mortality and morbidity have an important causal connection with inadequate prenatal care, which in turn relates to low income of parents.

A belief in the economic responsiveness of poor youngsters to improved educational opportunities underlies policies advocated by many educational theorists from Bentham to Conant. And this widely shared belief no doubt explains the emphasis which the Economic Opportunity Act places upon education and training. The appropriation under that Act, while it seems small relative to the poverty income gap, is large relative to present outlays for education of the poor. I would estimate that the half-billion dollars or so thereby added increases the national expenditure for this purpose by about one-seventh. To raise the level of educational expenditure for poor children—who are one-fifth of the nation's children but who consume about a tenth of educational outlay—to equal that of the average would cost in the neighborhood of $3 billion. Such an emphasis upon education and training is justified

by the fact that families headed by young adults will tend, in a few years, to be the most rapidly increasing group of poor families.

Summary

Past experience provides a basis for the belief that poverty can be eliminated in the U.S. in this generation. The poverty rate has been reduced at the rate of one percentage point a year; the poverty income gap is now down to 2 percent of GNP.

Preventing and countering the "events" which select people for poverty can help to maintain or accelerate the rate at which we have been making progress against poverty. For example, by returning to the 4 percent "full employment" rate of unemployment, we would instantaneously reduce the poverty rate by one percentage point. For another example, we could make a great stride toward early closing of the poverty income gap by modifying the income tax to pay out family allowances.

Another broad approach to the elimination of poverty is to break down the social barriers which restrict opportunities for the poor. Examples of this are legislating against practices of discrimination and making plans to bring the poor into the mainstream of community life.

The third approach is to make progressively greater investment in improving the abilities and motivations of the poor. Substantial increase in outlays for education and training is a promising example of this approach.

Reduction of poverty hinges on the attainment of other goals such as economic growth, full employment, income security, and equal opportunity. But it also turns upon the reduction of poverty itself since poverty to an important degree causes itself. Hence, any favorable break in the circle makes the next step easier. More nearly full employment makes barriers less meaningful; lower barriers shrink differences in motivation. Similarly, higher incomes for the poor work to reduce both acquired and at-birth limitations of ability.

But any one of the approaches will involve costs, and it would be valuable to know their comparative cost-benefit ratios. It is on this that, by theoretical and empirical research, including intercountry study, social scientists can make a distinctive contribution to the long-dreamed-of, but now explicitly stated, goal of eliminating poverty.

POVERTY: THE SPECIAL CASE OF THE NEGRO*

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Presumably, a Negro family receiving $2,400 annually would experience, because of such low income, discomfort identical with that experienced by a white family in exactly the same circumstances. Why then a special paper on Negro poverty? Because, in America, the white and Negro situations are never identical. Surely, because of discrimination, poor Negroes are psychologically more discomfited than poor whites. But economists do not investigate such discomfiture. Why, then, a special economics paper on Negro poverty?

Because at least five economic considerations distinguish Negro from white poverty. As Wordsworth observed of the echo, "Like,—but oh how different."

First, $1,000 buys less for a poor Negro than for a poor white.

Second, the demographic cross-section of the Negro poor is unlike that of the white poor.

Third, poor Negroes suffer though the general weal benefits from secular changes in urban renewal, education medians, agriculture, manufacturing location, technology, and social minimum wages.

Fourth, the effect of government transfer payments is different for poor Negroes than for poor whites.

Fifth, discrimination operates against Negroes to restrict access to education and to the jobs that can provide an escape from poverty.

These considerations will be discussed in turn.

Some Historical Perspective

When considering American Negro affairs, one must remember that social and economic conditions of Negroes are most responsive to unemployment rates. In 1900, 90 percent of American Negroes lived in the South, most on farms. The few urban Negroes were totally excluded from manufacturing and from all but menial and laborious jobs. The situation changed to the Negro's advantage only during German nationalism's wars. Wartime labor shortages induced managers of large manufacturing corporations to admit Negroes to the production jobs that permitted Negroes to make relative income gains.

During peacetime, the Negro position remained the same or dete-

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riorated. When labor markets softened between 1949 and 1959, the income position of Negro men relative to that of white men fell in every section of the country [1]. Rising productivity cut the number of whites and Negroes living in poverty, but the incidence of poverty among Negroes rose between 1950 and 1962 from 2 to 2½ times the white rate [26, p. 339].

The past decade's many admonitions and laws opposing discrimination could not raise the Negro's relative economic position in the face of rising unemployment. If Negroes are to approach economic and civil equality in the future, unemployment rates must fall.

Full employment affects all Negroes. Attention now turns to the characteristics distinguishing poor Negro from poor white Americans.

The Negro Dollar: Second-class Money

When citing statistics of poverty, the portion of Negro families receiving incomes below a particular figure, e.g., $3,000, is often compared with the portion of white families receiving incomes below $3,000. Such comparisons implicitly assume the Negro's $3,000 buys as much as the white's $3,000. It does not.

American cities have two housing markets: the city-wide white market and the circumscribed Negro market. Because supply is restricted, Negroes [33, p. 36] "received less housing value for their dollars spent than do whites. . . . Census statistics indicate that . . . non-white renters and home owners obtain fewer standard quality dwellings and frequently less space than do whites paying the same amounts." A Chicago welfare department study found [18, p. 13] "housing defects significantly greater for Negro than for white families, despite the fact that rents for Negro families are 28% a month higher than for whites in private dwellings."

Landlords are sometimes judged greedy extortionists for charging Negro tenants higher rents than whites. But they are operating in a market of restricted supply; high Negro rents reflect supply and demand relationships, not conspiratorial landlord greed. Since 15 percent of the consumption expenditures of urban Negro families is for shelter [27], real income is significantly reduced by relatively high rents.

Poor urban Negroes also pay more than whites for identical consumer durables bought on credit [3, pp. 12-20]. (Negroes pay more than whites for residential financing, too [31, p. 344].) The difference may be due to white reluctance to sell to Negroes (Becker's discrimination [2]), to Negro immobility, or to the sellers' assumption that poor Negroes are poorer risks than poor whites. Whatever the cause, real income suffers.

Poor Negro families average a half-person larger than poor white

families [11, p. 10]. Consequently, per capita real income of poor Negroes is even farther below per capita real income of poor whites.

If, then, $3,000 in Negro money buys only as much as $2,800 or even $2,500 in white money and is distributed over more people, one should keep in mind appropriate reservations when comparing percentage of whites with percentage of Negroes below some income level.

Differences in Demographic Characteristics

The Negro poor differ from the white poor in demographic characteristics. Remembering that Negro numbers will be understated, uniform dollar incomes can be used to identify nonwhite (not Negro) and white poor. Defining as poor, families with incomes under $3,000 and individuals living independently with incomes under $1,500 in 1959, four social-economic variables distinguish the nonwhite from the white poor.

First, the nonwhite poor are concentrated in the South. In 1960, 72 percent (52 percent)[1] of poor nonwhite families; only four of ten (27 percent) poor white families lived in the South (unless otherwise noted, all statistics in this section are from reference [25]). The 32 point difference in southern concentration resulted because, in 1960, the proportion of nonwhites was double the proportion of whites living in the South.

Second, low income is more of a rural phenomenon for whites than for nonwhites; 18 of every 100 (4 percent) poor white families, 12 of every 100 (3 percent) poor nonwhite families lived on farms in 1960. Fully 84 percent (79 percent) of nonwhite, only 44 percent (63 percent) of white farm families were poor in 1959, but nonwhites have withdrawn from farming more completely than have whites.

Third, the aging of husbands is a much more important cause of white than of nonwhite poverty. Other forces are important in causing nonwhite poverty. In 1959, 29 percent of poor white families but only 13 percent of poor nonwhite families were headed by a man older than sixty-four years. Among unrelated individuals, 40 percent of the white poor, only 26 percent of the nonwhite poor were past sixty-four.

Fourth, nonwhite poverty, far more than white, is associated with families headed by women. American Negro women have always borne exceptionally heavy family responsibility. In 1910 there were 20 gainfully employed white women for every 100 employed white men; there were 67 employed Negro women for every 100 employed Negro men [20, pp. 66-67]. Even in 1959, only 8 percent of white families but 21 percent of nonwhite families were headed by women. Three-fourths

[1] The figures in parentheses refer to individuals living independently.

of these nonwhite families were poor in 1959. Consequently, 32 percent of all poor nonwhite families, only 19 percent of all poor white families, were headed by women in 1959.

Urban Renewal, Shrinking the Supply of Dwellings

A lilting song of World War I charged

> It's the same the whole world over,
> It's the poor wot gets the blame,
> It's the rich wot gets the pleasure,
> Ain't it all a blooming shame.

Forces afoot today give the affluent society and even poor whites the pleasure while injuring poor Negroes. One of these forces is urban renewal. It replaces slums with aesthetically attractive, commercially profitable structures, some of which provide low-income housing superior to that which the private market could provide.

Yet urban renewal seems to effect a net reduction in housing supply for poor Negroes. L. K. Northwood found [32, pp. 107-08] "the supply of housing has been reduced in areas formerly occupied by Negro families. . . . 115,000 housing units were . . . planned to replace 190,500 . . . *a net loss of 75,000.*" Because many urban Negroes live in slums, 60 percent of the persons dispossessed by urban renewal demolition have been Negroes [31, p. 348].

The long-run tendency to reduce the supply of low-cost housing is aggravated in the short run because time must elapse between demolition of old and dedication of new buildings. During short runs as long as five years [8, pp. 132-33] urban renewal reduces housing supply by demolition uncompensated by new construction.

Poor whites may move elsewhere; poor Negroes must face reduced supply. Reduced supply should raise prices, and there is evidence that Negroes displaced by urban renewal pay rent 10 percent higher after relocation than before [14, pp. 72 and 82].

Until President Kennedy's November, 1962, executive order, the supply-restriction effect was even greater, for no federal rule prohibited urban redevelopers from practicing racial discrimination [30, p. 441]. The 1962 order alleviated the problem but could not end the irony that poor Negroes suffer from programs designed to promote urban welfare.

Education: The Illiterate Fall Farther Behind

E. F. Denison estimates [4, p. 73] that from 1929 to 1957 improved education "contributed 42 percent of the 1.60 percentage point growth rate in product per person employed." Improved education is

manifested in rising median school years completed. The 1950 Negro medians for men and for women, past age twenty-four, lagged white medians by 2.8 years. By 1960, Negro medians had pushed up a year and a third. So had white medians [26, p. 113]. Average Negroes remained in the same relative position, but rising educational medians increased the comparative disadvantage of the 2,265,000 nonwhite functional illiterates (less than five years of school) making up 23.5 percent of the 1960 nonwhite population past age twenty-four [23, pp. 420-21].

Many poor whites are illiterate, but figures on school years completed understate the number of illiterate Negroes and the size of their educational disadvantage. Understatements result for Negroes because so many attended inefficient segregated southern schools. Testing poor Negro literacy, Illinois departments of public aid recently sampled able-bodied Negroes aged 16-64 receiving public assistance (not a random sample of all Negroes). Each person was asked his school attainment; each took the *New Stanford Reading Test.* Of persons educated in Illinois, 3 percent were functionally illiterate; 35 percent tested as illiterate. Of persons educated in Mississippi, 23 percent were functionally illiterate; 81 percent, four of five adults, tested as illiterate [17, p. 118].

Of nonwhites living North or West in 1960, 41 percent had been born in the South [24, p. 2]. These educationally deprived poor southern Negroes are increasingly disadvantaged in regions where the median education of the local labor force and the quality of local schools rise each year.

Poor Negro boys are especially disadvantaged because of parental limitations and because their homes and the larger society offer so few successful men inspiring academic emulation. Special counseling and educational arrangements can offset those conditions and send slum boys to college [13, pp. 275-76], but society devotes few resources to such arrangements.

Left ever farther behind rising national educational norms, poor Negro families are ever less qualified to compete for jobs or to help their children acquire the education required to escape poverty.

Agriculture: End of an Exodus

Since 1945, the mechanization of cotton culture has revolutionized southern agriculture [16]. There has also been persistent change in crops grown and livestock raised [15]. These changes raised agricultural productivity and expelled hand labor from southern farms. In 1930, there were 882,000 Negro farms (with 4,680,500 residents). In

1950, there were 559,000 (with 3,167,000 residents); in 1959 only 265,000 (with 1,482,000 residents) [26, p. 618] [21, p. 599] [23, pp. 359-60].

The economy benefits as productivity rises. The effect on Negroes is less favorable. As whites left, the white farms that averaged 130 acres in 1930 grew to average 249 acres in 1959. But Negro farms showed little growth. They averaged 43 acres in 1930 and 52 acres in 1960 [34, p. 1035].

Change has not resulted in larger, more prosperous Negro farms. Change has expelled from southern farms the most ill-educated Americans.

Looking ahead, the Negro reservoir is nearly exhausted. The number of rural farm Negroes in 1960 was only 47 percent the number in 1950 [23, pp. 359-60]. The Negro exodus can never again approach the scale reached during the 1950's. Poor Negroes are already committed to the city.

Manufacturing Migration: Jobs Out of Reach

Since 1950, southern manufacturing has expanded more rapidly than northern. From 1950 to 1960, the number of manufacturing jobs grew 28 percent in the South, only 12 percent in the North [22, pp. 407-11] [23, pp. 730-32]. Because most poor Negroes live in the South and because Negroes' wartime income gains were based on accession of Negroes to production jobs in manufacturing, Negroes are particularly affected by shifts in manufacturing employment.

Manufacturing's southern migration to new markets and new sources of raw material [15, pp. 46-47] has distributed American resources more efficiently. It has taken jobs to poor whites but not to poor Negro men. Between 1950 and 1960, the number of jobs in southern manufacturing rose by 944,000. Of these 944,000 jobs, 12,000 went to Negro women (proportionately fewer than to white women); none went to Negro men [22, p. 410] [23, pp. 728-29].

Manufacturing: Technological Change Blocks the Exits

During wartime, rural southern Negroes proved themselves in manufacturing and developed vested interests in the growth of unskilled and semiskilled manufacturing jobs.

Today, technological change benefits all by raising productivity. It also changes America's occupational cross-section. In 1880 textile mechanization replaced skilled workers with unskilled rural immigrants [6, p. 63]. Negroes would prefer such changes today, but in 1964 skilled workers replaced unskilled.

In recent years, the occupations that during war gave Negroes a chance to get ahead have not grown as rapidly as the number of Negroes seeking work. Between 1947 and 1964, as male employment rose 10 percent, the number of manufacturing production jobs rose only 5½ percent [19, p. 3] [28, p. 14]. Between 1950 and 1960, male employment rose 6.9 percent; the number of semiskilled jobs in manufacturing rose only 4.1 percent [23, pp. 528-31].

Most unfavorable for aspiring unskilled poor Negroes, the number of men's laboring jobs in manufacturing fell 20 percent (by 200,000) between 1950 and 1960 [23, p. 533].

These changes in America's occupational cross-section result from technological developments that raise society's affluence; but, as present trends continue, manufacturing will offer fewer exits from poverty for Negroes handicapped by rural southern origins.

The Rising Social Minimum Wage and the Able-Bodied Unemployed

Two centuries ago, when sheep began gobbling up the people of England's countryside, the victims were deposited in cities in much the same condition as the untrained Negro migrants of today. The bold English peasantry, dispossessed from Sweet Auburn, could say of the city, "Thou found'st me poor at first and keep'st me so," but they were employed—though oppressively and irregularly.

Many Negroes transplanted to cities are unable to obtain steady work. Long's argument that America's social minimum wage rises above the marginal revenue product of society's least productive members [7, p. 16] applies especially to urban Negroes with rural southern antecedents. Law and respectable custom press upward on the social minimum wage. The general welfare benefits as many low-income persons receive more money and employers increase efficiency to offset higher costs [29, pp. 1139-40]. But the first increase in the minimum causes the discharge of the least able persons employed. Successive increases cause the discharge of successively more able persons among the less able employed [29, p. 1141].

It is the function of the market to choose technology appropriate to available resources as reflected in flexible resource prices. But the market does not operate below the social minimum. Weighed down with their heritage from the Southern Way of Life, able-bodied Negroes with marginal revenue products below the social minimum wage must either find employers paying below the minimum or depend on transfers.

So much for forces benefiting the general public but hurting poor Negroes.

Transfer Payments: Paternal Substitute and Golden Age Equalizer

For fifteen years, Negro unemployment rates have been double white rates [26, pp. 216-18]. This distinguishes Negro from white need for transfers, but does not distinguish poor Negroes from poor whites.

Respecting government transfers, poor Negroes do differ from poor whites because proportionately more Negro households have feminine heads and proportionately fewer Negroes are past sixty-four.

Relatively few Negroes receive OASDI (old age, survivors, and disability insurance). In 1962, 6.7 percent of the 12,500,000 recipients were nonwhite. This low figure was due to the nonwhite's shorter age span and the dissimilar work histories that led 73 percent of elderly whites but only 58 percent of elderly nonwhites to qualify [10, pp. 5 and 9]. In contrast, old age assistance goes to 38 percent of elderly nonwhites, 12 percent of elderly whites [10, p. 11].

OASDI brings elderly Negroes and whites close to income equality. For all persons, Negro income averages half of white income [1]. Yet the average income of nonwhites runs 80 percent the average total income of whites receiving OASDI [10, p. 5]. This happens because many Negroes continue in poverty while many whites sink into poverty after retiring.

Because Negro fathers so often decamp, Negro children receive a disproportionate share of ADC (aid to families with dependent children). Of 900,000 families (with 2,800,000 children) receiving ADC in 1963, 44 percent were Negro [9, pp. 3-4].

Per capita, ADC pays much less than retirement programs. Old age assistance meets 94 percent of the needs of the elderly; ADC supplies 58 percent of children's needs [12, p. 7]. Playing surrogate to absent fathers of poor Negro families, ADC never raises incomes or aspirations above levels at which the mothers' and absent fathers' "only legacy to their children is the same one of poverty and deprivation that they received from their own parents" [11, p. 12].

"The legacy of poverty" is a foreboding term. Seeking auspicious signs, at what points is Negro poverty most vulnerable to forces of improvement?

Prospects

Since poor Negroes pay more than poor whites for housing, any laws, changes of white hearts, or construction serving to increase the supply of housing open to Negroes will especially benefit the Negro poor.

Lengthening Negro men's lives, strengthening wedding bonds, and,

most important, improving job opportunities for Negro men will strengthen the poor Negro family. This will immediately add a male income earner to the family and will eventually induce male youth to look more ambitiously toward futures in the market economy.

During the 1950's, the numbers of men and of women of working age grew equally, but the number of jobs for women grew 34.5 percent, the number for men only 7.3 percent [23, pp. 528-31]. This was especially important for Negro women whose income rose relative to that of white women and Negro men [1, pp. 531-33]. The trend continues; between 1960 and 1964, the number of jobs for women grew fourteen times as fast as the number of jobs for men [26, p. 216].

Because half of southern Negro mothers of preschool children work [5, p. 9], mothers and children would benefit from preschool nurseries freeing the mothers for work and preparing the children for school. Because many receive ADC, higher payments or training schools for ADC mothers would particularly benefit poor Negroes.

Since poor Negro mothers are least informed regarding birth control [8, pp. 153-66], their education in this regard would permit great improvements in Negro health, real per capita incomes, and family manageableness.

Each year Negroes have less interest in programs aiding poor farmers, for farmers become more nearly all white each year. Contemplating old age, poor Negroes will benefit as urban migration and extension of coverage bring more under OASDI.

As social minimum wages rise, perhaps engineers will be able to provide employers with machinery that will combine profitably with the unskills of rurally educated southern Negroes. If not, they must depend upon transfers.

Finally, since southern segregated Negro schools have placed poor Negroes at a greater disadvantage than poor whites, since racial discrimination keeps qualified Negroes from demanding jobs, since weak labor markets remove the inducement that historically has been most important in helping Negroes score economic gains, the position of poor Negroes will improve dramatically in response to appropriate pressures at these three points.

Peroration

Because of discrimination in education and employment, there is one last important difference between the Negro and white poor. Logic rather than statistics suggests its existence. To begin, assume the innate ability distribution of Negroes is identical with that of whites. Next assume the inexorable winnowing out of those least able to earn

is the dominant cause of white poverty, but is only a partial cause of Negro poverty. It follows that poor whites are the least able whites, but that poor Negroes include those least able as well as many of middling to superior ability. These able Negroes are poor because of racial discrimination; society denied them access to the channels in which their earning ability could be developed and used.

The economist then concludes that the marginal efficiency of social capital invested in educating and finding work for the Negro poor could be much higher than the marginal efficiency of social capital similarly invested in the white poor. However, we know that the conversion of the poor Negro's potential into dollar product is very difficult in American society. The potential return is latent in the Negro poor. Able innovators are required if that potential is to be realized.

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DISCUSSION

George H. Hildebrand: These two highly relevant papers underscore a rather curious fact about the recent evolution of economic thought: that although we have accumulated a large and valuable body of fact about poverty, it has required the prodding of politicians, administrators, and advocates of civil rights to get academic economists to pay renewed attention to the scientific side of the problem itself. As Professor Schultz points out, this was not always so. Indeed, one need only to reread the latter parts of Pigou's *Economics of Welfare* to realize that at one time poverty was a reputable scientific question for the theorist—at least if, as in his and Marshall's case, his interests extended beyond a preoccupation with formal technique for its own sake. Perhaps there is a moral here: that economic theory reveals its greatest power when employed to illumine and explain important problems in actual life. If the history of science shows anything, it is that the great strategic hypotheses have all come to life because their proponents sought to account for otherwise puzzling observations.

Schultz's paper is written in this spirit and points the way to some fruitful uses of theory in the neglected poverty field. The fulcrum on which his analysis turns is the rise of per capita real income. This leads him to two ingenious suggestions. One is that the advance of nuclear family income encourages fragmentation of the unit itself, producing a statistical increase in the number of poor households because smaller units are more convenient for both young and elderly adults. The other is that our definition of the income minimum below which poverty exists and our willingness to provide transfer benefits to the poor both are positively associated with the rise of income per head.

Two points are in order. What we now call poverty turns not on some hard objective measure such as caloric intake, but instead upon broader, less tangible, and vaguely rising considerations of what a socially acceptable minimum ought to be. This is natural in a rich society but unrealistic for countries below this position. Beyond this, the now official $3,000 money income standards (1962 prices) for families and $1,500 for single persons, while useful in some ways, are too rough for technical purposes. They are inadequately responsive to differences in age structure and in number of dependents, and they make no allowance for income in kind.

Following up his earlier work, Schultz argues that investment in human capital plays a larger role than investment in material goods in bringing about the rise of per capita income. This, he suggests, upgrades the skill structure of the labor force both on the supply and demand sides and is the basic reason for the rise in earnings as a relative share of national income.

These ideas point to some modification of the highly enlightening neoclassical growth model developed by W. Arthur Lewis. There the decisive factors that couple rising income per head to growth itself are material capital formation, technical advance, and the exhaustion of the unlimited industrial labor reserve. Here the upgrading of the skill level by a different type of invest-

ment—in people rather than in things—becomes the critical strategic element. Perhaps a reconciliation can be had by recourse to temporal stages. Once material capital formation has already caught up with labor supply and has begun to generate an ever increasing social surplus, it then becomes possible to divert part of this surplus to ever larger outlays on education and training.

In the Schultz model, both the aggregate demand for labor and more specialized demands for skilled labor are positively elastic with respect to per capita income. This brings up the problem of unskilled labor. Does it also benefit from growth? Is it not possible that the demand for low-quality workers is negatively elastic with respect to income, as a kind of inferior good? And if so, does the employability of unskilled workers now and in the future turn increasingly upon widening rather than narrowing relative wage spreads in the sense of skill differentials of all sorts? The answer, I suggest, depends upon one's degree of optimism about the possibilities for nurture triumphing over nature; in short, for upgrading the skill potentials of all workers as growth proceeds. But this is a long story. In the meantime, perhaps we should be giving serious thought to methods for widening relative spreads once more. There are ways to do this without direct wage cuts: by greater reliance upon juvenile, learner, and apprentice rates, and by shifting from so much emphasis upon raising the federal minimum wage, keeping it uniform, and extending its coverage on uniform terms.

Like myself, Professor Lampman is a cautious optimist about improving man by intelligent changes in his environment. He correctly views poverty as the joint product of events and institutions, both largely beyond the control of people who are poor. Here he makes several informative observations, and I wish to enlarge upon them. Our present system of income security—public assistance, the social insurances, and the mechanisms created by collective bargaining—is too heavily weighted toward the interests of the successful middle class. This group distrusts public assistance and would keep it niggardly. More than this, as Milton Friedman has suggested elsewhere, a large part of government transfers to persons does not go to the poor at all. Furthermore, law and collective bargaining together have both created a "nonrandom slant" against the poor. Because of the growers' lobby, migratory farm labor has long been denied many statutory protections. Our $3 billion annual support price program is a subsidy scheme for big holders, doing virtually nothing for poor farmers and indeed actually damaging their interests. Urban and rural schooling for the poor is inferior in quality because of disproportionately low outlays per child and because of intentional gerrymandering of school zones. In the area of collective bargaining, well-paid union workers have erected a fortress of protections and income-adding advantages for themselves, of which the consequence is exclusion of many new entrants from opportunities for remunerative jobs. And so the poor workers end up in low-paid occupations having high disability rates or in unemployment. Thus the problem of unemployment today is largely one of youth and of the Negro. It is ominous and it is growing.

Both papers suggest that there are two basic strategies for attacking poverty. One is to increase government transfers to supplement low incomes, and

the other is to raise the earnings potential of the able-bodied poor. Both are needed, because roughly half the poor income units have no employable members. Traditionally, the transfer route has taken us into a diverse array of assistance programs for specially handicapped groups and to the more generalized social insurances. If we are to do more here, the choice lies between enlarging the established system of benefits and adding—or even possibly substituting—a new and far more general device, such as the family allowance or the negative income tax. Either of these new devices should be weighed against the alternative of spending more on provision in kind: better formal education, training, retraining, and literacy programs. If an allowance system could be devised that were confined to the poor and that gave incentive to attend school, it would have some merit, although it is my belief that more is required than attendance bonuses to overcome the malignant problems of drop-outs and poor vocational preparation. As for the negative income tax, it has great advantages and is, I think, superior to a family allowance. But it would neither produce equitable results nor avoid serious damage to work incentives unless it were very carefully designed. The rebate rate probably ought not to exceed 50 percent, and all forms of now excludable pretax income should be included in calculating income deficiency.

My preferred "policy mix" for dealing with poverty is to center the innovations upon upgrading earnings potential and to improve income supplementation through the existing benefit systems. The earlier manpower training legislation, the new Civil Rights and Economic Opportunity Acts, and the fiscal-monetary program to raise total demand are complementary approaches that all work to raise the earning power of the able-bodied poor. It is fortunate that they do, if only because this should end the sterile controversy over the causes and conquest of excessive unemployment. However, recognition of the need to do something directly about manpower has been belated. In Western Europe today the training and placement of the youth is acknowledged to be a responsibility of the entire community. Once that responsibility is accepted here, it will become possible to overhaul our systems of secondary education and apprenticeships, to fit them to modern times. If wage-price behavior in the future will allow us to continue to stress the side of overall demand as well, the vocational preparation of our youth will become meaningful, as the door to decent jobs. In this way, we may be able to conquer a lot of difficult problems together.

Harry G. Johnson: The papers presented in this session represent three economic approaches to the poverty problem. I would like to begin by suggesting that two other approaches—those of the economic historian and of the social philosopher—might be useful in placing the current concern about poverty in perspective. From the point of view of economic history, it is, I think, significant that this is the second instance of presidential concern about poverty in this century, and that in both cases the economic conjuncture has been one of unusually high and prolonged unemployment, suggesting that poverty is a stagnation-correlated fashion. It is also worth noting that the

maximum income required to be considered poor has been rising over the long run—a phenomenon not altogether happily conceptualized by Professor Schultz in an income elasticity—which implies that poverty is a socially relative category and that it may be extremely naïve to expect to make substantial permanent inroads on it. From the point of view of social philosophy, the poverty problem is largely a middle-class moral concern, and correspondingly programs for attacking poverty are conceived in middle-class terms and to a significant degree self-frustrating through concern for the preservation of middle-class values. Professor Lampman has made the point that existing social security serves more to block regression into poverty than to open escape routes from it; I would add the observation that contemporary thinking about poverty is dominated by the notion of elevating the poor into the middle class—hence the stress now laid on education as the key to the poverty problem—and is both seriously handicapped and forced into deviousness by the requirement that this elevation be accomplished in ways consistent with middle-class morality.

To be specific, poverty is invariably defined in terms of inadequacy of income (or more generally of disposable resources) to support a minimum standard of decent living. One might therefore naïvely suppose that the solution to the poverty problem would be simply to arrange income transfers to the poor on an appropriate scale. Such payments of social conscience money would not seriously strain the resources of an economy as affluent as this one, and they would have the advantage of eliminating a large part of the administrative overhead required by existing and proposed assistance and welfare programs. It is true that they would generate some waste, by making poverty more eligible than work to some people and by encouraging various kinds of fraud; but waste is one of the main uses of resources that an affluent society can and does afford, and given our society's tolerance of organized crime and of sharp business practices, the anxiety to prevent or punish fraudulent poverty is something of an eccentricity. It is also arguable that the experience of a decent income would itself have a powerful educative influence in inculcating middle-class habits and ambitions. Yet the simple solution of arranging adequate transfer payments is universally rejected out of hand, on the grounds that it would "impair incentives to work and save."

I am not arguing that poverty should necessarily be tackled exclusively by income transfers, but only that income transfers are dismissed summarily by arguments that either rely on a mythology of free enterprise that is inconsistent with concern about poverty, or depend implicitly on assertions about economic behavior that require more empirical investigation than they have been accorded as yet. This is my main quarrel with Professor Lampman's paper—which paper is on the whole a very useful, compact, and well-organized treatment of its subject. Lampman brushes aside "the idea that there has to be some given amount of poverty," and consigns to folklore a number of rationalizations of this notion. But in his own discussion of the approach of countering "events" that cause poverty—and specifically of the problem of childhood poverty—he is led by the same folklore to produce a compromise negative tax rate plan that would preserve poverty though ameliorating it by

reducing the poverty income gap. Lampman rejects "as unworkable any simple plan to assure a minimum income of $3,000." The term "unworkable" is a vague, practical sounding adjective, commonly employed to close off dangerous thought. What Lampman means by it is the testable but untested empirical proposition that $3,000 would cause many people to cease work, combined with the personal judgment that the resulting cost would be politically unacceptable. I am skeptical about the empirical proposition; but even if it were true, a rough calculation from Lampman's figures, made by assuming that all the present private income of the poor would have to be replaced by public transfers, produces a total bill of $27 billion, or 4½ percent of GNP, as compared with the poverty income gap of $12 billion or 2 percent of GNP. Four and a half percent of GNP may well be politically unacceptable; but it is really small potatoes as war finance goes, if war on poverty is really what has been declared. Lampman's alternative negative tax scheme is equally dependent on an untested proposition about incentives to work. It also ignores the strong possibility that the preoccupation of parents with the earning of money—especially in broken homes—is an important factor in the perpetuation of poverty among the children of the poor. I would myself prefer to recommend an explicit system of family allowances. More generally, I believe that in many cases of poverty income transfers would be the most efficient solution.

In conclusion, I offer a few brief comments. First, I suspect that if poverty specialists referred to modern growth models rather than the classical models based on the iron law of wages, they might be surprised by the slowness rather than the speed of progress in overcoming poverty. Second, it would seem to me desirable to work out what the prevalence of poverty would be exclusive of the effects of existing programs, to determine how far progress in reducing poverty has been automatic and how far it has been due to social policy intervention in the economy. Third, I would place more emphasis than Lampman does on the factor of discrimination—against Negroes, against women, against the aged, and against the uneducated—as a cause of poverty. Fourth, I concur in Lampman's emphasis on the interaction of causal factors in poverty; but this leads me to place much greater emphasis than he does on the importance of maintaining full employment for the reduction of poverty, on the grounds that a tight labor market is a powerful long-run solvent of discriminatory barriers to participation in the labor market at nonpoverty wages.

MALCOLM LIGGETT: Professor Batchelder has presented an impressive amount of evidence from primary and secondary sources with respect to the question at hand: the special case of Negro poverty. The time span considered was the postwar period, and usually the decade of the 1950's. In general, one is left with the impression that the recent decade has been marked by an absence of relative progress for the Negro. In fact, some of the indices suggest a retrogression, or at least the possibility of a retrogression in the near future.

To review the major items briefly: Batchelder indicates that (1) the rela-

tive income position of Negro men declined in every major geographical area of the country between 1949 and 1959, (2) the Negro's dollar buys less, (3) the general increase in educational attainment records has not benefited the Negro, (4) the geographical shift in manufacturing has been deleterious to the employment prospects of the Negro, (5) technological change generally has resulted in a changed composition of needed skills which works to the disadvantage of the Negro male, and (6) the community's sense of equity with respect to a social minimum wage impinges on the employment opportunities of Negroes. One would expect to find such a cataloguing of grievances in the preamble of a revolutionary document.

I wish to restrict my remarks to a set of points which suggest a somewhat different interpretation of the relative status of the Negro, especially as it has changed in the decade of the 1950's. There is one major reason for doing this: there is a great amount of agreement in the literature which suggests, as does Batchelder, that the Negro has made relative gains only under wartime conditions. First the disclaimers: there is no cause to question the evidence pertaining to the value of the Negro dollar, especially with respect to housing. Nor is there any cause for disputing the disparity in unemployment rates, the demographic differences between whites and nonwhites, and the suggestion that the changed composition of skills required by employers works a hardship on the recently rural Negro. However, alternative methods of presenting income and educational data reveal an impression left by Batchelder that is somewhat erroneous and potentially misleading.

The income data referred to are from Batchelder's recent article in the November number of the *Quarterly Journal of Economics*. These data are presented in the form of median income comparisons for the coterminous United States and four geographical areas. These figures show a relative decline in the ratio between Negro and white men in all four areas and stability in the ratio on a nationwide basis. Examination of the same type of income data when presented in another set of census classifications, "Rural Farm," "Rural, Nonfarm," and "Urban," lead to the same general conclusion of a slight decline between census years. Only one attempt at explanation of the decline in median incomes between these two groups will be made here. In the recent decade the percentage changes in the various income groups have moved against those who hold semiskilled and unskilled jobs. Of course, Negroes are heavily concentrated in these categories. Herman Miller has made this point very clear in his recent book, *Rich Man Poor Man* (p. 45).

TABLE 1

MEN'S INCOME BY OCCUPATION: PERCENT CHANGE

Year	Professional and Managerial Workers	Craftsmen	Semiskilled Factory Workers	Service Workers and Nonfarm Laborers
1939–61	243%	322%	331%	314%
1939–50	96	160	172	180
1950–61	75	62	59	48

Even though the median income comparisons generally indicate stability o decline over the decade, they may conceal an increase in the relative incom position of a portion of the Negro population in the higher income brackets Data are presented for four years: 1949, 1950, 1959, and 1960. These dat are taken from a recent monograph by Herman Miller.[1] If we lump the per centage of nonwhites earning $6,000 a year and over and divide this by th percentage of whites in the same category, we see the relative change in th nonwhite income position. (The lumping of nonwhites other than Negroe with Negroes does not upset the conclusion drawn here.) The index move from 8 in 1949 to 9 in 1950 and then to 17 and 18 in 1959 and 1960, respec tively.

This cursory review of income data reveals stability or decline in the rati of median income between Negro and white men. A major reason for this de cline must be the shift in percentage changes in annual incomes which has fa vored the higher income groups. However, the median income comparison conceal the evidence concerning a small, but rising, group of high-incom bracket Negroes.

Educational data are presented by comparing the difference in the numbe of school years completed by all persons over twenty-four years of age. Th conclusion was drawn that "average Negroes remained in the same relativ position." However, if we single out the younger age groups, we find that th difference in the number of school years completed has been narrowed in th past two decades. And the same is true when we examine the relative positio of nonwhites to whites in the upper end of the educational attainment data.

Income and educational data reveal upgrading relative to whites for a por tion of the Negro population. It is important to realize that the Negro commu nity has within it a group of well-educated and trained persons, and tha many of these were educated and trained in the period following World Wa II.

Batchelder makes the point that the regional shift in manufacturing ha resulted in an increase in manufacturing jobs in a region which is hostile t the employment aspirations of a large number of its inhabitants. Certainl this is true, but two qualifications are in order. First, his presentation fails t account for the relative decline of the number of Negroes in the South; henc his statement concerning the one-tenth of one percent of the new manufactu ing jobs going to Negroes is somewhat of a misstatement. Further, he did no indicate that manufacturing jobs in the West increased by a higher percentag and the same absolute amount as those in the South. While it is true that mor Negroes live in the South than in any other single geographical area, it is als true that the exodus continues.

I fear that I sound as though I am denying the existence of Negro poverty Quite the contrary, I believe that each point made by Batchelder is relevan to a portion, a frightfully high portion, of the Negro population of this coun try. However, I cannot agree that only wars have been beneficial to the up

[1] Herman Miller, *Trends in the Income of Families and Persons in the United States 1947 to 1960* (U.S. Government Printing Office, 1963), pp. 304, 306, 320, and 322.

grading of the Negro relative to the rest of the population.

The exodus from rural pursuits is moving poverty from the country to the city. In addition, we have in this country more than 2,200,000 nonwhite males over age twenty-four who have not completed a sixth-grade education. Many of these poorly educated, poorly trained, and recently rural persons now reside in our cities. The points made by Batchelder pertain especially to this group. However, the magnitude of the poverty problem is not overwhelming. Although we have the resources to handle this situation, it is not clear that we have the will.

ECONOMIC EDUCATION: EXPERIMENTS IN THE TEACHING OF ECONOMICS

THE EFFECTIVENESS OF "PROGRAMMED LEARNING" IN ECONOMICS*

By RICHARD ATTIYEH, *Yale University*
and KEITH LUMSDEN, *Stanford University*

This paper reports on an experiment undertaken to measure the effectiveness of a set of programmed materials which we and Sam Weiner, under the auspices of Behavioral Research Laboratories, designed for the teaching of economics at the high school level. Our objective in writing these materials was to help raise the standards of high school economics instruction and facilitate its spread. While there is little question that the greatest improvement in standards would come through an expansion in the supply of teachers who are well trained in economics, it is not clear that this would be either feasible or worth the cost. What the results of our experiment suggest is that with the techniques of programmed instruction, which economize on qualified teachers, it is possible to increase substantially the effectiveness of economics instruction.

The materials we prepared consist of seven programmed texts entitled *The American Economics Series*.[1] Taken as a set, they provide a comprehensive course in high school economics that is largely in accord with the recommendations of the National Task Force on Economic Education.[2] Taken separately, each text is a self-contained unit covering a specific area. The seven titles are: *The Free Enterprise System, The Gross National Product, Problems of Economic Stability and Growth, The Federal Reserve System, Taxes and Government Spending, International Trade,* and *Capitalism, Communism and Socialism.* Our goal for each of these programs was to give the student an understanding of fundamental economic concepts and the way they are used to analyze problems in the context of the U.S. economy. Thus, we stressed the analytic rather than the descriptive, although a substan-

* We would like to express our appreciation to Mr. Carl Poll, of Palo Alto High School, for making his students available for testing, to Behavioral Research Laboratories for clerical and technical assistance, to McGraw-Hill Book Company for providing the texts used in the experiment, and to Professor G. L. Bach for his helpful comments.

[1] Behavioral Research Laboratories, Palo Alto, Calif., 1963, 1964.

[2] See *Economic Education in the Schools: Report of the National Task Force on Economic Education* (Committee for Economic Development, 1961).

tial amount of institutional material was introduced in the treatment of economic policy.

A programmed text is essentially an exposition in which analysis proceeds by gradual degrees and in which continuous student participation, in the form of written responses, is required. In an ideal classroom setting each student advances at a pace most comfortable for his individual capacities, seeking assistance from his instructor when he encounters difficulty in the program. Under such circumstances, when a number of students complete a section of the program, it is desirable that they meet in small discussion groups with the instructor.

In this experiment the students who took our programs did not work under such ideal conditions. Specifically all students were obliged to proceed through the material at the same pace and no instruction by the teacher was permitted. Furthermore, the time allowed for completion of the course (ten weeks) was considerably less than normally would be the case. As a consequence of this time limitation, students were required to undertake major amounts of reading at home, and, for the last two or three programs, it was necessary to dispense with written responses.[3] While some of these conditions were imposed by the deadline for this paper, teacher participation was excluded in order to avoid any ambiguity as to the source of measured learning.

In order to measure performance before and after instruction, the students were administered two forms of a test for which a large national sample of scores has been collected. Each form consists of fifty multiple-choice questions covering a range of material roughly similar to that covered in *The American Economics Series*. This test—The Test of Economic Understanding[4]—was prepared by a distinguished panel of economists and educators headed by Dr. John M. Stalnaker. Prior to our study, it had been given to approximately 6,000 high school seniors from a cross-section of school systems. Of these, nearly 2,000 had taken at least a twelve week optional course in economics, the remainder having had no economics instruction at all. A summary of the scores of the national sample is shown in Table 1. Scaled scores are transformations of raw scores which permit direct comparison between the scores for the two forms.

The test sample is composed of fifty-eight seniors from Palo Alto High School, Palo Alto, California. These were divided into two groups, one taking form A, the other form B, as a pretest, and both groups taking both A and B as post-tests. Table 2 gives the test results for the Palo Alto sample.

[3] As revealed in a questionnaire, the students considered the programs to be far less effective when written responses were omitted.

[4] Science Research Associates, 1963.

TABLE 1

TEST RESULTS FOR THE NATIONAL SAMPLE

	Test Form	Mean Scores		
		N	Percent Correct	Scaled Score
With economics..........	A	927	61.0	20.10
	B	907	57.6	19.91
	combined	1834		20.01
Without economics.......	A	2307	48.7	16.52
	B	2294	48.0	16.67
	combined	4601		16.60

SOURCE: Science Research Associates.

How significant is the improvement in test scores shown in Table 2? The percentile rankings give a clear indication that the improvement by the Palo Alto students is substantially above average. If *The American Economics Series* were no more or less effective than conventional methods and if the average student with economics training in the national sample were no more or less able than his counterpart without training, then one would expect no change in the percentile rank of the Palo Alto pre- and post-test mean scores. That is, one would expect no difference between column (5) and columns (8) or (11) in Table 2. As it was, the Palo Alto pretest mean score ranked in the 62nd percentile among students without instruction, while the Palo Alto post-test mean score ranked in the 82nd (or 85th) percentile among students with instruction. Since for the national sample students with economics instruction are if anything the brighter group, the 20 (or more) point jump in percentile rank must be attributed to the above average effectiveness of the instruction given the Palo Alto students.

Further evidence of the effectiveness of *The American Economics*

TABLE 2

TEST RESULTS FOR THE PALO ALTO SAMPLE

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
		Mean Scores			Mean Scores After Instruction					
Pretest Form	N	Before Instruction			Same Form			Other Form		
		% Correct	Scaled	Rank*	% Correct	Scaled	Rank†	% Correct	Scaled	Rank†
A............	32	53.6	17.91	58	75.6	24.41	76	77.2	25.83	84
B............	26	57.0	19.65	70	79.6	26.50	88	80.0	26.04	86
Combined....	58		18.69	62		25.34	82		25.81	85

* Percentile rank among students in the national sample without economics instruction.
† Percentile rank among students in the national sample with economics instruction.

Series is given in Table 3, which shows the statistical difference between before and after test scores. Line 3 shows the average increase in scaled scores for the Palo Alto sample. Line 5 shows the average increase in scaled scores for the Palo Alto sample less the difference between the mean score for students with and without economics instruction from the national sample. Lines 4 and 6 show the appropriate "t" statistics. All of the entries in line 6 are significant at the .05 level of confidence. For the combined Palo Alto sample, which contains more information than either subsample taken separately, the mean difference between before and after scores is significantly greater at the .01 level of confidence than the difference between with and without instruction scores from the national sample.

A number of difficulties arise with the comparison made in line 5 of Table 3. Specifically, three biases influence the results. First, if, for the national sample, the average student with economics instruction is more intelligent than the average student without economics, then line 5 will be biased downward. Second, since there is a finite maximum to the possible scores that can be attained, the better a student does on the pretest the smaller is his possible improvement on the post-test. Since the average Palo Alto pretest score was above average, this factor also results in a downward bias in line 5. Finally, if for a given initial score more intelligent students will improve more, then, since the Palo Alto group appears to be more intelligent on the basis of the pretest scores, line 5 will be biased upward.

Some information concerning the effect of the latter two factors can be obtained from the cross-section of Palo Alto scores. Simple correlation analysis shows no significant relationship between change in score and the initial score. This suggests that the bias resulting from a finite upper limit on possible scores just offsets the bias from any conditional

TABLE 3

MEAN DIFFERENCES BETWEEN BEFORE AND AFTER SCORES
(Scaled Scores)

					Combined	
1. Test forms (before-after)	A A	A B	B B	B A	A A + B B	B A + A B
2. N	32	32	26	26	58	58
3. Mean difference	6.50	7.72	6.85	6.39	6.65	7.12
4. t	6.91	7.96	5.39	5.07	8.42	9.01
5. Mean difference less mean difference in scores with and without economics*	2.88	4.39	3.61	3.00	3.16	3.63
6. t	3.06	4.53	2.84	2.38	4.00	4.59

* Row 3 less appropriate differences calculated from Table 1.

TABLE 4
COMPARISON OF MEAN DIFFERENCES FOR NATIONAL AND PALO ALTO SUBSAMPLES
(Scaled Scores)

(1) Test Forms Before-After	(2) N	(3) Mean Differences: National Subsample	(4) Mean Differences: PA Subsample	(5) Mean Differences: PA Pooled Sample*	(6) Column (4) − Column (3)	(7) t	(8) Column (5) − Column (3)	(9) t
A A..........	30	3.71	6.50	6.65	2.79	1.89	2.94	2.12
A A..........	80	4.74	6.50	6.65	1.76	1.42	1.91	1.70
B B..........	158	2.18	6.85	6.65	4.67	3.43	4.47	4.81
B A..........	75	3.71	6.39	7.12	2.68	1.93	3.41	3.55
B A..........	182	3.69	6.39	7.12	2.70	2.00	3.43	3.72

* When before and after test forms are the same, the A A and B B results are pooled; when they are not the same, the A B and B A results are pooled.

relationship between intelligence and change in score. Thus, we can conclude that the results shown in lines 5 and 6 of Table 5, if anything, understate the relative effectiveness of *The American Economics Series.*

A third evaluation of the relative effectiveness of *The American Economics Series* is given in Table 4, which compares before and after test results for the Palo Alto sample with similar results for five groups included in the national sample. In each case the mean difference for the Palo Alto sample is higher. Unfortunately, these before and after scores are fragmented into five small samples. As a result the standard error of the difference between the mean differences is sometimes comparatively large and the Palo Alto mean difference is not always significantly greater than the national subsample mean difference at the .05 confidence level. There is no question, however, that if it were possible to pool these national samples and thereby permit finer discrimination between hypotheses, the Palo Alto mean difference would be significantly larger at a level of confidence well below .05. Some indication of the finer discrimination permitted by the pooling of samples is given by the increase in the value of the "t" statistics when the two Palo Alto subsamples are pooled. These results are shown in Table 4, columns (7) and (9).

A final test of the effectiveness of *The American Economics Series* can be made by comparing the scores of the Palo Alto sample with scores of college students who have completed a two-semester course in economics and school teachers who have taken the television course, "The American Economy."[5] The results are given in terms of raw

[5] These scores were reported in P. Saunders, "The Effectiveness of 'The American Economy' in Training Secondary School Teachers," *A.E.R.*, June, 1964, pp. 396-403; and, C. R. McConnell and J. R. Felton, "A Controlled Evaluation of 'The American Economy,'" *A.E.R.*, June, 1964, pp. 403-07.

scores in Table 5. The Carnegie Tech sample consists of 113 students who took the regular introductory course as sophomores. The University of Nebraska samples consist of 27 students who took the television course and 107 students who took a conventional university lecture course. The last group was composed of 71 school teachers who took the television course.

Unfortunately, the absence of preinstruction scores for these samples prohibits systematic comparison of improvement among these groups. One piece of information does indicate, however, that the rate of improvement by the Palo Alto group was somewhat larger than for the other groups. The mean score for a control group of school teachers who had no instructions was 67.8 percent correct. Thus the test group of school teachers scored 14.0 percent higher than the control group. This compares with an increase in scores for the Palo Alto group of approximately 23 percent (see Table 2). The main point, however, is that in terms of raw scores after instruction the Palo Alto High School group did nearly as well as the college student and school teacher groups; i.e., approximately 78 percent correct as opposed to 82 percent correct.

In the process of evaluating the test results a number of subsidiary questions were answered. First, how did the Palo Alto group answer different kinds of questions? Four classifications were made: simple, factual, theoretical, and policy. In all categories the Palo Alto group outperformed the national sample. However, the differences for simple and factual questions were considerably less than was the case for theoretical and policy questions. This reflects the emphasis we gave to the latter categories when writing the programs and, in addition, the comparative advantage that we believe programmed learning has in these areas.

Second, we were curious about the student reaction to these programs. Of fifty-six students answering a questionnaire, fourteen liked

TABLE 5

MEAN RAW SCORES FOR HIGH SCHOOL, COLLEGE, AND SCHOOL TEACHER SAMPLES AFTER INSTRUCTION*

	N	Percent Correct
Carnegie Tech	113	81.6
University of Nebraska (lecture)	107	80.0
University of Nebraska (TV)	27	81.8
School Teacher (TV)	71	81.8
Palo Alto High School	58	77.8
National High School sample	927	61.0

* All scores are for Form A only.

the programs "very much," twenty-four "liked" the programs, fourteen rated the programs "so-so," two disliked them, and two disliked them very much. It is interesting to note that the two who disliked the program very much were exceptionally bright students. One, in fact, earned a score of 100 percent on the post-test. Their main criticism of our programs—and this was echoed in various degrees by other students—was that there was too much repetition. In part this can be explained by the short period of time in which the programs were taken. What might be useful repetition after a period of time has elapsed would be of little value within a week or two. Also, a certain amount of repetition resulted from our desire to make each program an independent unit so that it could be used in classes where a complete course in economics was not desired. These two points notwithstanding, it is clear that great pains must be taken in writing programs to incorporate repetitive material with differences in approach or style to ensure maximum interest.

Among those who had a favorable reaction to the programs the most typical comment was that they liked them because the programs forced them to learn. In addition to the generally favorable reaction of the students, we are happy to report that Mr. Poll, whose students took these programs, plans to use them in future classes.

ROLE PLAYING IN TEACHING ECONOMICS

By Myron L. Joseph
Carnegie Institute of Technology

Teaching basic economics can be a very frustrating experience, particularly if your goal is to help students understand and formulate critical judgments concerning significant economic policy issues. For one thing, they arrive on the scene with many beliefs that they cling to with an unshakeable faith. Gold is, or ought to be, the basic source of value of our money supply. Government deficits are the cause of most economic problems and are to be avoided at all costs and under all circumstances. The unemployed are not ambitious enough to find work. Supply and demand and free competition make this the best of all possible worlds, and/or big businessmen set prices as high as they want to and get all the market can bear. The bureaucrats in Washington should not interfere with the economy, and we need less government spending, but the government should step in and do something about our serious social and economic problems.

Even more important as a teaching problem is student indifference; a compound of, "Why worry. Everything is fine the way it is," and "I couldn't do anything about it anyway." Students are reluctant to define issues in terms of broad social goals when they are so very much involved in trying to achieve their short-run personal ends. In their view a reasonable economics course should help them achieve financial security, and that is the critical test of relevance. Most of you have struggled to overcome these and other problems in the battle for the students' minds and interest. I do not think we can boast of our success. We may be able to achieve a reasonable distribution of answers on our final examinations, but the mature graduate frequently shows little evidence of his earlier economics education. In the micro area opinions are difficult to evaluate because conflicts of interest can be confused with lack of understanding of the price system. However, there is widespread belief that businessmen are entitled to a customary profit and that any failure to achieve this rate is sufficient evidence that prices should be higher. The gap in economic understanding is most dramatically illustrated by the automation panic, which spread rapidly in the absence of any substantial evidence that would justify the cries of alarm and in spite of the reassurance provided by historical data and economic analysis. The passage of the tax cut suggests that some progress has been made in the macro area, but I am unconvinced. Dur-

ing the recent political campaign I ventured off campus to discuss some of the economic issues with college educated audiences. From their questions and comments it was clear that they did not really believe the economy could stand a tax cut in the face of a deficit, or that the government could postpone repaying its debt much longer.

In his excellent paper[1] presented at the Association meetings last year, Leonard Silk quoted studies of public attitudes and statements of congressional leaders to show the "wide disparity between what the economists were teaching and what the public believed" concerning a tax cut. He went on to state his belief that "the same kind of public economic ignorance manifested on fiscal and debt policy could be demonstrated in many other areas." I am sure he is right.

But I do not have to go so far afield for evidence. My own students are extremely reluctant to apply the economic analysis taught in the classroom to problems of any complexity. Recently a relatively new instructor told me how disappointed he was with his students. He had been sure they were able to handle the theory he had been teaching, and they had performed successfully on several homework problems and quiz exercises. But when he assigned a policy problem, his students wrote at great length with no evidence that they knew any economics at all. Their performance unhappily resembles the behavior of our graduates. They shed with ease the theory we labor so hard to teach, and all that remains is a smattering of half-remembered terms. Once again in the real world, they know what is right without the aid of economics. My anecdotal evidence and Mr. Silk's conclusions appear to be supported by the preliminary results of a nationwide study conducted recently by two of my colleagues, Professors Bach and Saunders. Some preliminary findings of theirs suggest that one or even two economics courses leave little or no trace a few years after students have left the classroom.

If our graduates do not understand and remember economic analysis or accept it as relevant to public policy issues, we have failed. Whatever the cause, we are not having a strong enough impact on our students. Without ruling out the possibility that the subject matter of our courses may require critical reexamination, I suggest that we should focus more explicitly on the learning process.

If we are to convince our students that economic analysis will help them understand their environment, we shall have to aim for more than an intellectual understanding of the subject matter. Intellectual learning is not enough; economics must be accepted by students before they will use it outside the classroom. When I first started to teach I

[1] "The Problem of Communication," *A.E.R.*, May, 1964, p. 596.

was told the story of a student whose reaction to an exposition of the fractional reserve system was to advance on the instructor with intent to do bodily injury. He evidently felt his world was being attacked by the nonbelievers. The example may be extreme, but it illustrates the emotional barriers to communication that block the understanding and acceptance of economics. If our teaching is to have any effect, we must change strongly-held beliefs that may not be susceptible to reason alone. Student acceptance of economics may require more concern with persuasion and effective communication than most of us have shown. We must do more than demonstrate the cold, clear force of our logic in the traditional authoritarian pattern of handing down the truth from on high. We must experiment with new teaching techniques and take whatever steps we can to help economic learning survive beyond the end of the semester.

There are many ways to avoid the limitations inherent in the one-way communication pattern of the traditional lecture. The Socratic discussion technique, student projects, and debates all help to involve students in the learning process, so that they become partners rather than antagonists of the instructor. There is a great deal of evidence that under some conditions learning and acceptance may be improved by active student participation induced by role playing or by other devices. The mediating mechanisms are not completely understood, but group cohesion, heightened attention, improved comprehension, and the emotional content of the experience are among the factors that may be involved.[2]

I have tried to provide a number of participative learning experiences for students by placing them in role-playing situations. The teaching experiments I will describe were conducted over a number of years in elementary economics and labor economics classes. My excuse for presenting them to you is that they appeared to have had a stronger impact on my students than anything else I have done. The role-playing experiments heightened student interest and facilitated learning and acceptance of unfamiliar concepts. They provided a basis for repeated reenforcement as students referred back to their role-playing experiences when related subjects were discussed in class. The active student participation in the experiments helped to break down the barriers to effective student-teacher communication. I hope they will suggest new ways to approach the problems of economic education.

[2] Cf. Carl I. Hovland, Irving L. Janis, Harold H. Kelley, *Communication and Persuasion* (Yale Univ. Press, 1953), particularly Chap. 7, "Acquiring Conviction Through Active Participation," and the bibliography that follows, pp. 215-40; Dorwin Cartwright and Alvin Zander, *Group Dynamics, Research and Theory* (Row Peterson, 1960); David Rappaport, *Emotions and Memory* (Williams and Wilkins, 1942).

Market Price Determination

Students have a very difficult time understanding the concept of a market price. There is no problem with the algebra or graphics, but students perceive a startling difference between the world as they know it and the economics text. Everyone knows that the seller sets the price for his product; so what sense does it make to assume that the competitive firm must accept the market price as given. The student can see that market price as we define it equates supply and demand and clears the market, in the rather special sense in which we use that phrase. But he finds it very difficult to translate the nicely intersecting curves into something that can be applied to the world outside the classroom. The result, in my experience, has been to make the student highly resistive to analysis that depends on market determined prices. In an attempt to make market analysis more meaningful, I constructed a highly simplified market and gave the students a chance to participate in the determination of a market price. In preparation for the market I made up buy and sell instructions that were based on demand and supply functions that intersected at a price of $1.80 per bushel.[8] The instructions were to "Buy 1,000 bushels of wheat for *not*

TABLE 1

DISTRIBUTION OF BUY AND SELL INSTRUCTIONS

Price	Buyers (Not More Than the Price)	Sellers (Not Less Than the Price)
$2.80	4	—
2.60	4	2
2.40	4	2
2.20	4	2
2.00	4	2
1.80	4	4
1.60	2	4
1.40	2	6
1.20	2	6
1.00	2	4

[8] These figures are taken from one of many markets conducted since 1959. They differed in detail, but the same general procedures were followed in each case. My interest in the possibility of simulating a market was first stimulated by E. H. Chamberlin's article, "An Experimental Imperfect Market," *J.P.E.*, Apr., 1948, pp. 95-108. In 1952 Professor Vernon L. Smith reported on a series of experimental games designed to study some of the hypotheses of neoclassical competitive market theory. ("An Experimental Study of Competitive Market Behavior," *J.P.E.*, Apr., 1962, pp. 111-37.) His experimental procedures differed substantially from those used in our informal teaching experiments. His subjects were not able to interact and bargain freely, and each transaction removed a set of buy-and-sell instructions from the market without replacement. Consequently his market conditions did not remain relatively stable during a trading period. Instead, the trading periods continued until bids and offers no longer resulted in contracts, and the market was repeated with experimental conditions held constant over several successive trading periods. This procedure permits the orderly collection of data but does not, in my view, provide as close a simulation of the equilibrating process as the procedures used in our experiments.

more than," or to "Sell 1,000 bushels of wheat for *not less than*" the specified price. At the $1.80 price twenty-four transactions were possible and eight buyers and eight sellers were excluded from the market by their instructions.

Two classes were brought together in a room in which all seats had been pushed to the walls. One class was designated as buyers and the other as sellers, and the following instructions were distributed:

You are about to participate in the operation of a commodity market. You will be given an order to buy or sell 1000 bushels of wheat under certain conditions. In general you should not reveal your instructions to any of the other dealers, unless you have a particular reason for doing so. You should consider yourself to be an agent, acting in behalf of a client who has given you specific instructions. You have an obligation to do as well as you can for your client, and you are not permitted to violate the instructions.

When the market opens, at the signal of the instructor, you may proceed to carry out your order. Buyers will be identified by a handkerchief on their left arms. A transaction is completed when a single buyer and a single seller agree on the terms of a sale. As soon as you complete a transaction report to the instructor so that he may record and report your transaction. As soon as your transaction is reported, you should turn in your buy or sell order and receive a new one of the same kind. You may proceed immediately to complete a new transaction in accordance with your new order. If you are unable to complete a transaction within 10 minutes, you may obtain a new order from your instructor

When the market is closed, the instructor will determine and report whether the buyers or sellers have represented their clients more successfully.

When the students had a chance to make sure they understood the instructions, the market was started. They were free to circulate in the class and make purchases or sales at any time, as long as the transactions were consistent with their buy and sell orders. As soon as a sale was made, it was reported to the instructor, who recorded the sales price on the blackboard and announced it to the class. The students then turned in their orders to the other instructor and received replacements.

A few of the buy and sell instructions were left over at the beginning of the market, and they were given out as transactions took place and instructions were turned in. In the process of redistribution, the orders were shuffled so that the students could not know when they received a new order in what transaction it had been used previously. The process maintained a fairly constant set of market conditions over the market period, although there were undoubtedly some shifts caused by the lag in reporting sales and feeding orders back into the market.

A record of one of these experiments has miraculously survived and is reproduced in Table 2.

After an initial scattering of transactions, prices tended to move quickly toward the theoretical equilibrium. By the time the period was over almost all transactions were at or very near to the equilibrium price. In subsequent experiments we recorded the transactions for each ten-minute period on a separate section of the board. The students

TABLE 2

DISTRIBUTION OF TRANSACTIONS

Price	Number of Transactions
$2.60	1
2.50	0
2.40	1
2.30	0
2.20	6
2.10	1
2.00	15
1.90	25
1.80	27
1.70	20
1.60	16
1.50	13
1.40	5
1.30	1
1.20	3
1.10	1
1.00	1

were then able to observe the changing distribution of prices over time. My impression is that this technique speeded the concentration toward the equilibrium price.

One surprising aspect of an experiment of this type is how seriously the students take their roles. They bargained vigorously and were very anxious to learn which class had best represented their "clients" when the market was closed. We were forced to permit anyone who did not complete a transaction within ten minutes to obtain a new order when we discovered in one of the early experiments that frustrated students, whose instructions effectively excluded them from the market, finally ignored their buy and sell orders in order to participate actively.

When the market was completed, students were asked to report at the next class meeting on the differences between their market and the competitive market discussed in the text. The follow-up discussions demonstrated that they had been impressed by the coercive force of the market on the individual participants. They had learned, through their own experience, how supply and demand determine market price. With this background they were able to assess the significance of such factors as information, factor mobility, and product homogeneity on the market process.

The experiment made a strong impression on the students, and they referred back to their market experience throughout the semester. It helped them to understand the role of market forces in determining price and production and to comprehend the competitive firm's inability to deviate from the market price. The acceptance of these concepts as something more significant than an intellectual exercise provided a valuable base for the comparison of competitive markets with alterna-

tive market structures and for related policy discussions. These are, of course, my subjective evaluations of the experience, since we did not run a controlled experiment. However, I am convinced that students who participated in these markets will remember the experience and the major ideas that were developed from them long after the analysis that was taught more traditionally has ceased to have any real meaning.

Oligopoly Pricing

The pricing problems of large firms are remote from the experience of the undergraduate students. We designed a role-playing situation that let them experience the instability of an oligopoly and the pricing problems faced by a firm with a small number of large competitors. Through participation they learned of the strong temptation to cut prices when fixed costs are high and of the impact of price competition on oligopoly profits. They were able to feel the pressure to collude.

As background for the experiment we described an industry with three identical firms competing for the market. The demand for the product was assumed to be relatively inelastic, and marginal production cost was assumed to be low relative to the prevailing price, and constant. We explained that firms could choose to sell at a high (H) price of $4.00, or at a low (L) price of $3.00. Under these assumptions the class estimated the distribution of sales among the firms for each combination of high and low prices. Then, using an explicit cost function (costs = $65 + sales × $1.00) they calculated the profit that would result from each price pattern. The sales and profits alternatives for all combinations of high and low prices are presented in Table 3.

A simplified table showing only the profit alternatives for different price combinations was placed on the board. The class was arranged in groups of three, each student representing a single firm. They were asked to study the profit possibilities and to try to make as much money as possible for their respective firms through a series of fifteen or twenty price setting (high or low) decisions. The instructor prom-

TABLE 3

SALES AND PROFITS FOR COMBINATIONS OF HIGH AND LOW PRICES

Prices HHH	Price	Sales Units	Profits	Price	Sales Units	Profits	Price	Sales Units	Profits
HHH	H	30	$25	H	30	$25	H	30	$25
HHL	H	10	−35	H	10	−35	L	80	95
HLL	H	5	−50	L	50	35	L	50	35
LLL	L	35	3	L	35	3	L	35	3

(Costs = $65 + sales × $1)

ised to identify the most successful businessman in the class at the end of the experiment. The students within each group were instructed to make their price decisions independently and without any communication. After each of the three had selected and noted a price, they were told to reveal the decisions to each other and to record the profits they had earned as indicated by the table on the board. This procedure was repeated until the experiment was concluded.

One-third of the groups were asked to set the initial price for all firms at the high level, one-third at the low level, and one-third were not given any binding initial conditions. The predominant result was that all three firms were forced into the low-profit alternative. Most of the groups that started selling at the high price fell off the plateau and were unable to return to it. The other groups were rarely able to attain the uniform high price structure. Although the students were asked not to communicate there were many audible comments concerning the cupidity, stupidity, and ancestry of the participants. After the students had made about fifteen to twenty price decisions the experiment was terminated and the frequency of different price patterns reported to the class. The experiment was then repeated with instructions permitting the students in each group to communicate freely before each price decision. Under these conditions a majority of the groups succeeded in maintaining a uniform high price level for fourteen of the fifteen price decisions. But a substantial number were unable to collude or found that their agreements broke down after a few decisions.

These were not controlled experiments, and I make no claim that they shed any light on the behavior of oligopoly price-makers. But they did give the students an intense experience that made the economic analysis of oligopoly behavior more meaningful. The students analyzed the experimental conditions and assessed the importance of demand elasticity, product substitution, communication, the cost function, and their motivations in explaining their role behavior. They saw collusive agreements break down and felt the frustration of not being able to raise a price when it was clearly in the interest of all firms in the industry to do so. After their experience it was not difficult for them to understand the role of a price leader or the significance of tacit collusion. The experiment and the discussions that followed provided a dramatic background for the analysis of antitrust policies in a learning context very different from the traditional authoritarian presentation of subject matter. An experience of this kind helps to break down inhibitions and communication blocks in the classroom. The entire course gained through increased interest in the subject matter and the students' involvement in the learning process.

Labor-Management Disputes

Role playing can be used to help students understand the attitudes and perceptions of individuals whose points of view differ substantially from their own. Students from upper- and middle-class families find it difficult to understand the basis for conflict between union and management. There is a tendency to believe that one party (usually the union) does not really understand the situation, and if only the facts could be clarified, most disputes would disappear. I have made use of detailed case material to provide a context for student participation in grievance negotiations in union and management roles.[4] The case material is distributed and students assigned in groups of six or eight to roles as members of a union or management committee. Each group is asked to meet for not more than two hours to attempt to resolve the dispute described in the background readings. They meet without supervision, and from reports I have received, they seem to throw themselves into the assigned roles with enthusiasm.

At the next class meeting I ask for an oral report from each group. A representative of one of the committees volunteers to summarize the discussions and whatever partial agreement or definition of the issues his group arrived at. The volunteer is usually chagrined to discover that members of the opposing committee are unwilling to accept his summary and insist on presenting their own view of the negotiations. There are frequent disagreements over the details of settlements and on some occasions over whether any agreement was actually reached. When all the groups have reported, but before any general discussion of the substantive issues, I ask the students to step out of their roles and indicate by secret ballot which side of the dispute they believe was right. When the votes are tallied there is some surprise expressed at the substantial vote for the union position. I then ask the students to indicate how they voted, and whether they took a union or management role in the negotiations. On each occasion that I have used this exercise a substantial fraction of the students who took union roles voted for the union position, and almost without exception those who played management roles voted for management.

The students learned about the process of negotiation and improved their understanding of the substantive issues in dispute. But the most significant impact of the experience on the students was their realization that the role they played affected their perceptions and evaluations of the situation.

[4] There are published cases that can be used for this exercise. I have found the Rocket Chemical Company (C) case (ICH 5H16 HP 555), published by the Harvard Business School, particularly effective.

Summary

The role-playing experiments I have described were all designed to accomplish learning through an experience that was at least in part emotional. They depended on self-learning and on active student participation in the learning process. The major concepts and ideas that were developed through the experiments were not complex and in each case could probably be set down in relatively few words. These experiments and others like them involved a substantial investment in student time that could have been used to present additional subject matter. My view that investments of this type are worthwhile is based on a judgment that our highest priority task is to increase our impact on students. We must distinguish between teaching and learning. It may well be that if we taught less, the students would learn more. I feel somewhat timid about exposing this possibility when educational standards are accelerating all about me. I am sure that for many the best path to learning is through the intellect without the aid of special reenforcement techniques. But I am afraid that if we hope to make our subject matter relevant beyond the final examination, we will have to address ourselves more explicitly to that end. We should judge our performance by whether our students have learned and retained the basic concepts we have to offer, not as an exercise in not too difficult mathematics, but as relevant economics.

Role playing and other participative techniques appear to offer the possibility of deepening the learning that takes place and of increasing the acceptance of economics. Students seem to remember their role-playing experiences long after they leave the class, and my experience indicates that student involvement can improve our teaching through better comprehension, stronger interest, more self-learning, and improved student-teacher communication. My modest experiments suggest only that participative techniques can be used effectively in economic education. Certainly this is one path we should explore in our attempts to make economics more meaningful. However, we should experiment more systematically to determine the best uses of such techniques and to discover new ways to reenforce and supplement our teaching.

TESTS OF THE SUCCESS OF THE PRINCIPLES COURSE

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Although this paper draws on student questionnaires and letters from experienced teachers, it relies chiefly on true-false tests given to 32,000 students at eighty colleges since 1954. These were described in the March, 1960, *American Economic Review,* and criticized and defended one year later. All twenty-two sets, of fifty each, have been recorded for at least eight classes in principles at the start and at least ten after the course. Copies of some of the sets will be supplied by the author to any teacher requesting them, and, next summer, all sets if possible with grades for individual questions. Users can then make their own selections.

The comparisons today are confined to the 103 men's and 37 women's classes, in forty-four colleges, which took them both before and after the course. The mean grade of the 140 advanced from 54.1 to 62.3 percent, or from about twenty-seven to thirty-one out of fifty (twenty-five being the equivalent of zero). This is not self-evidently poor, without standards to show what improvement ought to be, or that these particular questions reflect what the course should teach, but it is certainly not impressive.

Of the forty-four colleges, two from the "Ivy League," though with only one class each, recorded an average gain of 31 percent of the possible improvement between their beginning scores and 100; five liberal arts colleges whose names are well known, with eight classes, a 21 percent gain; fourteen state universities, seventy-three classes, 18 percent; thirteen large private universities, twenty-nine classes, 16 percent; and ten colleges and small universities known mostly in their own states, twenty-eight classes, 13 percent. Averages in the South were in most cases lower than those elsewhere.

The range among the classes was from 40.5 and 37.4 percent of the possible gain down to two slight losses. The mean improvement was 17.9; the median, 17.5; the quartiles, 23 and 11.8. Final grades, which ranged rom 73.1 to 47.8, might have been made the measure of success, thus including forty-three classes tested only at the end of the course, but the percentage of potential gain achieved seemed preferable. (Other alternatives would be the numerical, or the percentage, advances from the beginning grade.) There are now enough precourse

scores to permit schools testing only at the end to have some idea of the starting point.

Women began 1.4 points below men at the same universities, and ended 2.6 below; they gained 15.8 percent as against 19. Those taking high school economics (pre-Task Force) started 1.6, or eight-tenths of one question, ahead. Five colleges offered both one and two-semester courses; advances in the former ranged from 50 to 81 percent of those in the latter, the mean being 62.

Seven areas are covered: production and public finance have five questions each, and the others, eight each. Gains, on the first 14 sets, have been: production (which includes basic concepts, costs, and business organization), 23 percent of possible improvement; income-employment, 21; money, 20; prices and international economics, 18; public finance and distribution, 16. For example, only 37 percent before the course but 75 after it (these are medians of the class averages) recognized as fallacies that a free enterprise system implies the right of sellers to confer on prices to charge, rediscount policy is the best credit control tool, tariffs should keep out goods produced at lower wages, governments should fight recessions by balancing budgets, and stronger American unions account for higher wages than in Europe. Not many of the questions fit into the three categories—factual, conceptual, and problem-analytical—of the McConnell-Felton article in the June *A.E.R.* The majority deal with cause and effect or policies and few use technical terms.

The eight for which scores declined the most during the course fall into two groups. In one, the early commonsense view gave way to a slight majority accepting the fallacies stated (grades dropping from 64 to 45): that (1) advertising cannot reduce prices, of "certain" products, by leading to mass production; (2) the greatest inflations have resulted from private sector credit expansion; (3) interest rates decline during booms; and (4) a rise in internal public debt need never cause worry. On four questions, an original guess (51 percent right) became a general error (32 percent): that (1) $1.00 deposited in a banking system keeping 20 percent reserves will add $5.00 to loans and investments; (2) advanced countries have raised most of their capital abroad; (3) a percentage tax on profits of a monopoly exploiting its position fully is easily shifted to consumers; and (4) our money supply has increased only 2 percent annually since 1939.

Although we may oversell our views, there is some progress in dispelling misconceptions on the same topics. The percentage believing that advertising and selling are more constructive than price competition decreased from 52 to 37; and the belief that public debt should not exceed national income, from 66 to 34. Some guidance to teachers

on where and how to put their emphasis is the first benefit I would expect from such tests.

A second gain can be an evaluation of the large lecture class, with graduate assistants for its recitation periods. Inquiries located only two universities which had attempted this. At one, the lecture section outscored the remaining discussion sections by half a point to a point on about 140 questions in three successive years. At the other, the lecture sections gained 14.8 points, the discussion groups 16.3, on question set A from the March, 1960, *A.E.R.* The small sections scored significantly better on all 19 attitudinal questions addressed to the students. This university also tested its history classes: the small groups led significantly on 5 of the 19 and were slightly ahead on 12; but they progressed only 36 percent (from about fourteen right at the start) toward answering all thirty multiple-choice questions as against 44 percent for the lecture group. Covariance analysis, taking account of beginning scores, verbal-mathematical test scores, and the individual section deviations, found these differences nonsignificant. It should be noted that the lectures were still in the experimental stage, and that differences among the teachers were ruled out in the experiment.

These data tend on the whole to confirm the conclusions of W. J. McKeachie in Nevitt Sanford's *The American College* (Wiley, 1962). Eight experiments had shown as much or more information retained by lecture students, although four had found better attitudes or superior problem solving in small groups. Since the one virtue of the big class is supposed to be its economy in professorial time, it is gratifying to find that the students can learn as much, too. The lecturer puts more effort into preparation and his presentation is better organized than in loosely-run discussion sections. Whatever the advantages of the lecture, the small group keeps its superiority for training in thought and expression—provided it is not made a straight lecture itself.

Raymond T. Bye, of Pennsylvania, in a letter, stresses the Socratic method as learned from Taussig. Ira B. Cross, of California, writes that the big class cannot succeed unless graduate assistants are chosen and trained with the greatest care. Both—and others still teaching whom I shall not name—insist that success of the competing approaches depends primarily on the personalities involved. A comment relayed from one university runs: "Course is fine, Dr. X is great, get rid of ignorant graduate students." But sometimes it is the other way around.

Four further teaching suggestions have support in the true-false experiments—two minor ones: to keep lecture and reading in step and to avoid excessive abstraction; and two major ones: emphasis on adequate student preparation and on student thinking.

The first point is an inference from the experience of two professors at a small college who gave the published questions. One, described as a good lecturer giving a planned survey of theory, not coordinated with textbook reading, secured from his students 14 percent of the potential improvement. His colleague, called a "drillmaster" who stayed right with the text in his recitations, came up with 27. I tried to test this point and others in a survey of 1964 summer courses. Grades ranged from 12.3 percentage points above the previous scores for selected questions to 11.5 points below. The six schools with highest scores said they had kept class work and preparation in step daily; only 6 of the 11 lower-ranking schools so claimed. On the same point, several teachers who have administered the test at the start, but not at the end, explained: "I was just too far behind to spare twenty-five minutes."

The second suggestion may be put in the words of a teacher whose gain in the true-false scores, 29 percent, had been among the best: "I am near enough to my student days to remember how repellent too much abstract theory is to the average non-mathematical, non-scientific beginner. Illustrations keep the student listening and give the principle the support it needs."

A third, and important, suggestion is greater emphasis on class preparation by the students. Six of the seven summer courses with highest grades, but only four of the other ten, required more reading than just the textbook. This sample is small (106 colleges announcing a summer principles course had been addressed to secure thirty-four acceptances and seventeen actual scores), but there is something to think about in the letter from a college whose students were found to be devoting little more than half the time to economics as to English composition, foreign languages, natural sciences, or mathematics. The usual explanation was: "We need two full hours to prepare these demanding subjects, but we can read our ten pages of Samuelson in an hour." Let me review briefly five approaches to this problem:

1. Workbooks. In the summer survey, four colleges required these. Seven made them optional, mostly guessing that 20 or 25 percent took the option. I inquired more closely at two other schools known to have been using them, but one proved to have dropped out because of student copying. At the other, nine students checked "used it very little" and "not worth the effort." Two conscientious students found the workbook helpful, but were not asked whether the same time spent in study might not have helped as much. Problems are valuable, and some colleges can afford graduate assistants to check the papers, but the dilemma raised by copying remains.

2. Supplementary readings. These are popular and useful, but there are two difficulties. Some students, though not all, need to spend their

available time mastering central points rather than pursuing interesting supplements. Many professors simply assign the readings and then ignore them. As for "recommended" readings, I was enlightened by learning after a semester that not one of sixty students had looked at any.

3. Reports and term papers. Only two of the seventeen summer courses required these. Most teachers lack time to correct them and want them postponed to later courses whose members know more economics. James Harvey Dodd, of Mary Washington, has been an exception: he writes that papers on personal or community economic problems were one means of interesting his students in economics.

4. Two textbooks. Eleven colleges reported this solution in the Kopf-Stauss survey, *The Teaching of Elementary Economics* (Holt, 1960). One school, which entered two classes in these tests, ranked eighth out of forty-four. Its aim was to increase study time on basic material, and this seemed more likely if a second text was added than if students were asked to keep on poring over the first.

5. Forcing more thorough study through stiffer grading or short daily quizzes, such as five true-false questions. One teacher released his better students from daily preparation after one term and witnessed a decline in their grades. Experts in education agree that "distributed learning," with regular preparation, is more lasting than "mass learning" through examination cramming. The lecture or discussion can also be less elementary when most of the class is prepared.

The fourth lesson from the tests, obvious but vital, is the need of training in constructive and critical thinking. The men's college scoring highest said that it emphasized step-by-step solution of problems by students; the women's college relied entirely on discussion, with classes from twelve to twenty-three in size.

In one experiment, each problem area was discussed a day ahead of the textbook reading. Student ideas were written on the blackboard and erased when proved wrong. The residue was improved; then mimeographed to be the class notes. To keep the thinking original, evident acquaintance with the text was penalized, postponement of reading to the assigned day was encouraged by testing on it then, and no grades were given on discussion. This class surpassed others on the examination. Unfortunately, the strain on the teacher and on the office ended the experiment.

Many discount these tests because they distrust objective questions, arguing that they do not test powers of reasoning or expression. True, a few students may win high grades by lucky guessing, and essays are indispensable to test expression, but a class average cannot be high on fifty questions if reasoning is not used. The fear that brighter students

will go wrong by thinking of qualifications was not sustained when 1,136 papers were checked. The 182 students scoring 76 or higher surpassed the rest on every single question. It must be granted that some questions in each set are inferior to the rest (all but a dozen or so have been kept so as not to lose the record of experience). Finally, some questions will be on topics not covered by a particular teacher or book.

For all this, classes still take the test on an equal basis, and a score of 76 to 80, depending on the set, can be treated as an A. Ninety is about the highest that any one can achieve with only half a minute per question. Five out of each 50 questions may well be ambiguous to some, leading to defensibly "wrong" answers, but it can hardly be said that nine-tenths of our essay questions are clear to all. As to grading, the Educational Testing Service has found that it takes five readers of an essay to average out the personal biases. True-false sheets, incidentally, even without machines or low-priced graders, can be laid alongside each other and corrected in one-third to one-sixth of the time required by essays, depending on whether the essay reader is making the sustained mental effort necessary for fairness.

Objective questions offer the only practical means of comparing different classes, colleges, and methods. If a class has averaged 68 on final examination essays, is there any other way to determine whether this meant poor students or difficult questions except to have some objective grades? If it also scored 68 on questions for which the norm was 63, its final average should be nearer a B then a D. Unwarranted disparities can be avoided by locating the class as a whole through objective scores while marking each individual mainly on essays.

Many teachers seem to lack interest in comparing their own success with that of others. For example, less than 15 percent of those writing for copies of these tests ever returned complete scores for comparison. Hopefully, N.Y.U.'s new ninety-minute macroeconomics test may achieve a good sample. It is using matching and multiple-choice questions, considered by many experts superior to true-false ones. Another multiple-choice experiment is the Stalmaker "Test of Economic Understanding," for which grades of 80 and 82 were given in the June *A.E.R.*; a score of 68.2 for 148 Drew and Rutgers beginning students suggests a gain of about 40 percent of the potential.

Enlarging their samples will increase the value of all tests for those interested in grading students fairly and measuring their own teaching progress. We all know that costs of experiment must be incurred to improve any industrial product. In economic education, the costs are minimal and the product important.

DISCUSSION

IRWIN L. HERRNSTADT: The motive for Professor Whitney's and Professor Joseph's papers is dissatisfaction with the effectiveness of our undergraduate teaching of economics. (I cannot comment upon Professor Attiyeh's and Professor Lumsden's motive, because their paper did not arrive.) Professor Whitney appears to document what many of us have long suspected; namely, that beginning students seem to learn very little economics. (However, I must admit serious reservations about the test questions I have seen and a number of his findings.) He does not explain why so little is learned nor advance any novel solutions. However, he does propose wider use of his special battery of questions to determine which teaching methods seem best.

In contrast, Professor Joseph offers both an explanation and a solution. Professor Joseph's diagnosis is disturbing. Even if students do catch on, he says, they soon forget or reject what they have learned because economic abstractions are too "unreal" to be "believed" or else challenge personal prejudices. The trouble is not entirely cognitive. I think most teachers of beginning economics are aware of the resistance accorded unpopular ideas. I for one believe that role playing is a useful device for meeting both problems posed by Professor Joseph, even though it is time consuming and sacrifices coverage and probably is not applicable to much material (e.g., macroeconomics). Moreover, it probably requires more classroom preparation than some of us are accustomed to give.

It is the unreal nature of our abstractions that warrants further consideration, because there is an implication that we have the cart before the horse. We seem anxious to adopt new instructional techniques without first asking what we want to teach to whom. Ours is a differentiated market. On the one hand, we have to train professional economists; on the other hand, we have to convince the nonspecialist that economics has something useful to say to him. Yet at the beginning level we treat both groups alike and have almost abandoned the nonspecialist, who is our audience. We neither have persuaded him that our concepts are relevant to his personal or public life, nor have we provided him with the tools to discriminate between those concepts that are relevant and those that are not.

I should like to suggest some reasons why. First, our sights are high. In an introductory course of no more than one year's duration, we spin many intricate webs that offer very modest explanations but take a great deal of the student's time to absorb even superficially. Then we expect the student to apply these half-learned abstractions to a very complicated world. Similar demands are not made of those who take beginning courses in the humanities, the sciences, or even the fine arts. Where worldly applications are asked of students, as in sociology or political science, for example, the subject matter is descriptive, not analytical, and probably easier to relate to personal experience.

Second, our basic economics offers the student one-dimensional models that

no economist would accept unqualifiedly. Students bright enough to grasp even some economic principles probably are bright enough to doubt the relevancy of such limited explanations of a complex world.

In short, we probably try to cover too much in too involved a way and succeed only in leaving students either confused or skeptical. We have failed to tie together for the beginner the economy as it really functions and the artificially simplistic theories we have for explaining it at the introductory level. In part, we have failed to acquaint students sufficiently with economic institutions and processes so that our abstractions seep through. In part, we have relied excessively on exactness at too early a stage, when the student's limited knowledge and experience make it inappropriate. Professor Joseph's paper seems to bear this out. Students first had to realize that economic theorizing had some applicability and were abstractions of actual processes.

Let me give some illustrations. We go through an elaborately detailed demonstration to make the simple point that excess reserves can lead to easy money condition which can stimulate the economy. But we lack time to explain the seeming contradiction that during an upswing in business activity, net free reserves drop, money markets tighten, and interest rates rise. Any student who reads the *New York Times* (and we hope he does) can discover that peculiarity for himself. Again, we stress in a rigorous, mechanical way the fundamental relationship between a change in investment spending and a change in income but fail to make distinctions crucial for distinguishing between the minor and major cycle. Finally, we compare the results of union wage policies with the results of a competitive labor market that probably never existed, but rarely so inform the student.

Unless we reconcile our elementary theorizing with such realities, can we expect the student to see the relevancy of much of basic economics, let alone summon the motivation to explore its intricacies? A knowledge of some theory is indispensable, but it must be convincingly related to a world the student recognizes, it must carefully spell out its assumptions, and for beginners it must not be so painstakingly exact that the point is missed.

It is not the method as much as what it tries to teach that will be the measure of our success. We can teach very simplistic—and hence unbelievable—theories too precisely, or we can try to make a complicated world more comprehensible and economics more credible. Programmed instruction can be used for either task, but it cannot make the irrelevant relevant nor the intricate simple.

If programming cannot solve problems of content, what can it do? It has a number of pedagogical advantages quite aside from the virtues claimed by operant learning theory and quite aside from a flexibility that allows the individual to progress at his own pace.

First, a successful program requires the teacher-programmer to specify his objectives, make explicit his assumptions, sharpen his definitions, and present his material in a systematic, consistent fashion. Nothing can be left dangling, because each successive frame is knit to the preceding one. Second, a program forces the student to pay close attention; that is, to study the material. He becomes involved in the learning process; he begins to integrate the material

as he answers questions and solves problems. He helps construct the concept, so to speak, as he works through it step by step. He begins to educate himself along a carefully delineated path.

The program thus can be used potentially to impart an understanding of properly qualified theoretical concepts, which then can be applied in the classroom and their appropriateness discovered. Instead of devoting class time to helping beginners digest poorly understood text material, the instructor can devote it to helping them develop and use analytical skills—something he supposedly has been doing all along. The beginner will not become a professional economist, but at least he will apppreciate what the professional is trying to do and why intricacy is not evidence of evasiveness or confusion but of careful analysis of a complex world.

DANIEL R. FUSFELD: I did not receive the paper by Attiyeh and Lumsden in advance; so I will limit my comment to some observations on my own experiences with preparation of programmed material.

The most valuable part of the process is preparing the material itself. The first step is to identify in detail the specific objectives which the instructor wants his student to achieve. This step immediately orients the instructor toward students rather than subject matter, and gets him thinking in terms of desired student behavior rather than organization of topics. The second step is to select and organize material which will enable students to achieve the objectives: texts, illustrations, problems, examples, and other material. Everything extraneous must be cut away in order not to distract the student from his goals.

At this point the programmer can develop a program to move the student ahead step by step, with continuous feedback of correct answers to provide motivation leading the student from carrot to carrot until he has learned the material.

But well-motivated college students probably do not need this Pavlovian incentive. If they are clearly told at the beginning of a course exactly what they are expected to know or to perform at the end and if the materials used are clearly tied to these objectives, there is little need for a teacher. Students will do the rest, although teachers may be needed to help them over difficulties and provide motivation.

One of the major faults of all the programmed material in economics I have seen is its failure to tell students at the start where they are going to end up. They need the artificial motivation of continuous feedback of correct answers. But if structured properly, the materials and their organization can provide motivation and most of the learning job can be left to students.

The better results from programmed instruction, as compared with other teaching methods, probably results more from superior organization of material and systematic presentation than from any other single cause. The real test of programmed instruction will come from comparing its results with the teaching done by the man who prepared it.

Turning now to Dr. Whitney's paper, I find that it leaves me very uneasy, and I am not sure I know why.

Perhaps it is the unspoken twin assumptions, first, that there is a large body of correct economic principles which ought to be emphasized by all teachers of introductory economics, and, second, all students ought to learn it.

He may be right about the existence of a correct body of generally accepted principles, but I am enough of a skeptic to believe that 75 percent of what we now think is true will probably turn out to be false. Today's truths need to be taken *cum grano salis* and our students trained to do so, much in the spirit of Lord Acton's comment that the path to true knowledge begins with a large and liberal discontent. For this reason I abhor true-false and most other objective examinations. I would rather have my students ask the right questions than know the right answers.

Furthermore, the knowledge which can be stated in categorical and objective terms is the least important part of our subject: knowing what it means is a higher accomplishment, and that can be shown only on essay examinations.

Thomas Henrich, the philosopher who used to play right field for the New York Yankees, once said, "Catching a fly ball is easy. Knowing what to do with it afterwards makes you a major leaguer." I want my students to be major leaguers, and I could not care less if they do poorly on true-false examinations.

I suspect that these reactions are shared by most teachers of elementary economics. We could probably train our students to score well on Whitney's examination, but there are more important things to do. (Incidentally, several of the answers deemed correct by Whitney are wrong, in my opinion.)

These comments lead directly into Dr. Joseph's paper on role playing as an educational technique. It is a path to intuitive rather than rational understandings, and I know that my colleagues who demand rigorous and accurate proofs will look down their noses at it. I use some other devices to achieve similar results in parts of my introductory course: songs from the union movement and a selection of arbitration cases to enable middle-class students from the suburbs to understand the motives and attitudes of organized workers, for example. Students may not be able to explain in a reasoned way exactly what those attitudes are or why workers have them, but they will certainly understand and be able to draw their own conclusions about them. The educational purpose of these devices is to achieve greater student participation in the learning process—a result devoutly to be wished.

The understandings achieved by role playing, however, are essentially at the emotional level. The technique itself was first developed to enable individuals to gain insight into their own emotional problems and relations with other people. The objective was insights which would result in changed behavior. Joseph's objectives are a little different. He wants his students to achieve a better knowledge of economic principles rather than to change their behavior. This represents quite a shift in the purposes of the technique, and we need to know more than Joseph has told us about how well role playing is adapted to the learning of a body of knowledge. The rather large emotional "kick" his students obtained may not indicate any substantial learning at all.

It is likely to be the result of insights into themselves rather than into the subject matter. That is what one would expect from role-playing techniques.

Even if that is the case, Joseph's students may have had an immensely valuable educational experience. They might not have learned much economics, but they may well be better individuals as a result of the role playing.

The teacher never knows what his students will learn from the wide variety of offerings he lays before them. The least significant things may have the greatest impact, without his ever being aware of it. Each student brings to the classroom his own unique combination of knowledge and interests, into which he must fit the things we teach in a way which is meaningful to him. The teacher's job is not to insist that he learn a specific body of subject matter in a particular way, but to assist him in coming to his own conclusions and reaching his own understandings. They may be different from ours—or Samuelson's—but that is all to the good.

RENDIGS FELS: My theme will be multiplicity—the multiplicity of objectives of economic education, the consequent need for a multiplicity of tests to determine how far we are achieving each of the different objectives, the multiplicity of teaching methods and aids and literature on economic education, the growing difficulty for anybody except a specialist to keep up with it all, and the coming need for an advisory service to help those of the 2,000 institutions of higher learning that do not have a specialist on economic education.

The papers presented here clearly justify having a session on economic education at the annual meetings of our Association. Fifteen years ago, economists called upon to discuss economic education felt no obligation to do original research or to say anything new. As a result, what was published on economic education was seldom worth reading. Happily the situation has changed. Serious work is going on. The papers presented here compare favorably with the general run of papers presented in recent years at the annual meetings, even though, like most papers in the *Papers and Proceedings* issues of the *American Economic Review,* two of the three are not up to the quality of the regular issues. But welcome as these contributions are, they add to a literature growing so rapidly that it is hard to keep up with.

The new data Professor Whitney has gathered since he last reported on "Measuring the Success of the Elementary Course" is welcome, but I wish he had (1) improved his true-false tests instead of keeping the same questions for the sake of comparability, (2) provided a critique of his tests in comparison with the new Stalnaker tests, and above all (3) had specified exactly what objective or objectives he is trying to measure our success in attaining. The multiple-choice questions of the Stalnaker Committee seem to have two important advantages: they are technically superior, and they are based on a detailed statement of objectives; namely, the economic concepts specified in the report of the National Task Force on Economic Education. Too few of Whitney's questions deal with the most fundamental ideas; too many deal with matters that, given the time limitations of the elementary course, we should give low priority to or omit altogether. Take, for example, the (false)

statement on one of his tests, "Postwar demobilization resulted in a sharp rise in unemployment (to 7% of the labor force in 1946)." Such questions should be omitted in favor of more questions on resource allocation, the price mechanism, national income concepts and theory, and money. The Stalnaker questions, with only a few exceptions, stick to fundamentals. Nevertheless, an excellent set of tests could be constructed by culling from the 1,100 questions on Whitney's twenty-two tests the hundred or so best questions. For the specific purpose of measuring the success of the elementary college course, tests so derived would be superior to the Stalnaker tests, which were devised with high school students and teachers in mind.

The Whitney and Stalnaker tests are general in the sense that they cover all elementary economics. That we also need more specific tests for more specific purposes is brought out by Myron Joseph's interesting hypothesis. Joseph's concept of what he is trying to accomplish in his teaching plainly differs from the objectives the Stalnaker Committee had in mind and from the not-very-clearly defined objectives of Whitney. Joseph says of one of his role-playing experiments: "I am convinced that students who participated in these markets will remember the experience and the major ideas that were developed from them long after the analysis that was taught more traditionally has ceased to have any real meaning." Joseph's hypothesis ought to be tested, but for that purpose the Whitney and Stalnaker examinations will not be suitable. We need a series of questions on, let us say, supply and demand, to be answered by students several years after they have had the elementary course. The former students would preferably be in four groups: one taught by Joseph's role-playing technique, another by programmed instruction, a third by conventional methods, with the fourth a control group without formal training in economics.

Similarly, special-purpose examinations are needed to test the hypothesis advanced by George Stigler two years ago. Stigler believes that students who have had no economics would do just as well at evaluating arguments for and against current policy proposals as would students who had had a course in economics some years earlier. Unlike the questions of Whitney and the Stalnaker Committee, Stigler's are not the sort that could be answered out of a textbook. Stigler's teaching objective differs from Joseph's, from Whitney's, and from the Stalnaker Committee's.

We need, in fact, a large variety of tests for a large variety of different purposes. Every college in the U.S.A. is different. Different teachers, though they may share the same broad objectives, differ widely on priorities. Teachers need to be able to select from a battery of examinations those which best test how far they are attaining their specific goals. Research workers investigating the comparative effectiveness of different teaching methods need a variety of examinations constructed for their particular hypotheses.

The development of tested tests to test testable hypotheses will be a great help in evaluating experimental teaching methods such as role playing, programmed instruction, and educational television, but the accumulation of research findings will exacerbate the problem already faced by overworked teachers of elementary economics—the problem of not only selecting a text-

book from the fifty on the market but also deciding what supplementary material to use, what teaching methods to adopt as best for the local situation, what objectives to emphasize in the light of what is realistically possible. Even now decisions are often made on the most superficial basis. The problem will get worse as more is learned about economic education. The gap will increase between what could be done and what is actually done for lack of a local specialist on economic education. The time is approaching for setting up a service of expert assistance available to colleges without their own specialists in this new and growing field. Publishers of elementary textbooks should provide this kind of service, but they are not likely to. If they do not, our Association should seek foundation support to fill this need.

AMERICAN ECONOMIC ASSOCIATION

PROCEEDINGS

OF THE

SEVENTY-SEVENTH

ANNUAL

MEETING

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CHICAGO, ILLINOIS
DECEMBER 28-30, 1964

PROCEEDINGS OF THE AMERICAN ECONOMIC ASSOCIATION

ANNUAL BUSINESS MEETING, DECEMBER 30, 1964
CONRAD HILTON HOTEL, CHICAGO, ILLINOIS

The Seventy-seventh Annual Business Meeting of the American Economic Association was called to order in Bel Air Room of the Conrad Hilton Hotel, Chicago, Illinois, at 2:00 p.m. by President G. J. Stigler. The minutes of the business meeting of December 29, 1963, were approved and the minutes of the Executive Committee meetings and reports of officers and committees of the Association were ratified. These reports are published in these "Proceedings" and constitute the official actions of the Association when approved at the annual meeting. The report of the Secretary was presented by H. F. Williamson. The schedule of times and places for future annual meetings through 1971 was outlined. Attention was called to the fact that the total number of members and subscribers as of November 30, 1964, was 17,816, a net increase of 1,537 for the year.

In addition, the Secretary reported on the following activities of the Association:

1. *Clearinghouse Project.* This project is now in full operation, with over 2,700 individuals now included in the roster of economists interested in overseas assignments.

2. *National Register of Scientific and Technical Personnel.* Nearly 12,000 questionnaires were returned in connection with this project and are now being processed by the National Science Foundation. The data will be made available to a special committee of the A.E.A. which will prepare a report covering the economics profession.

3. *Foreign Graduate Student Screening Project.* Under a grant from the Ford Foundation, the Association is undertaking to establish procedures for improving the screening of foreign graduate students applying for admission to economics and agricultural economics departments in the United States.

4. *Group Flights.* Four group flights were arranged for attendance at the Chicago meetings and it is planned to arrange similar flights for future meetings.

5. *Association Archives.* The archives of the Association have been classified and are available to interested scholars.

The report of the Treasurer was summarized by H. F. Williamson. Special attention was called to the fact that the net operating income of the Association for the year ending November 30, 1964, was $60,102 and that the unappropriated surplus was $206,397. The full details are shown in the Treasurer's report printed below.

J. W. Bell reported for the Finance Committee on the changes in the Association's investment portfolio. The full report of the Finance Committee is published below.

The report of the Managing Editor, published below, was summarized by J. G. Gurley. He expressed his appreciation for the effective work done by the

Editorial Board and consultants in preparing the *American Economic Review.*

The Secretary then presented the report of the Committee on Elections and the certification of the election of new officers for the year 1965, as follows:

In accordance with the bylaws on election procedure, I hereby certify the results of the recent balloting and present the reports of the Nominating Committee and the Committee on Elections.

The Nominating Committee, consisting of Gottfried Haberler, Chairman, Avery B. Cohan, Richard M. Cyert, G. A. Elliott, Marc Nerlove, and Nathaniel Wollman, presented to the Secretary the list of nominees for the respective offices:

For President-elect
Fritz Machlup

For Vice-Presidents
Nicholas Georgescu-Roegen
Alexander Gerschenkron
Charles J. Hitch
William Arthur Lewis

For Executive Committee
Moses Abramovitz
Neil H. Jacoby
Henry A. Latané
Robert H. Strotz

The Committee on Elections, consisting of Frederick A. Ekeblad, Chairman, Millard F. Long, and Harold F. Williamson, prepared biographical sketches of the candidates and ballots were distributed early in November. The canvass of ballots was made on December 19, 1964, and the results were filed with the Secretary.

From the report of the Committee on Elections, I have the following information:

Number of envelopes without names for identification	164
Number received too late	6
Number of defective ballots	—
Number of legal ballots	4,581
Number of returns from the mail ballot	4,751

On the basis of the canvass of the votes cast, I certify that the following persons have been duly elected to the respective offices:

President-elect (for a term of one year)
Fritz Machlup

Vice-Presidents (for a term of one year)
Charles J. Hitch
William Arthur Lewis

Members of the Executive Committee (for a term of three years)
Moses Abramovitz
Neil H. Jacoby

Following the report of the Committee on Elections, President Joseph J. Spengler took over the chair and introduced Fritz Machlup, the new President-elect.

The President recognized James A. Morris, Chairman of the Committee on Resolutions, who presented the following resolution:

Be it resolved that the Association expresses its deep appreciation and thanks to those who have participated in the planning, organizing, and conducting of the Seventy-seventh Annual Meeting. The Association is particularly indebted to its President, G. J. Stigler, for his fine conduct of his office and for the preparation of a Presidential Address of deep scholarship and great contemporary relevance; to its President-elect, J. J. Spengler for arranging an outstanding program centering on problems of basic importance to the membership and to our society; to the Executive Committee for assisting in the task of planning and staffing the program; to the Committee on Local Arrangements for the fine performance of its duties; and to the many participants in the program who have prepared and presented new ideas in economics for our enlightenment.

JAMES A. MORRIS, *Chairman*
CALVIN B. HOOVER
GERALD WARREN

The meeting was adjourned at 2:45 p.m.

HAROLD F. WILLIAMSON, *Secretary*

MINUTES OF THE EXECUTIVE COMMITTEE MEETINGS

1. Minutes of the spring meeting held in New York City, March 6-7, 1964:

The *second meeting* of the *1964 Executive Committee* was called to order at 9:50 a.m. at the New York Hilton Hotel, New York, New York, March 6, 1964. The following were present: G. J. Stigler, presiding, W. J. Baumol, E. D. Domar, J. G. Gurley, Gottfried Haberler, B. F. Haley, H. G. Johnson, E. S. Mason, J. J. Spengler, James Tobin, W. A. Wallis, and H. F. Williamson. Absent were: R. M. Solow and Robert Triffin. Present as members of the Nominating Committee were: A. B. Cohan, R. M. Cyert, G. A. Elliott, Marc Nerlove, and Nathaniel Wollman. Present as guests were: B. W. Lewis and Theodore Morgan.

1. *President's Remarks* (G. J. Stigler). President Stigler outlined the items on the agenda to be covered at the meetings.

2. *Minutes.* The minutes of the Executive Committee meetings of December 26 and 29, 1963, previously sent out to members were approved.

3. *Report of the Secretary* (H. F. Williamson).

Annual Meetings. The Secretary reported that plans were well under way for the 1964 annual meeting at Chicago and that E. T. Baughman, Vice-President of the Federal Reserve Bank of Chicago, had agreed to serve as Chairman of the Committee on Local Arrangements. The hotel headquarters will be the Conrad Hilton.

Attention was called to the fact that preliminary arrangements had been worked out for holding the 1970 annual meeting in Detroit. The Secretary was instructed to explore the possibility of holding the 1971 annual meeting either in Canada or New Orleans.

Following a discussion of the alternatives, it was decided to start the two December, 1964, Executive Committee meetings at 3:00 p.m. on December 27, with the expectation that the work of the Executive Committee would be completed before adjournment that evening.

Papers and Proceedings. It was announced that the plan was to print 18,500 copies of the 1964 *Papers and Proceedings* and that the total number of manuscript pages would be approximately 1,200.

As a means of reducing the size of the *Papers and Proceedings,* the Secretary was instructed to ask committee chairmen and Association representatives to keep their reports to a minimum.

Handbook. The Secretary reported that distribution of the *Handbook* to members had been completed and that efforts were being made to promote sales to subscribers and others. The question was raised concerning plans for the publication of the next edition. It was agreed that consideration should be given to the possibility of publishing a supplement within the next two years and that $8,000 per year out of current income should be set aside in a reserve fund for the publication of the next edition of the *Handbook.*

Clearinghouse Project. The Secretary reported that approximately 2,500 completed questionnaires had been returned by the first week in March and that these were being coded in order that requests could be filled more efficiently. A number of requests had already been received for suggestions of candidates for overseas positions, while a number of government agencies, foreign universities, and foundations had indicated their intention to use the Clearinghouse.

In order to publicize the Clearinghouse, copies of our policy statement and procedures were mailed to over a hundred universities and research institutes in developing countries and some twenty-five foreign economic journals. Further attempts will be made to publicize the availability of the service offered by the Clearinghouse.

N.S.F. Register. The Secretary reported that questionnaires would be mailed to members of the Association and others in the economics profession during the fourth week in March, at the same time that questionnaires were being mailed to members of other disciplines. The President announced that he would appoint a committee to draw up a report on the profession, based on the data available from the questionnaire.

Staff Salaries. Upon the recommendation of the Secretary, it was VOTED to raise the annual salary of Miss Gertrude Tait by $600, effective as of March 1, 1964.

4. *Report of the Treasurer* (H. F. Williamson). The Treasurer referred to the Treasurer's Report and the Report of the Auditor, which had not been available for the December

meeting of the Executive Committee. It was VOTED to accept the final report of the Treasurer and of the Auditor.

It was recommended that the treasurer prepare an annual budget covering the operations of the Association for submission at the spring meeting of the Executive Committee.

5. *Report of the Managing Editor* (J. G. Gurley). The suggestions of the Managing Editor for replacements on the Editorial Board were discussed and approved. The Editor reported that there was no pressing need at the present time for expanding the coverage of the *A.E.R.* but did request authorization to vary the size of particular issues. It was VOTED to grant this request.

The Editor reported on negotiations with the Royal Economic Society for a two-volume joint publication in Great Britain of the *A.E.R.* and Royal Economic Society survey articles. He noted particularly the suggestion that the American Economic Association join the Royal Economic Society in subsidizing a part of the publication costs in order to keep the price of the final publications low enough to make them available to students. Following a discussion, it was VOTED to authorize the Managing Editor to enter into an agreement with the Royal Economic Society to publish the survey articles with the understanding (1) that the amount of the subsidy provided by the American Economic Association should not be more than $1,120 and (2) that the United States price of the publications would be consistent with the publication price in Great Britain.

6. *Reports of Standing and Special Committees.*

Committee on Research and Publications (I. B. Kravis). No report.

Committee on Surveys of Foreign Economic Research (G. H. Hildebrand). No report.

Committee on Economic Education (B. W. Lewis). The Chairman, Pro Tem, reported that the members of his Committee (one member dissenting on proposals 1 and 2) recommended to the Executive Committee the adoption of the following proposals: (1) The acceptance and an early publication in the *A.E.R.* of the policy statement on the role of the Association in economic education as determined in a meeting of the Executive Committee on December 26, 1963. (2) The appointment by the new chairman of the reconstituted Committee on Economic Education of a part-time assistant to aid him in office work, correspondence, and in liaison with the Joint Council on Economic Education and other groups interested in economic education and that $2,000 be made available by the A.E.A. over the next twelve months for compensation for the assistant and for office expense. (3) The inclusion, for a trial period of five years, of one session on economic education in the program for the annual meeting, to be arranged by the Committee on Economic Education. Following a discussion, it was VOTED to accept the proposals made by the Committee on Economic Education, with the understanding that arrangements for the public announcement of the policy statement on the role of the Association in economic education be worked out between G. L. Bach and Moe Frankel, of the Joint Council on Economic Education. It was agreed that the scheduling of a session on economic education at the annual meeting should be in addition to the fifteen regular sessions ordinarily scheduled.

Committee on Honors and Awards (B. F. Haley). The Chairman reviewed his report submitted at the March, 1963, Executive Committee meeting, outlining the problem of according greater recognition to distinguished members of the profession. Following the discussion, the Chairman was instructed to explore further the possibility of establishing a category of honorary fellows.

Committee on the International Economic Association (H. S. Ellis). No report.

Index of Economic Journals (J. P. Miller). No report.

Committee on the Screening of Foreign Graduate Students in Economics (Theodore Morgan). The Chairman presented the Committee's revision of the November 30, 1963, draft of a proposal to improve the method of selection of foreign graduate students in economics and agricultural economics. The proposal recommended by the Committee included: (1) the formation of a Consortium open to all economics departments in the United States universities offering graduate degrees in economics and agricultural economics; (2) the establishment of an information office on foreign schools and graduate student applicants, to be operated under the general direction of the Secretary of the American Economic Association. The office, in cooperation with the members of the Consortium, would work out procedures for improving the selection of foreign applicants to graduate schools in various ways, including the gathering of information on the quality of training offered by foreign universities, colleges, and other educational institutions, the preparation of an informational booklet on graduate training in the United States, to be distributed abroad, and the arranging on request of a member of the Consortium for interviews with foreign applicants. Following the discussion, it was VOTED to authorize

the President to apply to the Ford Foundation for a grant to implement these proposals.

Nominating Committee (Gottfried Haberler). The Executive and Nominating Committees met as an Electoral College to consider nominees for the office of President-elect for 1965. After discussion, the nominee was selected and his acceptance obtained. Nominations for the other offices were discussed.

7. *Reports of Representatives and Members of Advisory Committees.*

A.C.L.S. (O. H. Taylor). The President read a report from O. H. Taylor on the annual meeting of the A.C.L.S. Because he was retiring from Harvard University at the end of this academic year, Professor Taylor asked that his resignation as the representative to the Council be accepted. It was agreed that his resignation should be accepted and that the President should appoint a successor.

S.S.R.C. (G. H. Hildebrand). No report.

N.B.E.R. (W. L. Thorp). No report.

A.A.A.S. (B. F. Hoselitz). No report.

I.E.A. (B. F. Haley). Professor Haley reported that the I. E. A. had raised the annual dues of the member associations by 50 percent. He noted that the A.E.A., the Royal Economic Society, and the Association Francaise de Science Economique had in the past paid double their assigned dues of $200 annually. Following a discussion, it was VOTED that the A.E.A. should continue to pay $400 annual dues to the I.E.A., unless the British and French associations decided to double their current dues, in which case the A.E.A. would pay 50 percent more, which would be $600 annually. Professor Haley also outlined the financial problems facing the I.E.A., noting that it appeared quite likely that the organization's future activities might have to be curtailed because of lack of funds.

I.I.E. (B. H. Higgins). No report.

Census Advisory Committee (Solomon Fabricant). No report.

UNESCO (K. E. Boulding). No report.

J.C.E.E. Special Publications Advisory Committee (G. L. Bach). No report.

N.S.F. Board (O. H. Brownlee). No report.

National Academy of Sciences-National Research Council (Robert Dorfman). No report.

8. *Unfinished and Miscellaneous Business.*

At the December, 1963, meeting of the Executive Committee, the Secretary was instructed to consider whether the policy of rotating members adopted for the Census Advisory Committee should be applied to all standing committees. Following a discussion, it was VOTED that the policy should be applied to all committees; namely, that all members of standing committees may be reappointed at the discretion of the President, but that no one should be reappointed after serving two consecutive terms until at least one year has elapsed.

The Secretary-Treasurer noted that his present term expired on December 31, 1964. It was VOTED to extend his term of office for an additional three-year period, ending on December 31, 1967.

In view of the extra work involved in the administration of the Clearinghouse, it was VOTED to increase the annual salary of the Secretary-Treasurer by $3,000, effective as of March 1, 1964. It was understood that the extra salary should be charged against the Clearinghouse Project and should terminate if this project is not renewed. Following a discussion of the advisability of establishing a committee to make an annual review of the salaries of the members of the administrative staff, it was VOTED to authorize the President to appoint such a committee, which would include the retiring president, the president, and the president-elect.

The Secretary called attention to the policy adopted by the Executive Committee in December, 1946, of granting complimentary memberships to all members of sixty-five years of age or older who have retired from their professional activities, provided they have been members of the Association for twenty-five years or more. Following a discussion, it was VOTED that the future policy of the Association should be to grant such members a complimentary membership but without the privilege of receiving the publications.

Professor Spengler outlined his plans for the 1964 program.

The meeting adjourned at 11:00 a.m., March 7.

2. Minutes of the meeting held in Chicago, Illinois, December 27, 1964:

The *third meeting* of the *1964 Executive Committee* was called to order at 3:05 p.m. at the Conrad Hilton Hotel, G. J. Stigler presiding. Others present were: W. J. Baumol, E. D. Domar, G. J. Gurley, Gottfried Haberler, H. G. Johnson, R. M. Solow, J. J.

Spengler, James Tobin, W. A. Wallis, and H. F. Williamson. Absent were: B. F. Haley, E. S. Mason, and Robert Triffin. Present as guests were: G. L. Bach, J. W. Bell, Solomon Fabricant, C. J. Hitch, I. B. Kravis, W. A. Lewis, M. F. Long, Fritz Machlup, Simon Rottenberg, and Arthur Smithies. The meeting was adjourned at 11:10 p.m.

1. *President's Remarks* (G. J. Stigler). The President reviewed the order of the items on the agenda.

2. *Minutes*. The minutes of the March 6-7, 1964, meeting were approved.

3. *Report of the Secretary* (H. F. Williamson). The Secretary presented the schedule of future meetings, membership growth and composition, publication costs of the 1964 *Papers and Proceedings*, requests to reprint, the use of the mailing list, and new activities of the Association—all more fully treated in the Secretary's Report as presented below. It was VOTED to approve and accept the Secretary's report.

It was VOTED to hold the annual meeting of the Association in New Orleans in 1971, and it was VOTED to hold the spring meeting of the Executive Committee in New York City, March 12-13, 1965.

4. *Report of the Treasurer* (H. F. Williamson). The Report of the Treasurer, published in full below, was summarized. Special attention was called to the fact that the net operating income for the fiscal year ending November 30, 1964, was $60,102. It was VOTED to accept the report of the Treasurer.

5. *Report of the Auditor* (H. F. Williamson). It was VOTED to accept the report of the auditor as printed below. The Secretary was instructed to express to David Himmelblau & Co. the appreciation of the Executive Committee of their services.

6. *Report of the Finance Committee* (J. W. Bell). J. W. Bell outlined the main features of the report of the Finance Committee, published below. It was VOTED to accept the report and to reappoint the members of this Committee to serve during 1965. The Secretary was instructed to express to the members of the Committee the appreciation of the Executive Committee for their services.

7. *Report of the Managing Editor* (J. G. Gurley). The Managing Editor reviewed the report published below. He called special attention to the fact that the number of manuscripts received during 1964 was considerably larger than the number received in 1963 and that there had also been a substantial increase in the quality of the articles submitted. He presented his proposed budget for 1965, which was approved. It was VOTED to accept the report and to authorize the Managing Editor to increase the number of pages in each volume of the *American Economic Review* from approximately 1,200 pages of text to approximately 1,300 pages of text.

8. *Reports of Standing and Special Committees.*

Committee on Research and Publications (I. B. Kravis). I. B. Kravis commented on his report, published below, reviewing the new volumes to be added to the "Reading Series" and the progress being made on the translations of the work of foreign economists. The Secretary noted that the publication of Volume VI of the *Index of Economic Journals* is scheduled for February, 1965. The Secretary was instructed to ask J. P. Miller if he would be willing to supervise the preparation of Volume VII of the *Index of Economic Journals*. It was VOTED to appoint G. J. Stigler and H. F. Williamson as an *ad hoc* committee to make arrangements for the publication of the translation series.

Journal of Economic Abstracts (Arthur Smithies). The Editor reviewed the results of the operation of the *Journal of Economic Abstracts* for 1964, calling attention to the fact that the operating loss for the year amounted to $10,215. He noted that as of December, 1964, the *Journal* had 6,000 paid-up subscribers. In order to break even on expenses at the subscription rate of $2.00, it would be necessary to double the present circulation of the *Journal*. Following a discussion, it was VOTED to transfer the *A.E.R.* listing of periodical articles to the *Journal of Economic Abstracts*, and it was also VOTED to appoint Arthur Smithies, J. G. Gurley, and H. F. Williamson as a committee to present recommendations regarding future operations of the *Journal* at the Executive Committee meeting in March.

Committee on Economic Education (G. L. Bach). The Chairman furnished a report, printed below. It was VOTED to accept the report.

Committee on Honors and Awards (B. F. Haley). Fritz Machlup commented on the written report of the Committee published below. After discussion, it was VOTED to accept the following amendments to the bylaws of the Association.

1. Bylaw 1, Membership, Paragraph 3 now reads: "Foreign economists of distinction, not exceeding twenty-five in number, may be elected honorary members of the Association." It is proposed to amend this paragraph by adding:

Past presidents of the Association and members who have been awarded the Walker

Medal shall be distinguished fellows. In addition, the Executive Committee may elect up to a total of twelve additional distinguished fellows, but not more than two in any one calendar year, from economists of high distinction in the United States or Canada.

2. Candidates for distinguished fellowships shall be nominated by the Nominating Committee or the Executive Committee, and they shall be elected by the combined vote of the two committees. The Nominating Committee shall solicit and give due consideration to the recommendations of the Committee on Honors and Awards.

3. The Nominating Committee should be free to make no nominations in any particular year. However, it should not be limited as to the number of candidates it may nominate in any year. It should accompany each nomination with a supporting brief.

4. Election to distinguished fellowship does not preclude election to any office of the Association.

Committee on the N.S.F. Report on the Profession (N. A. Tolles). The Chairman outlined the types of information on the economics profession that would be available from the N.S.F. survey of scientific personnel. It was suggested that the Chairman report at the March meeting of the Executive Committee on the Committee's plans for utilizing this material.

Committee on the International Economic Association. The Secretary read a letter from B. F. Haley, suggesting the establishment of a standing committee to advise our representatives to the I.E.A. Following a discussion, it was decided that such a committee was not needed at this time.

Nominating Committee. J. J. Spengler announced that G. J. Stigler would serve as Chairman of the Nominating Committee for 1965.

9. *Reports of Representatives and Members of Advisory Committees.*

A.C.L.S. (Ralph Andreano). No report.

S.S.R.C. (G. H. Hildebrand). No report.

N.B.E.R. (W. L. Thorp). Report printed below.

A.A.A.S. (Bert Hoselitz). No report.

I.E.A. (B. F. Haley). The report submitted by B. F. Haley is printed below. It was VOTED to increase the annual dues payment of the A.E.A. to the I.E.A. from $400 to $600, beginning in 1964.

I.I.E. Advisory and Policy Board Representatives (Benjamin Higgins). No report.

Census Advisory Committee (Solomon Fabricant). It was VOTED to accept the report of the Chairman as published below.

UNESCO (K. E. Boulding). The President announced the reappointment of K. E. Boulding as our representative to UNESCO.

J.C.E.E. Special Advisory Board (G. L. Bach). See report of the Committee on Economic Education, printed below.

N.S.F. Board (O. H. Brownlee). No report.

National Research Council—National Academy of Sciences (Robert Dorfman). Report printed below.

10. *Unfinished and Miscellaneous Business.*

The Secretary reported on the attempts to get foundation support for A. W. Coats to complete the history of the Association during the summer months of 1965. Following discussion, it was VOTED to authorize the Secretary to pay up to $3,000 for this research.

M. F. Long reported on the new Foreign Screening Project. The report is published below.

The President announced the reappointment of Abe Fortas as the Association's Counsel for a three-year term.

The Secretary reported on a request for joint subscription rates for foreign journals. It was decided not to consider any such arrangements.

J. J. Spengler announced that at the request of K. E. Boulding, he was appointing an advisory committee to our representative to UNESCO.

REPORT OF THE SECRETARY FOR THE YEAR 1964

Annual Meetings. The final report of the 1963 meeting in Boston indicated a total registration of 4,528,[1] of which 2,651[1] were members of the American Economic Association. The net income from the meeting was $19,263.09. Of this amount, the Association received $10,633.92, based on the number who registered as members.

The schedule for future annual meetings is: 1965, New York City, New York Hilton; 1966, San Francisco, San Francisco Hilton; 1967, Washington, D.C., Sheraton Park Hotel; 1968, Chicago, Pick-Congress Hotel; 1969, New York City, New York Hilton Hotel; 1970, Detroit, Sheraton-Cadillac Hotel; and 1971, New Orleans (hotel to be confirmed).

Dr. Fred H. Klopstock, Manager of the research Department of the Federal Reserve Bank of New York, will serve as Chairman of the Committee on Local Arrangements for the 1965 meeting in New York City.

Membership. Exhibit I below shows that the total number of members and subscribers was 17,816 as of November 30, 1964, a net increase of 1,537 for the year.

Advertising and Announcements. The number of advertising pages in the *American Economic Review* for 1964 was 162 paid and 54 exchange pages compared with 155½ and 56 pages in 1963. The "Vacancies and Applications" announcements totaled 33 pages for 1964 compared to 30 for 1963.

Papers and Proceedings. A comparison of the size and cost of the *Papers and Proceedings* for the period 1957-64 is made in Exhibit II below.

Permissions to Reprint and Translate. The number of permissions to quote from, reprint, or translate articles from the *Review* and the *Papers and Proceedings* totaled 201 for 1964 compared to 192 for 1963. Under the policy adopted by the Executive Committee at the March, 1962, meeting, permission to reprint or translate is granted only after the consent of the author has been obtained by the editor or publisher.

Use of Mailing List. Requests for the use of the mailing list continue to come largely from publishers of books and periodicals and from corporations wishing to send out reports or reprints of speeches. The sale of the mailing list continues to be an important source of income for the Association.

Group Flights. For the first time, group flights originating in five major cities and open to members of any of the Allied Social Science Associations were scheduled for the 1964 annual meetings in Chicago. Less than the required minimum number of twenty-five individuals signed up for the flight originating in Los Angeles, but the number participating in flights from the other four cities were: New York, 53; Boston, 35; Washington, D.C., 45; and San Francisco, 45. Since the cost of the group flights is approximately 20 percent less than regular tourist or economy round trip fares, it would appear that we should continue to offer this service to our members.

[1] These figures were incorrectly reported for the Pittsburgh meeting in the 1963 report. They should have been 3,853 and 2,421, respectively.

Association Archives. The project to organize the historical records of the American Economic Association has been completed. The material retained, which fills about twenty-five standard file drawers, is segregated by subject matter for each year for the period 1885-1959. In addition to various reports, and memoranda not published in the *Papers and Proceedings,* the material includes the correspondence of officers, members of the executive committees, and others active in the affairs of the Association.

We are now organizing the editorial records of the *American Economic Review,* covering the period 1912-45. At present, it appears that we will end up with about eight file drawers of editorial correspondence, which will be a valuable addition to the records of the A.E.A.

We expect the archives to be of interest to scholars working in the areas of intellectual and social history and are prepared to provide them with space and facilities for research.

Asia Foundation Grant. The Asia Foundation has given an additional sum of $2,500 to enable the Association to continue assistance to Asian students.

Clearinghouse Project. Since November, 1963, the A.E.A. Clearinghouse has received over 2,700 questionnaires from members of the economics profession indicating their availability and interest in serving overseas as consultants, researchers, or teachers. The range of professional qualifications of the respondents is most impressive, and during the past year, we have sent out approximately 1,000 copies of questionnaires of individuals qualified for the more than 90 specific positions for which we were asked to suggest candidates.

Thus far the great majority of requests have come from various agencies of the U.S. government and from individuals or institutions with contracts to staff overseas economics departments or research institutions. We anticipate an increase in the number of requests directly from overseas educational and research institutions as information about the service becomes more widespread.

Total expenditures on the Clearinghouse during the fifteen-month period September 1, 1963-November 30, 1964, amounted to $13,939, while income from operations was $690.10. Thus of the original grant of $35,000.00, we have a balance of $22,220.51 to cover operations through the remainder of the term of the grant, which ends August 31, 1966.

Our policy to date has been to provide our services to nonprofit organizations without charge. We expect that by the end of 1965 our services will be well enough established to charge all prospective employers a fee for each position for which they ask us to suggest candidates. We hope that income from such fees, plus sustaining funds from some agencies, will be sufficient to cover the future costs of operations. Judging from the enthusiastic reaction from the prospective employers with whom we have dealt, it seems clear that there is a definite need and desire for the type of services provided by the Clearinghouse.

Standing Committees

CENSUS ADVISORY COMMITTEE
Solomon Fabricant, *Chairman* (1967)
John P. Lewis (1964)
Frank E. Morris (1964)
Gottfried Haberler (1965)
Bert G. Hickman (1965)
Lorman C. Trueblood (1965)
Donald J. Daly (1966)
Lawrence R. Klein (1966)
Beryl W. Sprinkel (1966)
Geoffrey H. Moore (1967)
John Lintner (1967)
H. Gregg Lewis (1967)
Arthur Okun (1967)
Daniel Creamer (1967)

COMMITTEE ON ADMINISTRATIVE STAFF SALARIES
George J. Stigler, *Chairman*
Joseph J. Spengler
Fritz Machlup

COMMITTEE ON ECONOMIC EDUCATION
G. L. Bach, *Chairman* (1967)
R. A. Gordon (1965)
Ben W. Lewis (1965)
Marshall R. Colberg (1967)
Rendigs Fels (1967)
Emanuel T. Weiler (1967)

COMMITTEE ON HONORS AND AWARDS
Bernard F. Haley, *Chairman* (1966)
Gardner Ackley (1964)
Kenneth J. Arrow (1964)
Fritz Machlup (1964)
Earl J. Hamilton (1966)
Martin Bronfenbrenner (1966)

COMMITTEE ON RESEARCH AND PUBLICATIONS
Irving B. Kravis, *Chairman* (196
Karl A. Fox (1964)
Harry Johnson (1964)
Franco Modigliani (1965)
W. L. Thorp (1965)
G. H. Hildebrand (1966)
Bert F. Hoselitz (1966)

INSTITUTE OF INTERNATIONAL EDUCATION, ADVISORY AND POLIC BOARD REPRESENTATIVES
Benjamin Higgins, *Chairman* (1966)
Simon Rottenberg, *Acting Chairman* (1967)
Andrew Kamarck (1965)
John Sheahan (1965)
Anthony Tang (1966)
Raymond Mikesell (1967)
Henry Rosovsky (1967)

J.C.E.E., SPECIAL ADVISORY COMMITTEE
Ben W. Lewis (1965)
G. L. Bach (1966)
Rendig Fels (1966)

Committees Appointed During the Year

COMMITTEE ON ELECTIONS
Frederick A. Ekeblad, *Chairman*
Millard F. Long
Harold F. Williamson, Ex Officio

COMMITTEE ON THE I.E.A.
Gottfried Haberler

COMMITTEE ON THE N.S.F. REPORT ON THE PROFESSION
N. Arnold Tolles, *Chairman*
Alice Hanson Jones
Ewan Clague

COMMITTEE ON SCREENING OF FOREIGN STUDENTS
Theodore Morgan, *Chairman*

Benjamin Higgins
Charles Dean
Simon Rottenberg
Harold F. Williamson

FINANCE COMMITTEE

C. Wells Farnham, *Chairman*
Corliss D. Anderson
James Washington Bell
Harold F. Williamson

NOMINATING COMMITTEE

Gottfried Haberler, *Chairman*
Avery B. Cohan
Richard M. Cyert
G. A. Elliott
Marc Nerlove
Nathaniel Wollman

Council and Other Representatives

A.A.A.S.

Bert Hoselitz (1965)

A.C.L.S.

Ralph L. Andreano (1966)

I.E.A.

Bernard F. Haley (1965)
Robert M. Solow (1968)

N.B.E.R.

Willard L. Thorp (1965)

NATIONAL RESEARCH COUNCIL, NATIONAL ACADEMY OF SCIENCES

Robert Dorfman (1965)
Henry M. Oliver (1966)
Lionel McKenzie (1967)

NATIONAL SCIENCE FOUNDATION

O. H. Brownlee

S.S.R.C.

Karl A. Fox (1964)
Franco Modigliani (1965)
G. H. Hildebrand (1966)

U.S. NATIONAL COMMISSION FOR UNESCO

Kenneth E. Boulding

Representatives of the Association on Various Occasions

UNIVERSITY OF NEW MEXICO SEVENTY-FIFTH ANNIVERSARY CONVOCATION
Philip G. Hudson
UNIVERSITY OF DENVER CENTENNIAL CONVOCATION
Carl McGuire
LOYOLA UNIVERSITY, LOS ANGELES, GOLDEN ANNIVERSARY
Dudley Pegrum
BROWN UNIVERSITY BICENTENNIAL ACADEMIC CONVOCATION
Chelcie C. Bosland
CALDWELL COLLEGE FOR WOMEN SILVER JUBILEE
Donald Grunewald

INAUGURATIONS

Ralph Candler John, Simpson College
W. E. Alley
Lawrence Curtis Wanlass, College of the Virgin Islands
Luz M. Torruellas
H. LaMarr Rice, Lincoln Memorial University
Kenneth C. Spaulding
Perry Fridy Kendig, Roanoke College
William A. Sandridge

John Wieland Oswald, University of Kentucky
  J. Fred Holly
William Harwood Hinton, Houston Baptist College
  John P. Owen
William Walsh Hagerty, Drexel Institute of Technology
  Willis J. Winn
Howard Mitchell Phillips, Birmingham-Southern College
  R. M. Havens
Alan Simpson, Vassar College
  Henry H. Villard
Karl William Meyer, Wisconsin State University, Superior
  Richard O. Sielaff
Gordon E. Hermanson, Davis and Elkins College
  Thomas C. Campbell
Albert Bowker, New York Cities Universities
  Carl Shoup
Earnest S. Brandenburg, Drury College
  Robert S. Hardie
William L. Reilly, Le Moyne College
  M. A. Eggers
John Walter Bachman, Wartburg College
  J. Bruno Tulasiewicz
Joseph Anthony Sellinger, Loyola College, Baltimore
  Carl Christ
Kenneth Rast Williams, Florida Atlantic University
  James C. Vadakin
Robert Harry Spiro, Jacksonville University
  Ralph H. Blodgett
James Henry McCrocklin, Southwest Texas State College
  Carey C. Thompson
John Frederick Olson, Oklahoma City University
  Jim E. Reese
  David Ross Boyd
Gerard John Campbell, Georgetown University
  John J. Hooker
Howard R. Bowen, University of Iowa
  Karl A. Fox
University of Utah
  Willard Bingham Doxey

Respectfully submitted,
HAROLD F. WILLIAMSON, *Secretary*

EXHIBIT I

MEMBERS AND SUBSCRIBERS

	Totals 11/30/63	Gain or *Loss*	Totals 11/30/64
Class of membership:			
Annual	10,152	808	10,960
Junior	1,331	226	1,557
Family	147	9	156
Complimentary	97	*1*	96*
Life	228	11	239
Honorary	18	*1*	17
Total members	11,973	1,052	13,025
Subscribers	4,306	485	4,791
Totals	16,279	1,537	17,816

* Includes 23 who do not receive publications.

EXHIBIT II

PUBLICATION COSTS

Papers and Proceedings				Handbooks		
Year*	Number of pages	Number of copies	Cost	Number of pages	Number of copies	Cost
1957	754	12,400	$16,253	548	10,100	$15,815
1958	677	12,700	15,471	32	9,300	1,434
1959	689	14,000	16,780			
1960	745	14,800	18,914			
1961	675	15,800	19,759			
1962	615	16,000	18,277			
1963	753	17,700	23,440			
1964	698	18,500	23,362	472	16,000	48,626

* This is the year of publication and pertains to the meeting of the preceding year.

REPORT OF THE TREASURER FOR THE YEAR ENDING NOVEMBER 30, 1964

The comparative results of the operation of the Association for 1963 and 1964 are shown in Table 1. Except for receipts from republication, an increase in returns from all other sources during 1964 brought the total income for the year to almost $221,000, approximately $41,500 above total income for 1963. Largely because the major portion of the cost of the 1964 *Handbook* was charged off in 1963, total expenses for 1964 were just under $161,000, some $18,700 below total expenses for 1963. The result was a net operating income for 1964 of $60,102, compared to a net loss for 1963 of $108.

The comparative financial status of the Association for 1963 and 1964 is shown in Table 2. After taking into account deductions from the surplus to cover the cost of Volume VI of the *Index of Economic Journals* and appropriations set aside for the Committee on Economic Education and a future edition of the *Handbook*, the Association's unappropriated surplus for 1964 of $206,397 was approximately $44,400 larger than at the end of 1963.

Tables 3 and 4 give summaries of the investment portfolios and returns on investment for the years 1945-64. An alphabetical list of the Association's security holdings for 1964 is shown in Table 5.

The prospects for a favorable net operating income are encouraging for 1965, despite the possibility of somewhat higher printing costs and administrative expenses as the size of the Association continues to increase. The only major extraordinary expenditure under consideration for 1965 would involve the preparation of Volume VII of the *Index of Economic Journals*. At the same time, it seems reasonable to assume that the Association's normal sources of income will continue to expand at a moderate rate.

Respectfully submitted,
HAROLD F. WILLIAMSON, *Treasurer*

TABLE 1

COMPARATIVE RESULTS OF OPERATIONS, 1963 AND 1964

	11/30/63	11/30/64
Income		
Membership dues	$ 84,839	$ 94,477
Subscriptions	35,260	38,955
Sales	3,850	5,315
Advertising	23,653	43,473
Republications income	12,574	397
Handbooks	—	7,244
Sale of mailing list	5,165	7,283
Sundry income	945	1,034
Dues and publications income	$166,286	$198,178
Interest	$ 6,088	$ 6,594
Dividends	5,041	6,364
Less custodian fees	332*	357*
Profit on sales of securities (net)	2,349	10,216
Investment income (less fees)	$ 13,146	$ 22,817
Total income	$179,432	$220,995
Expenses		
Office salaries	$ 31,325	$ 37,333
Accounting service (IBM agency)	6,570	—
Other administrative expenses	13,274	16,106
Annual meeting	5,626*	8,631*
Executive Committee	2,645	1,870
Other committee expenses	876	1,133
Administrative and operating expenses	$ 49,064	$ 47,811
Review printing	$ 49,941	$ 53,735
Papers and Proceedings printing	23,440	23,362
Handbook printing	36,529	12,097
Editorial office (*Review*):		
Contributors	1,963	1,580
Editorial and clerical salaries	17,053	20,023
Other expenses (net)	1,551	2,285
Publications	$130,477	$113,082
Total expenses	$179,541	$160,893
Net operating income or loss	$ 109*	$ 60,102

* Denotes red.

TABLE 2

COMPARATIVE FINANCIAL CONDITION, 1963 AND 1964

	11/30/63	11/30/64
Assets		
Cash on deposit and on hand	$ 94,958	$117,505
Receivables (net)	13,919	19,992
Prepaid expenses and inventories	841	2,794
Furniture and fixtures (net)	6,417	3,709
Investments at cost:		
Bonds	90,227	149,198
Stocks	119,524	131,247
Total assets	$325,886	$424,445
Liabilities and Funds		
Accounts payable	$ 15,767	$ 14,274
Deferred income	18,004	19,748
Committee on Research and Publications	32	32
Committee on Economic Education	—	368
Reserve for *Handbook* supplement	—	8,000
Outside grants:		
Clearinghouse for economists (Ford)	33,285	22,290
National register (N.S.F.)	957*	1,982*
Register of economists (Ford)	74	—
Economic abstracts (Ford)	12,600	7,344
Foreign economic research translations (Ford)	20,694	15,750
Foreign economic surveys (Ford)	36,689	26,635
Survey articles (Rockefeller)	1,625	1,625
Travel grant (Carnegie)	1,578	982
Asia Foundation grant	728	1,654
Foreign student screening project (Ford)	—	74,591
Agricultural Development Council, Inc.	—	1,161
Sundry	172	176
Life memberships	23,600	25,400
Total liabilities and funds	$163,891	$218,048
Surplus		
Balance at beginning of period	$169,640	$161,995
Expenditures in excess of grant, *Index of Economic Journals*	5,536*	5,500*
Contribution for publication of Jaffe's Life of L. Walras	2,500*	—
Cancellation of appropriation for Committee on Professional Standards and Ethics	500	—
Appropriation for *Handbook* supplement	—	8,000*
Appropriation for Committee on Economic Education	—	2,500*
Life memberships written off less reinstatements	—	300
Net income or loss for period	109*	60,102
Unappropriated surplus	$161,995	$206,397
Total footings	$325,886	$424,445

* Denotes red.

TABLE 3

INVESTMENT PORTFOLIO

Year	At Par	Cost			Market
	Bonds	Bonds	Stocks	Total	Stocks and Bonds
1945	$ 40,000	$ 36,705	$ 44,955	$ 81,661	$103,574
1948	35,000	33,108	48,624	81,732	84,841
1950	35,000	33,108	51,978	85,087	104,177
1951	43,000	43,340	49,764	93,104	117,316
1952	42,000	42,312	58,934	101,246	130,836
1953	68,000	68,308	46,458	114,766	134,562
1954	61,000	61,518	38,082	99,600	132,280
1955	75,000	75,370	59,394	134,764	166,772
1956	75,000	75,370	60,237	135,607	168,337
1957	75,000	75,370	55,084	130,454	151,638
1958	75,000	75,370	67,741	143,111	175,609
1959	75,000	75,386	67,652	143,038	191,506
1959*	175,000	175,616	67,652	243,268	291,506
1960*	160,000	160,508	94,910	255,418	299,768
1961*	170,000	169,794	109,071	278,865	356,131
1962*	125,000	125,367	116,699	242,066	293,039
1963*	90,000	90,367	119,524	209,891	284,160
1964*	150,000	149,198	131,249	280,447	371,556

* Includes bonds held in temporary operating fund.

TABLE 4

RETURN ON INVESTMENTS

Year	Bonds	Stocks	Total	Rate of Return on Cost
1945	$1,479	$2,488	$3,968	4.71%
1948	1,194	2,944	4,139	5.06
1950	1,117	3,860	4,977	5.85
1951	1,026	4,607	5,633	6.05
1952	1,117	3,681	4,799	4.75
1953	1,435	3,587	5,022	4.36
1954	1,621	2,961	4,582	4.58
1955	1,750	3,002	4,752	3.53
1956	1,770	3,336	5,106	3.76
1957	1,770	3,397	5,167	3.90
1958	1,770	3,182	4,952	3.46
1959*	2,518	3,231	5,749	3.90
1959†	3,894	3,231	7,125	2.90
1960†	6,693	3,772	10,465	4.09
1961	5,460	4,143	9,603	3.44
1962	4,838	4,489	9,327	3.85
1963‡	3,320	5,041	8,361	3.98
1964‡	3,341	6,364	9,705	3.46

* Does not include income from bonds held in temporary operating fund.
† Includes income from bonds held in temporary operating fund.
‡ Does not include interest on savings account.

TABLE 5

List of Securities Held by the Association

Stocks

Number of Shares of Stock	Issue	Cost	Approximate Market Value 11/30/64
300	Abbott Laboratories	$ 6,133	$ 12,900
100	Bayer A. G.	6,822	7,300
300	Central and South West Corp.	2,101	15,000
200	Continental Illinois National Bank and Trust Co.	6,619	8,600
200	Deere and Co.	4,240	8,800
200	Disney	8,254	9,000
200	Gulf Oil Corp.	1,394	12,200
200	Houston Lighting and Power Co.	827	10,200
100	Inland Container Corp.	4,944	5,100
38	International Business Machines Corp.	9,300	15,656
100	International Nickel Co. of Canada	3,911	8,600
100	Marsh and McLennan	3,687	3,900
100	McIntyre Porcupine Mines	4,818	6,100
150	Montgomery Ward and Co.	5,013	6,000
100	Motorola	6,746	9,300
200	Olin Mathieson Chemical Corp.	8,731	8,200
275	Peoples Gas Light and Coke Co.	3,562	12,650
150	Rex Chain Belt Co.	6,621	8,100
200	Royal Dutch Petroleum	8,530	8,800
100	Siemens and Halske	5,519	6,800
200	Standard Oil Co. (Ind.)	3,650	8,600
100	Swedish Ball Bearing	9,023	8,800
175	Wells Fargo Bank-American Trust Company	3,522	8,400
150	Zenith Radio Corp.	1,977	9,900
50	Zerox	5,305	4,700
		$131,249	$223,606

Par Amount	Bonds	Cost	Approximate Market Value 11/30/64
$20,000	U.S. Treas. Bonds, 3⅞%, due 11/15/71	$ 19,951	$ 19,600
5,000	U.S. Treas. Notes, 4%, due 11/15/65	5,000	5,000
50,000	U.S. Treas. Bonds, 3¾%, due 5/15/66	50,000	50,000
15,000	U.S. Treas. Bonds, 2½%, due 12/15/72–67	14,456	13,350
60,000	U.S. Treas. Bills, 3/22/65	59,791	60,000
	Bonds	$149,198	$147,950
	Stocks	131,249	223,606
	Total	$280,447	$371,556

REPORT OF THE FINANCE COMMITTEE

December 22, 1964

Executive Committee
American Economic Association
Evanston, Illinois

GENTLEMEN:

The accompanying tables show the list of securities held by the Association at the end of the fiscal year, November 30, 1964, and the changes made during that year. Table 1 gives a classified list of the stocks and the bonds according to maturity and records the cost and approximate market values on November 30, 1964. The securities account of the Association does not include "temporary operating funds" which are deposited in a savings account and approximated $27,000 at the end of the last fiscal year.

The total market value of the securities account as of November 30, 1964, was $421,056. This compares with $334,160 at the end of the previous year. However, it should be noted that during the last fiscal year $60,000 was transferred from temporary operating funds to the investment account and that only about $27,000 of the increase in value was due to appreciation.

It has been the practice of the Association to set aside a portion of its resources consisting of grants made for specific purposes subject to early withdrawal to finance projects under way. These resources have been invested in U. S. Treasury securities or held in savings accounts. The transfer of $60,000 to the securities account during the past fiscal year will leave approximately $27,000 in the "temporary operating fund," which under present conditions seems adequate.

You will note from Table 2 that again few changes were made in the securities account during the year. The Finance Committee remains mildly optimistic as to the trend of general business and corporate profits and believes that the federal government will make great efforts to prevent or postpone any significant business recession. Therefore, the Committee believes that a fairly aggressive investment position is still desirable in spite of the high level of the stock market and long duration of the present period of business expansion.

Respectfully submitted,
C. WELLS FARNHAM, *Chairman*
CORLISS D. ANDERSON
JAMES WASHINGTON BELL
HAROLD F. WILLIAMSON

TABLE 1

INVENTORY AND APPRAISAL OF SECURITIES AND CASH AS OF NOVEMBER 30, 1964

	Par or Shares	Cost	Market Value
FIXED INCOME SECURITIES			
Cash Equivalent			
Savings Deposit 4%	$50,000	$ 50,000	$ 50,000
U. S. Treas. Bills 3/22/65	60,000	60,000	60,000
U.S. Treas. 4% 11/15/65	5,000	5,000	5,000
U.S. Treas. 3¾% 5/15/66	50,000	50,000	49,500
		$165,000	$164,500
Bonds			
U.S. Treas. 3⅞ 11/15/71	$20,000	20,000	19,600
U.S. Treas. 2½ 12/15/72–67	15,000	15,275	13,350
		$ 35,275	$ 32,950
TOTAL FIXED-INCOME SECURITIES		$200,275	$197,450
COMMON STOCKS			
Utilities			
Central & South West	300	2,100	15,000
Houston Lighting & Power	200	827	10,200
Peoples Gas	275	3,506	12,650
			$ 37,850
Financial			
Continental Illinois National Bank	200	6,619	8,600
Wells Fargo Bank	175	3,521	8,400
Marsh & McLennan	100	3,685	3,900
			$ 20,900
Merchandising			
Montgomery Ward	150	5,013	6,000
Paper and Textiles			
Inland Container	100	4,944	5,100
Machinery and Construction			
Deere & Company	200	4,240	8,800
Rex Chainbelt	150	6,620	8,100
			$ 16,900
Mining and Metals			
International Nickel	100	3,911	8,600
McIntyre Porcupine Mines	100	4,818	6,100
			$ 14,700
Oil and Gas			
Gulf Oil	200	1,393	12,200
Royal Dutch Petroleum	200	8,530	8,800
Standard Oil of Indiana	200	3,650	8,600
			$ 29,600
Chemicals and Drugs			
Abbott Laboratories	300	6,135	12,900
Olin Mathieson	200	8,731	8,200
			$ 21,100
Electrical Products			
Motorola	100	6,746	9,300
Zenith Radio	150	1,977	9,900
			$ 19,200

TABLE 1 (*continued*)

	Par or Shares	Cost	Market Value
Office Equipment			
International Business Machines	38	9,300	15,656
Xerox	50	5,305	4,700
			$ 20,356
Miscellaneous			
Walt Disney Productions	200	8,255	9,000
Foreign			
Bayer, A. G.	100	6,820	7,300
Siemens & Halske	100	5,520	6,800
Swedish Ball Bearing	100	9,023	8,800
			$ 22,900
TOTAL COMMON SOTCKS		$131,243	$223,606
TOTAL SECURITIES		$331,518	$421,056
TEMPORARY OPERATING FUND			
Savings Deposit	$27,066		

TABLE 2

SUMMARY OF SECURITIES PURCHASED AND SOLD
YEAR ENDED NOVEMBER 30, 1964

		Cost	Proceeds	Profit or Loss
Stocks				
	Shares			
Sales:				
American Potash & Chemical...	150	$ 6,284.25	$ 5,828.20	$ 456.05*
Houston Lighting & Power.....	100	413.37	4,614.21	4,200.84
Socony Mobil Oil Co...........	150	3,882.25	10,340.54	6,458.29
Wells Fargo Bank.............	5	500.00	512.95	12.95
		$11,079.87	$21,295.90	$10,216.03
Purchases:				
Walt Disney Productions.......	200	$ 8,253.88		
Royal Dutch Petroleum........	200	8,530.26		
Xerox Corp..................	50	5,304.76		
I.B.M. Corp..................	½	213.88		
Wells Fargo Bank.............	5	500.00		
		$22,802.78		
Bonds				
	Par Value			
Sales:				
U.S. Treas. Notes, 1½%, 10/1/64.	$ 8,000.00	$ 8,000.00	$ 8,000.00	
U.S. Treas. Notes, 3¾%, 5/15/64.	5,000.00	5,000.00	5,000.00	
		$13,000.00	$13,000.00	
Purchases:				
U.S. Treas. Notes, 4%,11/15/65.	$ 5,000.00	$ 5,000.00		
U.S. Treas. Bonds, 2½%, 12/15/72-67...............	8,000.00	7,180.00		
U.S. Treas. Bills, 12/3/64......	60,000.00	59,790.95		
		$71,970.95		

* Denotes red.

REPORT OF THE AUDITOR

December 18, 1964

Executive Committee
American Economic Association
Evanston, Illinois

DEARS SIRS:

In accordance with instructions, we have examined the accounts and related records of the American Economic Association for the year ended November 30, 1964, and now submit our report thereon together with the following exhibits:

Particulars	Exhibit
Statement of Financial Position—	
November 30, 1964	1
Year Ended November 30, 1964—	
Statement of Income and Expense	2
Statement of Special Funds	3

Results from Operations

Net income for the year ended November 30, 1964, was $44,402 compared with net loss of $7,645 for the year ended November 30, 1963, as shown in the following summary:

Particulars	Year Ended November 30, 1964	1963	Increase Decrease
Income:			
Dues..............................	$ 94,477	$ 84,839	$ 9,638
Interest and dividends (net)........	12,601	10,797	1,804
Profit on sale of securities (net)......	10,216	2,349	7,867
Miscellaneous income.............	1,034	945	89
Total income..................	$118,328	$ 98,930	$19,398
Expense:			
Publications..............	$113,082	$130,477	$17,395*
Less—Publication income.........	95,385	75,337	20,048
Net publication expense.......	$ 17,697	$ 55,140	$37,443*
Administrative and other operating expenses....................	40,529	43,899	3,370*
Appropriations, etc. (net)..........	15,700	7,536	8,164
Total expense.................	$ 73,926	$106,575	$32,649*
Net income or loss.............	$ 44,402	$ 7,645*	$52,047

* Denotes red.

Interest on bonds owned was accounted for in accordance with stated rates; dividends received on stocks were compared with amounts reported in published records, as dividends paid.

Net publication expense, as shown in the following summary, amounted to $17,697 for the current year compared with $55,140 for the preceding year:

Particulars	Year Ended November 30, 1964	1963
Expenses:		
Printing of—		
Review	$ 53,735	$ 49,941
Directory and Handbook	12,097	36,529
Proceedings	23,362	23,440
Editor's honorarium	8,000	8,000
Payments to contributors	1,580	1,963
Editorial clerical salaries	12,023	9,053
Editorial supplies and expense	2,285	1,551
Total expenses	$113,082	$130,477
Less—Income:		
Subscriptions, other than members	$ 38,955	$ 35,260
Sales of copies	5,315	3,850
Advertising	43,473	23,653
Republications and reprints	398	12,574
Handbooks	7,244	—
Total income	$ 95,385	$ 75,337
Net publication expense	$ 17,697	$ 55,140

Charges for the cost of printing the December, 1964, issue of the *Review* and reprints had not been made by the publishers at the time of our examination. The publishers estimate the cost of the *Review* printings and reprints at $12,850; this amount is included in the expenses for the year.

Financial Position

Financial position of the Association at November 30, 1964, and 1963 is set forth in the following summary:

Assets	November 30 1964	1963	Increase Decrease
Cash on deposit and on hand	$117,505	$ 94,957	$22,548
Receivables (net)	19,992	13,919	6,073
Prepaid expenses	2,794	841	1,953
Equipment, furtniture and fixtures (net)	3,709	6,417	2,708*
Investments at cost—			
Bonds	149,198	90,227	58,971
Stocks	131,247	119,525	11,722
	$424,445	$325,886	$98,559
Liabilities, Funds and Surplus			
Accounts payable	$ 14,274	$ 15,767	$ 1,493*
Deferred income	19,748	18,004	1,744
Funds	158,626	106,520	52,106
Life memberships	25,400	23,600	1,800
Unappropriated surplus—			
Balance at beginning of year	161,995	169,640	7,645*
Net income or loss for year	44,402	7,645*	52,047
	$424,445	$325,886	$98,559

* Denotes red.

Cash on deposit was satisfactorily reconciled with balances confirmed directly to us by the depositories.

The receivables of the Association were not confirmed by correspondence with debtors. Based upon the Association's past experience the reserve for doubtful accounts appears to be adequate to cover normal losses.

Changes in the investment account were verified by the examination of brokers invoices and other supporting data. Securities held at November 30, 1964, were confirmed directly to us by the State Bank and Trust Company of Evanston, Illinois, custodian for the Association and by Blunt, Ellis & Simmons, stock broker.

Funds

The receipts and expenditures during the year for the various funds and grants are detailed in Exhibit 3. Total funds available for specific projects increased to $158,626.45 at November 30, 1964.

We express our appreciation for the courtesies and cooperation extended our representatives during the course of the examination.

Very truly yours,
DAVID HIMMELBLAU & CO.
Certified Public Accountants

STATEMENT OF FINANCIAL POSITION
NOVEMBER 30, 1964

Assets

CURRENT ASSETS:		
Cash on deposit and on hand—		
State Bank and Trust Company, Evanston—		
Commercial account	$ 40,378.76	
Savings account	77,066.49	
Petty cash	60.00	$117,505.25
Receivables—		
Review advertising	$ 13,016.00	
Accrued interest and dividends	1,092.03	
Publication sales, etc.	5,353.72	
Membership dues	1,158.00	
	$ 20,619.75	
Less—Reserve for doubtful accounts.	627.66	19,992.09
Inventory of postage		208.25
Unexpired insurance and prepaid expense		2,585.44
Total current assets		$140,291.03
INVESTMENTS (at cost):		
Bonds (market value—$147,587.00)	$149,198.05	
Stocks (market value—$223,750.00)	131,247.27	280,445.32
EQUIPMENT, FURNITURE AND FIXTURES (less accumulated depreciation)		3,709.41
		$424,445.76

Liabilities, Funds and Surplus

CURRENT LIABILITIES:			
Accounts payable			$ 14,274.09
DEFERRED INCOME:			
Prepaid subscriptions		$ 13,886.26	
Prepaid dues		5,861.25	19,747.51
SPECIAL FUNDS (Exhibit 3)			158,626.45
LIFE MEMBERSHIPS AND SURPLUS			
Life memberships		$ 25,400.00	
Unappropriated surplus—			
Balance November 30, 1963	$161,995.48		
Net income for year ended Nobember 30, 1964 (Exhibit 2)	44,402.23	206,397.71	231,797.71
			$424,445.76

AUDITOR'S OPINION

Executive Committee
American Economic Association:

In our opionion, the accompanying financial statements present fairly the financial position of the American Economic Association at November 30, 1964, and the results of its operations for the year ended that date, in conformity with generally accepted accounting principles applied on a basis consistent with that of the preceding year.

Our examination was made in accordance with generally accepted auditing standards and included such tests of the accounting records and other auditing procedures as we considered necessary in the circumstances.

DAVID HIMMELBLAU & CO.

EXHIBIT 2

American Economic Association
Statement of Income and Expense
for the Year Ended November 30, 1964

Particulars			Amount	
INCOME:				
Dues—				
Regular, junior and family members			$84,483.97	
Subscribing and contributing members			9,993.00	$ 94,476.97
Investments—				
Interest on bonds and savings account			$ 6,594.26	
Dividends			6,363.83	
			$12,958.09	
Less custodian fees			357.42	12,600.67
Gain on sale of securities				10,216.03
Miscellaneous income				1,034.30
Total income				$118,327.97
EXPENSE:				
Administrative and other expense—				
Secretary's salary		$ 7,699.98		
Office salaries		29,632.83		
Addressing service income less expense		7,282.63*		
Office machine rentals		2,444.92		
Postage		3,661.13		
Stationery and printing		1,786.94		
Insurance		594.86		
Executive committee expense		1,869.87		
Other committee expense		1,132.89		
Annual meeting income less expense		8,631.00*		
Annuity payments		2,611.52		
Social security taxes		1,157.02		
Provision for depreciation		443.83		
Telephone and telegraph		669.45		
International Economic Association		400.00		
Office supplies		602.90		
Miscellaneous expense (net)		1,734.05	$40,528.56	
Publication expense—				
Printing of—				
Review		$ 53,734.79		
Directory and Handbook		12,097.21		
Proceedings		23,362.13		
Editorial honorarium		7,999.98		
Payments to contributors		1,580.00		
Editorial clerical salaries		12,023.47		
Editorial supplies and expense		2,284.88		
		$113,082.46		
Less—Publication income:				
Subscriptions other than members	$38,954.94			
Sales of copies	5,315.39			
Advertising	43,473.50			
Handbooks	7,243.92			
Reprints	397.53	95,385.28	17,697.18	58,225.74
Excess of income over expense (forward)				$ 60,102.23

Deduct		
Expenditures in excess of grant for *Index of Economic Journals* for the year ended November 30, 1964		$ 5,500.00
Appropriation for *Handbook* Supplement		8,000.00
Appropriation for Committee on Economic Education		2,500.00
Life memberships reinstated	200.00	
	$16,200.00	
Less—Life memberships written off	500.00	15,700.00
Net income to Surplus (Exhibit 1)		$ 44,402.23

* Denotes red.

EXHIBIT 3

AMERICAN ECONOMIC ASSOCIATION
STATEMENT OF SPECIAL FUNDS
FOR YEAR ENDED NOVEMBER 30, 1964

Fund	Balance November 30, 1963	Grant Received	Appropriation	Expended	Refunded to Grantor	Balance November 30, 1964
Carnegie Corporation of New York grant for travel expenses of delegates to international meetings	$ 1,578.64			$ 597.00		$ 981.64
Rockefeller Foundation grant for survey articles on recent developments	1,625.00					1,625.00
The Ford Foundation grants—						
Preparation of a special register of economists	73.73				$73.73	
Translation of books and articles of major significance into the English language	20,694.54			4,944.73		15,749.81
Preparation and publication of articles surveying economic research in foreign countries	36,688.62			10,053.24		26,635.38
Fund for *Journal of Economic Abstracts*	12,599.90	$10,000.00		15,255.77		7,344.13
Clearinghouse for Economists	33,284.93			10,994.22		22,290.71
Foreign Student Screening Project	—	83,000.00		8,409.27		74,590.73
Committee on Economic Education	—		($ 2,000.00 (B)	2,000.00		—
			500.00 (C)	132.12		367.88
Agricultural Development Council, Inc.	—	1,250.00		88.50		1,161.50
Asia Foundation	727.74	2,500.00		1,573.37		1,654.37
Committee on Publication and Research	31.62					31.62
National Science Foundation (A)	957.14*					1,982.12*
Reserve for *Handbook* Supplement	—		(D) 8,000.00			8,000.00
Sundry	172.00	100.00		96.20		175.80
Total funds (Exhibit 1)	$106,519.58	$96,850.00	$10,500.00	$54,144.42	$73.73	$158.626.45

(A) Grant of $18,600.00 for Economic section of National Roster of Scientific and Technical personnel. Expenditures are to be billed to Foundation on a monthly basis. Balance shown is due for current charges not reimbursed.
(B) $2,000 appropriated at March, 1964, Executive Committee meeting was advanced to G. L. Bach, but no report has been received as to disbursements therefrom.
(C) $500.00 for travel expense set up at president's request is to be submitted for approval at December meeting.
(D) Appropriation provides for $8,000 per year for two years, out of current income.
* Denotes red.

REPORT OF THE MANAGING EDITOR FOR THE YEAR ENDING DECEMBER 1964

The number of manuscripts received during 1964 was 431, which is more than 30 percent higher than last year's figure of 329. Table 1 gives comparative figures for the past five years.

TABLE 1

MANUSCRIPTS RECEIVED 1960–64

	1964	1963	1962	1961	1960
Manuscripts received	431	329	273	305	276
Articles	236	142	157	179	158
Communications	195	187	116	126	118
Percentage of articles accepted	9	10	15	12	16

Table 2 provides the breakdown of the volume's contents between articles, review articles, communications, book reviews, etc. No survey articles were included in 1964, which partly accounts for the reduction of pages for articles. The increase over previous years has been in the Communications Section, particularly comments and replies.

TABLE 2

SUMMARY OF CONTENTS, 1962–64

	1964		1963		1962	
	Number	Pages	Number	Pages	Number	Pages
Leading articles	19	367	19	508	18	442
Review articles	2	16			5	72
Communications:						
Original	13	95	11	66	7	65
Comments and replies	33	131	16	57	16	84
Book reviews	190	386	209	372	197	386
Classified lists:						
New books		76		71		68
Periodicals		73		74		50
Dissertations		47		39		33
Notes		46		46		52
		1,237*		1,233*		1,252*

* Plus some blank pages.

The eighth survey article, on "The Theory of Bargaining," which was scheduled for publication in 1964, was not published. The Rockefeller grant for the survey articles was terminated in October, 1964, and the remaining funds have been returned to that Foundation.

Table 3 shows the several most popular "fields" in 1964, as reflected in the subject matter of all manuscripts submitted during the year. More than 25 percent of all manuscripts fell in the three areas of monetary theory and policy, production functions, and underdeveloped economies. Other popular "fields," not shown in the table, were income distribution, cost of capital to the firm, security and financial markets, and structural unemployment.

TABLE 3
NUMBER OF MANUSCRIPTS SUBMITTED IN THE MOST POPULAR FIELDS IN 1964

Subject Matter or Field	Number of Manuscipts Submitted
Monetary and banking theory and policy	44
Production functions	40
Economics of underdeveloped countries	34
Fiscal and tax theory and policy	17
International financial problems	17
Methodology	10
Consumption, saving, or investment functions	10

Table 4 summarizes the subject-matter distribution of accepted articles, review articles, and communications for the past seven years; the figures in parentheses give the distribution for 1964 only. The most interesting figures are those for leading articles plus original communications, since to some extent these figures indicate the areas in which most work of publishable quali-

TABLE 4
SUBJECT-MATTER DISTRIBUTION OF ACCEPTED MANUSCRIPTS, 1958–64 AND 1964

	Articles	Review Articles	Original Communications	Comments; Replies	Totals
General economics	4	2	3	5 (2)	4 (2)
Price theory	25 (4)	4 (1)	18 (5)	15 (10)	62 (20)
Income theory	14 (1)	2	8	23 (2)	47 (3)
History of economic thought	4		1	2	7
Economic history; development; national economics	15 (1)	3	4	18 (2)	40 (3)
Social accounting	4 (1)	1	1	1	7 (1)
Economic systems	3			4	7
Business fluctuations	8 (1)	1	3	7 (2)	19 (3)
Money and banking	7	3	5 (1)	16 (4)	31 (5)
Public finance	2	1	8	8	19
International economics	17 (4)	3 (1)	6 (3)	14 (6)	40 (14)
Business finance	7 (2)			11 (4)	18 (6)
Business organization	7				7
Industrial organization	10 (1)		3	15 (1)	28 (2)
Land economics; housing	4 (1)	1	2		7 (1)
Labor economics	7 (1)		1	3 (1)	11 (2)
Consumption; welfare; population	8 (1)		2 (1)	5 (1)	15 (3)
Unclassified	2 (1)		8 (2)	3 (2)	13 (5)
	148 (19)	21 (2)	73 (12)	150 (37)	392 (70)

NOTE: The 1958–64 figure is followed in each case by the 1964 figure in parentheses.

ty and of broad interest to economists is being done. The five fields showing the highest concentration for the last seven years are: price theory (47), income theory (24), international economics (26), economic development, etc. (22), and money and banking (15).

Table 5 presents the expenditures in 1964 for the four regular issues of the *Review* in comparison with the estimated budget and with the actual expenditures in 1963. Actual expenditures have risen over last year owing principally to increases in printing and mailing expenses and to higher costs of editorial assistance—the former because of the increased number of pages per copy and the larger number of copies printed, and the latter because of a $2,000 salary to a new book review editor and increased expenses associated with the sharp rise in the number of manuscripts handled during the year.

TABLE 5

ACTUAL AND BUDGETED EXPENDITURES

	Budget 1964	Actual 1964	Actual 1963
Printing and Mailing	$51,000	$53,551.57	$51,644.09*
Editor's Salary	8,000	8,000.00	8,000.00
Editorial Assistance	12,500	13,460.06	9,702.06
Supplies (including postage)	1,175	1,348.23	1,165.50
Contributors	1,600	1,424.50	2,308.00
Office equipment and decoration	170	122.07	812.63
	$74,445	$77,906.43	$73,632.28
Less:			3,250.00†
			$70,382.28

* Corrected from 1963 Report.
† Less Rockefeller grant for survey articles.

Table 6 gives detailed information about printing cost by issues during 1964. The number of copies printed in 1964 averaged 18,500; in 1963, 18,050; in 1962, 16,125; in 1961, 15,650; in 1960, 15,000; in 1959, 14,125; and in 1958, 12,975. The successive increases are of course mainly related to the increases in membership and subscriptions.

TABLE 6

COPIES PRINTED, SIZE, AND COST OF PRINTING IN 1964

	Copies Printed	Pages		Costs		Cost Including Reprints
		Net	Gross	Issue	Reprints	
March	18,500	260	336	$12,516.57	$131.79	$12,648.36
June	18,500	314	360	12,583.84	137.14	12,720.98
September	18,500	386	448	15,073.62	128.61	15,202.23
December	18,500	279	360	12,850.00*	130.00*	12,980.00*
		1,239	1,504	$53,024.03	$527.54	$53,551.57

* Estimated.

The estimated costs for the coming year are presented in Table 7, based on a volume of 1,475 pages, including advertising (or about 1,200 pages of text) and an average number of copies of 19,000. A salary increase of $300 for Miss Ladd is included in the amount for editorial and clerical assistance.

TABLE 7

RECOMMENDED BUDGET FOR 1965

Printing (including paper, postage, reprints)	$53,500
Managing editor's salary	8,000
Editorial and clerical assistance	14,000
Supplies and postage	1,350
Contributors	1,425
Office equipment and decoration	100
	$78,375

During the year I have had the advice and assistance of the following foreign correspondents, who have been particularly helpful with regard to the selection of foreign books for listing and review: Maurice Flamant (France); Erich Schneider (Germany).

Three members of the Board of Editors complete their three-year terms of office at this time: Albert Hirschman, Martin Bronfenbrenner, and Roland McKean. The Association owes them a heavy debt of gratitude for the generous expenditure of time they have made in the interests of the *Review,* and I very much appreciate their constant willingness, even when other obligations pressed, to review manuscripts and offer editorial advice. I nominate as their successors on the Board, for three-year terms beginning in 1965: William J. Baumol, James Buchanan, and Richard Easterlin.

During the year I have frequently sought the aid of members of the profession in addition to the members of the Editorial Board—partly to relieve the latter of what would otherwise be an impossibly heavy burden and partly to obtain advice of specialists in particular areas not represented on the board. The following have assisted in this way:

M. Abramovitz
I. Adelman
A. Alchian
W. Allen
A. Ando
H. Averch
G. Bach
J. Bain
B. Balassa
F. Bator
W. Baumol
D. Bear
A. Bergson
E. Bloch
G. Borts
K. Boulding
H. Bruton
E. Budd
M. Burstein
R. Cameron
C. Christ
L. Cohen
J. Conard
A. Conrad
R. Cyert
P. David
P. Davidson
H. Demsetz
E. Denison
E. Despres
D. Dewey
D. Dillard
D. Durand
R. Eisner
W. Fellner
R. Ferber
F. Fisher
A. Fishlow
M. Frankel
I. Friend
W. Galenson
L. Gallaway
F. Gehrels
R. Goode
M. Gordon
R. A. Gordon
M. Gort
M. Greenhut
Z. Griliches
H. Grubel
B. Haley
D. Hamberg
L. Hansen
E. Heady
R. Heflebower
B. Hickman
R. Hinshaw
D. Hodgman
G. Horwich
B. Hoselitz

H. Houthakker
R. Jones
D. Jorgenson
A. Kahn
J. Kendrick
P. Kenen
J. Kershaw
C. Kindleberger
I. Kravis
S. Kuznets
S. Lebergott
H. Leibenstein
W. Leontief
H. Levinson
D. Lusher
G. Maddala
S. Maisel
B. Malkiel
D. Martin
E. Mansfield
J. Margolis
J. McKie
R. McKinnon
G. Meier
A. Meigs
D. Meiselman
A. Meltzer
J. Meyer
H. Miller
M. Miller
E. Mills
H. Minsky
F. Modigliani
J. Montias
T. Morgan
M. Morishima
R. Musgrave
R. Nelson
M. Nerlove
G. W. Nutter
G. Orcutt
D. Paauw
M. Peck
R. Pfouts
E. Phelps
C. Philbrook
J. Power
R. Quandt
G. Ranis
M. Reder
R. Rhomberg
M. Reid
A. Rivlin
G. Rosen
J. Rothenberg
R. Russell
A. Sametz
P. Samuelson
A. Sen
E. Shaw
M. Shubik
H. Simon
G. Sirkin
D. Slater
V. Smith
D. Snider
E. Solomon
R. Solow
H. Spiegel
G. Staller
J. Stein
G. Stolnitz
L. Tarshis
L. Telser
R. Thorn
C. Tiebout
J. Tobin
N. Ture
R. Turvey
H. Uzawa
W. Vickrey
T. Whitin
O. Williamson
S. Winter
P. Wonnacott
L. Yeager

Respectfully submitted,

JOHN G. GURLEY, *Managing Editor*

REPORT OF THE COMMITTEE ON ECONOMIC EDUCATION

The March 6, 1964, policy statement of the Executive Committee of the Association directs this Committee to move actively to develop a positive program to help improve economic understanding. This report summarizes briefly our assessment of the situation as we assume responsibility and indicates the steps which have been inaugurated since last March.

Present Situation

The past five years have seen a large increase in professional interest in the general problem of economic education, especially economic education below the college level. For over a decade, the A.E.A. has had a standing committee on economic education, which has arranged sessions at the annual meeting on this topic and which in 1959 commissioned an important analysis of economics in high school textbooks. Beginning in 1960, however, the Association made a major move in nominating members of a National Task Force on Economic Education, five well-known economists who, with two other members, became an independent group to answer the question, "What is the minimum understanding of economics essential for good citizenship and attainable by high school students?" Over 250,000 copies of *Economic Education in the Schools*, the report of the Task Force, are now in print in complete or summary form. A copy has been sent to every school system in the United States. There is considerable evidence that this document is having a significant impact on the approach used by many school administrators and instructors to the teaching of economics below the college level and on writers of textbooks and other teaching materials.

In 1962 the Association agreed to cosponsor a year-long television course, "The American Economy," shown nationwide over C.B.S. and all educational TV stations in 1962-63, and repeated on many in 1963-64. The Association approved the national teacher, and the members of the National Task Force served as a policy and advisory committee on the program, representing the interests of the Association. This program was viewed on a typical day by over one million watchers, the largest watching audience for any year-long educational television program in history. As is indicated below, a major analysis of the impact of the program has been commissioned, and a report is being prepared for publication in the *American Economic Review*, hopefully in the June, 1965, issue.

The Association played a cooperative, but less formal, role in suggesting prominent economists to participate in a Materials Evaluation Committee, established in 1961 to follow up the National Task Force report, by providing a suggested list of some 100 supplementary teaching materials that might be useful to high school teachers wishing to implement the suggestions of the Task Force. In all cases, alternative readings were suggested to avoid any implication of a "party line" or one "best" set of readings.

A special Committee on the Measurement of Economic Understanding, including five well-known economists, was established in 1963 to devise a "Test of Economic Understanding" that would, in the eyes of professional economists and of testing experts, help school officials and teachers determine the degree of economic understanding achieved by high school students (with or without a formal course in high school economics).

In addition, the Association in 1963, in connection with a Ford Foundation grant, appointed a special Publications Advisory Committee to the Joint Council on Economic Education, to advise the J.C.E.E. on the preparation of special supplementary materials on economics for use in high school courses in American history, business education, and other areas outside "economics" as such.

The combination of these activities, coupled with a widespread groundswell of popular interest in economic education, represents the largest attempt on record by professional economists, working with other interested educators and laymen, to improve the quality of economic education in the high schools. Our assessment is that considerable progress has been affected, and that more is under way. But the big part of the task remains to be done. This is the job of carrying through to the operational stage of better teaching in the classroom the new developments which have been outlined.

Committee Activities

The Committee has agreed to concentrate, at least for the time being, primarily on the teaching of economics through the high schools and the basic course in colleges, letting other activities receive attention primarily as they relate to economic education at these central levels. It may be desirable to broaden or change this focus at a later date. Given our assessment of the present situation, of the priorities where professional economists might be of assistance, and of the interests of most economists, we have initiated the following steps.

1. We have inaugurated a program to broaden and improve the contacts between competent professional economists and interested school administrators and teachers throughout the country at the state and local levels. This is in response to a widely expressed need at those levels for help from competent, objective economists as the schools attempt to determine what economics to teach, what materials to use in teaching it, what measures of success to accept, what points of emphasis to choose, and the like.

We have begun on a small scale, attempting to interest perhaps 25 or so competent economists (in addition to those already involved in such activities) who might be interested in serving as "consultants" to school administrators and teachers in their nearby areas. Our goal is partly to help the school systems involved, but also to demonstrate the usefulness and respectability of such participation by leading economists. These individuals have all agreed to devote an appreciable amount of time to getting acquainted with the practical problems of teaching economics in the schools, so as to increase the value of their assistance. We hope before next autumn to be able to write to state and local school officials in the areas concerned about the availability of

these economists, as an addition to the economic resources already available to such schools.

2. It is our assessment that many college and university faculties in economics are concerned about the effectiveness of their elementary courses, and that, while they may not have a large amount of time or energy to devote to major reconsideration of the course without some special impetus, they would welcome assistance in the form of evidence on imaginative approaches being tried in other institutions. We therefore have cooperated with the Joint Council on Economic Education in approaching the Kazanjian Foundation, and the J.C.E.E. has received a grant of $2,000 to finance the publication and distribution of a small volume including detailed accounts of perhaps a half-dozen current experiments in the teaching of basic economics which might be of interest to instructors in other schools. We hope that the sample will include course experiments at different types of institutions, since obviously teaching situations vary from institution to institution. While printing and distribution will be handled by the J.C.E.E., the design and implementation of the project, including selection of the courses to be reported, will be in the hands of this committee.

Steps are under way to assemble the half-dozen courses to be included in the first publication. Individual economists or institutions having such courses are invited to write any member of the committee about their work, since the problem of identifying the most interesting basic course experiments is a difficult one. If the Committee finds many more course experiments that it feels may be of widespread use and interest than can be included in the first volume, we will approach the Kazanjian Foundation for a grant to support a second volume a year later. The search process is under way, and we hope that the first volume will be ready for the printer before the end of 1965.

3. Under the A.E.A. policy statement, three members of this Committee serve as a Special Advisory Committee to the Joint Council on Economic Education. This subcommittee has been active in helping the J.C.E.E. recruit additional competent economists for its staff and for associated field activities. We are pleased to report that the Joint Council has added two well-known economists to its headquarters staff, and that it has welcomed our suggestions for increased stress on the economic content of the teaching materials and other activities it has been supporting. The Joint Council has under way a major $1.8 million nationwide project to help carry into the operational stage of teaching many of the economic ideas that have been developed during the five-year push described above. It is working closely with this Committee in developing and carrying out this new project.

The J.C.E.E. also has under way, with our advice, a special project to prepare economic units for inclusion in the American history course, which is taken at the eleventh grade by virtually every high school student in the United States. We have assisted the J.C.E.E. in persuading Professors Calvin Hoover and Henry Villard to undertake primary responsibility, working with high school teachers, for the preparation of these experimental materials. One set will deal with the role of the market and the developing role of government in organizing and allocating resources in the American economy, and the other

with economic growth and fluctuations. These materials are scheduled for a tryout in the high schools during the coming autumn semester before final publication soon thereafter.

4. Although this Committee has not been responsible, the profession will be interested in a special analysis commissioned by the Learning Resources Institute, with the advice of the members of the National Task Force on Economic Education, to evaluate the effectiveness of the nationwide TV course, "The American Economy." This analysis has been done by the National Opinion Research Center, under a general program mapped out by the members of the National Task Force. It has been completed and copies are available from Learning Resources Institute. A summary of the results is scheduled for publication in the *American Economic Review,* probably in June, 1965. This is the first stage in what the Committee hopes will be a major increase in the amount and quality of research in this area.

The Committee is aware of the wide diversity of views concerning the proper role of economists in economic education below the college level, and concerning the priority to be assigned to different kinds of activities. We invite the suggestions and comments of any member of the Association.

G. L. BACH, *Chairman*
MARSHALL COLBERG
RENDIGS FELS
R. A. GORDON
BEN W. LEWIS
E. T. WEILER

REPORT OF THE CENSUS ADVISORY COMMITTEE

The Census Advisory Committee of the American Economic Association met with the Director and Staff of the Census Bureau in January, 1964. The Assistant Secretary of Commerce, the Deputy Assistant Secretary of Commerce, Mr. and Mrs. Richard Ruggles, and participants in the International Statistics Training Program also joined in the meeting.

The Census staff reported on the progress of new economic statistics programs being developed at the Bureau: (1) development of a historical establishment record computer file from the Annual Survey and Census of Manufactures, 1949-63, for economic analysis purposes; (2) a study of the variation in prices of new houses over the period 1959-61, for the purpose of developing a construction price index; (3) a statistical compilation of annual series measuring economic growth, to supplement the Business Cycle Developments report; (4) conversion of the present import and export commodities classification systems to one which will be compatible with the Standard Industrial Classification and the Standard International Trade Classification (scheduled for completion by early 1965); (5) experimentation in developing annual population and income estimates for standard metropolitan statistical areas through use of Internal Revenue Service individual income tax return data. It was mentioned by the Census Staff that the Bureau programs were properly being directed to the future, as well as to current, needs of the American economy. Committee members commented on these programs and suggested continued work in these and related fields.

The Bureau staff also revealed several new developments in the use of electronic equipment to increase efficiency and output. In addition to the new and more powerful 1107 computers, the Bureau also uses an electronic data-plotter to plot graphs directly from computer tape; a telephone device to transmit data on punch cards from the Jeffersonville Census office directly to a computer tape in Suitland; and a machine to convert microfilm to tape and tape to microfilm.

The major topic of the meeting was the Committee members' discussion of the need for making unpublished Census Bureau data more accessible to the outside researcher. The Bureau now provides special tabulations of such data on a reimbursable cost basis. Some Committee members felt that these costs, while low relative to commercial rates, are often too high for the typical university researcher. One suggestion, already under study by the Bureau, is the creation of regional Census data centers at various universities, each having complete and corrected files of Census source data tapes. This approach, the Committee felt, would reduce some of the costs involved and increase the analytical flexibility of the research project.

There are major problems facing the Bureau, however, in the establishment of such regional data centers. These problems include: (*a*) the need to preserve confidentiality of data collected by the Bureau, in compliance with the law; and (*b*) the high costs involved in providing sufficiently complete and

corrected tapes, in the proper format and sequence, for efficient use by university and other researchers. Nevertheless, the Bureau staff will continue in its efforts to search for solutions to this general problem of data accessibility.

The Technical Subcommittee on Business Cycle Developments of the Census Advisory Committee met with the Director and Staff of the Census Bureau in May, 1964. Also participating in the meeting were the Assistant Secretary of Commerce, the Director of the Office of Printing and Publications of the Department of Commerce, the Director of the Office of Publications of the Department of Commerce, and a representative of the Bureau of the Budget.

The main focus of the discussion, naturally, was on ways and means of improving *Business Cycle Developments.*

Members of the Census Advisory Committee are:

	Term Expires June 30
Morris A. Adelman	1965
Daniel Creamer	1968
Edward F. Denison	1965
Solomon Fabricant (Chairman)	1968
Bert G. Hickman	1966
Werner Z. Hirsch	1965
Edgar M. Hoover	1965
H. Gregg Lewis	1968
John Lintner	1968
Sherman J. Maisel	1967
Robert R. Nathan	1965
Arthur M. Okun	1968
Guy H. Orcutt	1966
Ralph W. Pfouts	1967
Gideon Rosenbluth	1967

Members of the Technical Subcommittee on Business Cycle Developments are:

Donald J. Daly	1967
Gottfried Haberler	1966
Bert G. Hickman (Chairman)	1966
Lawrence R. Klein	1967
John P. Lewis	1965
Geoffrey H. Moore	1968
Frank E. Morris	1965
Arthur M. Okun	1968
Beryl W. Sprinkel	1967
Lorman C. Trueblood	1966

SOLOMON FABRICANT, *Chairman*

REPORT ON THE FOREIGN STUDENT SCREENING PROJECT

In September the Association started a new project aimed at assisting the graduate departments in the selection of foreign students. The Ford Foundation has granted the Association $83,000 to support the program for five years; additional financial assistance is being sought from the Department of State.

The program is still in the planning stage. Tentatively the following activities are scheduled for the first year: (1) publication of a bulletin of information describing the graduate programs offered at universities in the United States in economics and agricultural economics; (2) compilation of information on departments of economics in foreign universities, including the character of their programs, the level and quality of the work offered in economics, and their systems of measuring and recording academic attainment; (3) arrangements for the interviewing of foreign applicants in their own countries by persons familiar with the requirements of American graduate programs in economics and able to judge students' qualifications; (4) provision of a clearinghouse of information on applicants. Detailed information on these programs has already been sent to the chairmen of those departments with sizable enrollment of foreign students; the remainder of the chairmen will be contacted early in 1965.

Negotiations are now under way with the Office of Education in the Department of Health, Education and Welfare to determine their interest in compiling and publishing the bulletin of information on American graduate departments.

Respectfully submitted,
MILLARD F. LONG, *Director*

REPORT OF THE REPRESENTATIVE TO THE NATIONAL RESEARCH COUNCIL-NATIONAL ACADEMY OF SCIENCES

During the last two years I have taken an active part in the work of the Behavioral Sciences Division of the National Research Council and have served on its Executive Committee. The Behavioral Sciences Division was organized only two years ago, inheriting the work of its predecessor division which was limited chiefly to the fields of physical anthropology and psychology. Therefore, the major task of the new division has been to organize itself and to extend its activities into the fields of economics, political science, sociology, and the nonphysicial aspects of anthropology and psychology. We have discussed many possibilities but have agreed on only one major enterprise. This enterprise is to sponsor a symposium on the contributions that the various behavioral sciences can make to the understanding of the process of economic development. The Council of the National Academy of Sciences has approved this undertaking and we are at present in the process of seeking funds for financing it and of appointing directors to organize the planned symposium.

The division also has submitted to the Committee on Public Policy of the National Academy of Sciences a report on the needs of the behavioral sciences for support of basic research. This report is to be transmitted to the Daddario Committee of the House of Representatives for consideration by the appropriate subcommittees of the House. I contributed the section of the report dealing with the needs of economics.

Respectfully submitted,
ROBERT DORFMAN

REPORT OF THE REPRESENTATIVE TO THE INTERNATIONAL ECONOMIC ASSOCIATION

In 1964 the International Economic Association held three conferences:

1. A conference on price formation in various economies was held at Jerusalem, Israel, in March. This was the annual conference arranged for younger economists. Participants from the United States were L. R. Klein and J. Margolis.

2. A conference on the distribution of national income was held at Palermo, Italy, in September in connection with the annual meeting of the I.E.A. Executive Committee. Participants from the United States were A. A. Alchian, M. Bronfenbrenner, B. F. Haley, M. W. Reder, and R. M. Solow.

3. A conference on problems of economic development of the socialist countries of East Europe, a regional conference, was held at Plovdiv, Bulgaria, in the first week of December.

Other conferences in preparation are one on the economic problems of housing, for younger economists, to be held in the spring of 1965, with Sherman Maisel as chairman of the program committee; and a conference on the economics of agriculture to be held at Rome, Italy, in September, 1965, in connection with the meeting of the I.E.A. Council.

Three volumes resulting from earlier conferences have appeared during the past year: *The Economics of Take Off into Sustained Growth*, *Economic Development with Special Reference to East Asia*, and *Economic Development of Africa South of the Sahara*. Four further volumes are in press.

The Association has also been working on two projects which arise out of special contracts with UNESCO: a bibliography for the teaching of economics in Africa and Asia, and a textbook for the teaching of economics in Africa.

B. F. HALEY

REPORT OF REPRESENTATIVE TO THE NATIONAL BUREAU OF ECONOMIC RESEARCH

The current program of the National Bureau includes research in five areas: Economic Growth; National Income, Consumption, and Capital Formation; Business Cycles; Financial Institutions and Processes; and International Economic Relations. Seventeen reports resulting from the National Bureau's program were published in 1964 and eleven books, staff reports, and conference proceedings are scheduled for publication by mid-1965.

Conferences on the following subjects are scheduled for early 1965: Industrial Composition of Income and Product (April 9-10); Investment Behavior (June 10-12); Measurement and Interpretation of Job Vacancies (February 11-13).

New research in process and planned for early inauguration includes studies dealing with the following areas: the competitive position of the United States in a changing world economy; tax policies for economic growth; effect of education on individual earnings and economic growth; productivity and employment in the service industries and their relation to the nation's growth; interest rates—structure, behavior, and linkages in the money and capital markets; short-term economic forecasting—appraisal and possibilities of improvement.

At the 1964 Annual Meeting, Charles G. Mortimer was elected a Member and Director at Large to fill the unexpired term of Theodore V. Houser, who died December 17, 1963. W. Allen Wallis was reelected a Member and Director by Appointment of the American Statistical Association.

Officers elected for 1964 were Albert J. Hettinger, Jr., Chairman; Arthur F. Burns, President; Frank W. Fetter, Vice President; Donald B. Woodward, Treasurer; Solomon Fabricant, Director of Research; Geoffrey H. Moore and Hal B. Lary, Associate Directors of Research; and William J. Carson, Executive Director and Secretary.

Expenditures and appropriations for the National Bureau's operations in 1964 totaled approximately $1,443,000. Income was received mainly from interest and dividends on capital sum grants, grants for specific projects, contributions and subscriptions, and sales of publications. New grants were received in 1964 from the National Science Foundation, Office of Manpower, Automation and Training, Department of Labor, and the Alfred P. Sloan Foundation.

Effective September 1, 1964, Columbia University Press became the distributor of the National Bureau's books published prior to 1953 and since September 1, 1964, and all Occasional Papers and Technical Papers. Princeton University Press is the publisher and distributor of the books published by the National Bureau between 1953 and September 1, 1964.

WILLARD L. THORP

REPORT OF THE POLICY AND ADVISORY BOARD OF THE ECONOMICS INSTITUTE INSTITUTE OF INTERNATIONAL EDUCATION

The Seventh Session of the Economics Institute was held at the University of Colorado from June 25 to August 26, 1964. The purpose of the Institute is to provide orientation and additional training for foreign students who are about to begin graduate training in economics at United States universities.

The 1964 Institute had a total enrollment of sixty-one students from twenty-six different countries. This is the largest number of students in its history. Six students were from each of the following countries: India, Indonesia, and Thailand, five each from Japan and Mexico, four each from the United Arab Republic and Colombia, three each from Brazil and the Ryukyu Islands, two each from Bolivia and Greece, and one each from sixteen outher countries.

Upon conclusion of the Institute the students proceeded for graduate study of economics at twenty-six different United States universities, with the largest numbers being enrolled in Vanderbilt (8), Wisconsin (8), Colorado (7), Yale (4), Chicago (3), Minnesota (3), and Williams (3). The age range of the students was from twenty-one to thirty-nine with a median age of twenty-eight and a modal age of twenty-five.

The program of the Economics Institute emphasizes economic analysis with training in this area being supplemented where necessary by work in oral and written English and in mathematics and statistics. The Director of the Institute is Wyn P. Owen, of the University of Colorado, and the staff is drawn from all parts of the country.

A summary of costs of the Economics Institute for 1958-1964 follows.

SIMON ROTTENBERG, *Acting Chairman*

SUMMARY OF COSTS OF THE ECONOMICS INSTITUTE 1958–64

YEAR	NUMBER OF STUDENTS	EXPENSES: Administrative Expense*	EXPENSES: Other Expense†	EXPENSES: Total Expense	EXPENSES: Total Expense per Student
1958	36	$12,395	$ 35,451	$ 47,846	$1,329
1959	49	8,453	45,745	54,198	1,106
1960	46	6,326	47,781	54,107	1,176
1961	53	4,509	52,778	57,287	1,081
1962	50	3,000	57,704	60,704	1,214
1963	55	11,160	67,535	78,695	1,413
1964	60	9,225‡	72,249‡	81,474‡	1,358‡
Totals	349	$55,068‡	$379,243‡	$434,311‡	
Average expense per student					$1,239‡

* Administrative expense consists of off-Campus charges, paid directly by I.I.E.: administrative overhead, pre- and post-Institute orientation and travel, Policy Board costs, and other direct charges. Starting expenses made the first year's outlay expecially large. There was no meeting of the Board in 1962 which lowered costs for that year. Staff changes at I.I.E. plus approximately $2,000 in Policy Board costs accounted for the large increase in this item for 1963.

† On-Campus charges.

‡ Estimate: All figures are rounded to the nearest dollar.

REPORT OF THE COMMITTEE ON RESEARCH AND PUBLICATIONS

In the "Reading Series" a new volume on business cycles (edited by Lawrence R. Klein and R. A. Gordon) was sent to the publishers toward the end of 1964. The volume on welfare economics (Kenneth Arrow and Tibor Scitovsky, editors) is expected to be ready for publication in the coming year. Volumes were commissioned in international trade (Harry G. Johnson and Richard Caves, editors) and in agricultural economics (Gale Johnson and Karl Fox, editors).

The three-year grant from the Ford Foundation was renewed to support translations of foreign economics until June, 1967. The Foundation also agreed to transfer to this project the unexpended balance of its grant for the surveys of foreign economics. New translations that have been commissioned during the year include Galiani's *Della Moneta* and Tugan-Baranovsky's *Russian Factory*. The Committee expects to commission a translation of Pareto's *Manuel* shortly. It invites further suggestions from the profession concerning important foreign works that ought to be translated into English, both from those who would like to see such translations available and from those who would be willing to prepare them.

At the request of George Hildebrand, Chairman of the Committee on Surveys of Foreign Economics, the President of the Association, George Stigler, transferred further responsibility for work in this area to the Committee on Research and Publications. The Richard D. Irwin, Inc., has expressed its willingness to publish in volume form the five surveys of foreign economics that were distributed as supplements to the *American Economic Review*, provided that the Association turn over the plates to Irwin without charge. The Committee recommends to the Executive Committee that the offer be accepted.

The Committee on Research and Publications recommends to the Executive Committee of the Association that a second series of surveys on foreign economics be sponsored. The new series might include some non-English language not covered in the first series, such as Latin America, Sweden, and the centrally planned economics of Eastern Europe. It would, however, be primarily addressed to surveys of policy-oriented work in economics in other English-speaking countries such as Canada and Australia.

The Committee carried on some preliminary negotiations with prospective publishers of the translation series. It recommends to the Executive Committee that one of the offers made to publish the translation series without cost to the Association be accepted.

IRVING B. KRAVIS, *Chairman*
KARL FOX
GEORGE HILDEBRAND
BERT F. HOSELITZ
HARRY G. JOHNSON
FRANCO MODIGLIANI
HAROLD F. WILLIAMSON

COMMITTEE ON HONORS AND AWARDS

A Proposal for Broader Recognition of Distinction

The Association now accords recognition of professional distinction of the highest order to its most eminent members by (1) election to the presidency and (2) award of the Walker medal. (It similarly recognizes professional distinction in the case of foreign economists by election of not more than twenty-five honorary members.) At its spring 1964 meeting, the Executive Committee accepted the view that, given the present size of the profession, the annual election of a president and the quinquennial award of the Walker Medal are inadequate means for recognition of professional distinction; and it requested the Committee on Honors and Awards to prepare a proposal for the election of a designated number of honorary members from its U. S. membership. The following is the Committee's proposal:

1. Bylaw 1, Membership, Paragraph 3 now reads: "Foreign economists of distinction, not exceeding twenty-five in number, may be elected honorary members of the Association." It is proposed to amend this paragraph by adding:

Past presidents of the Association and members who have been awarded the Walker Medal shall also be honorary members. In addition, the Executive Committee may elect up to a total of twelve additional honorary members, but not more than two in any one calendar year, from economists of high distinction in the United States.

2. The responsibility for nominating to the Executive Committee U. S. candidates for honorary membership should rest with the Nominating Committee. This committee should, however, give consideration to recommendations, if any, made by the Committee on Honors and Awards each year.

3. The Nominating Committee should be free to make no nominations in any particular year. However, it should not be limited as to the number of candidates it may nominate in any year. It should accompany each nomination with a supporting brief.

4. The Executive Committee should not add to the list of nominees recommended by the Nominating Committee. Regardless of the number of nominees so recommended, the Executive Committee may elect no more than two per year in the new category; and the total number of U. S. honorary members, in addition to past presidents of the Association and holders of the Walker Medal, must not exceed 12.

5. Election to honorary membership of U. S. economists does not preclude election to any office of the Association.

Comments

1. Three members of the Committee on Honors and Awards, in considering the above set of proposals, favored giving the responsibility for nominating U. S. honorary members to the Nominating Committee; one member favored giving the responsibility to the Committee on Honors and Awards; no member favored giving it to the committee that nominates foreign honorary members. The Nominating Committee is most likely to be informed as to presidential possibilities that have to be passed by; the Committee on Honors and Awards

has the advantage of greater continuity as well as that of having periodically reviewed possible candidates for the Walker Medal. On the other hand, the committee on foreign honorary members deals with an entirely different population, and its members should be chosen with primary regard for their knowledge of the contributions of foreign economists. If the Nominating Committee is given primary responsibility and if the Committee on Honors and Awards is permitted or invited to submit suggestions to the former committee, the recommendations coming to the Executive Committee should be the product of adequate advance study.

2. In our proposal, election is limited to U. S. economists. (By U. S. economist we mean economists ordinarily resident in the United States, but not excluding those temporarily resident abroad.) The Executive Committee should consider, however, whether eligibility should be broadened to include Canadian economists. The fact that our Association often includes Canadian economists in the slate of nominees for various offices suggests that such a broadening of eligibility might be appropriate. In that event, the words "and Canada" should be added to the last sentence of the proposed bylaw amendment. Of the membership of the Committee on Honors and Awards, two favored limiting the new category of honorary members to U. S. economists; three would recommend that leading Canadian economists (or perhaps the Canadian membership of our Association) should be consulted on the matter, and their wishes followed.

3. It is desirable that the honor of election in the new category of honorary members should be regarded as very close to that of election to the presidency.

a) This is the reason for limiting the total number of elected U. S. honorary members to 12 and the maximum number elected in any one year to 2. (The five members of the Committee on Honors and Awards that participated in the preparation of these proposals were unanimous in their support of these two figures.) The number of foreign honoray members is currently 19, but is potentially 25; the number of past presidents is currently 21; the two living holders of the Walker Medal are also past presidents. So the potential number of honorary members, if the present proposal is adopted, would currently be 58—25 foreign and 33 U. S.

b) The importance of making election to the new category of honorary members a high honor is also one reason for including all past presidents and all holders of the Walker Medal in honorary membership. Another reason for including past presidents is to make it clear that election to honorary membership is no obstacle to later election to the presidency—although there should be no presumption that this would be likely to occur.

4. The program, if adopted, should be reviewed after a trial period of, say, ten years.

5. These proposals have been prepared by the Committee on Honors and Awards at the request of the Executive Committee. The latter Committee should be reminded that, of the six members of the Committee on Honors and Awards, only three favor the creation of the new category of honorary members.

BERNARD F. HALEY, *Chairman*

ECONOMISTS' SALARIES IN 1964

A First Report and an Invitation from the Committee on the N.S.F. Report on the Profession

The median salary of some 10,000 full-time employed respondents with a specialization in the field of economics was $12,000 in 1964 (Table 1). The average (median) salary of the economists so employed was the same as those of statisticians and physicists and was considerably higher than those of any of the other nine disciplines included in the 1964 National Register. Data for this study are based on questionnaires accumulated by the American Economic Association for the National Register of Scientific and Technical Personnel, a program of the National Science Foundation. These salaries were reported to the nearest $100. Data reported here were for those individuals who indicated economics as their area of greatest competence, based on total education and work experience. They do not necessarily reflect the

TABLE 1

Distribution of Basic Annual Salaries of Scientific and Technical Personnel, by Field of Specialization, 1964

Field of Employment	Salary in Thousands of Dollars					Number of Respondents in Thousands	
	Median	Lowest Decile	Lower Quartile	Upper Quartile	Highest Decile	Full-time Employed Reporting Salary*	Total
Social sciences							
Economics	12.0	7.8	9.3	16.0	20.0	10.0	12.1
Statistics	12.0	8.0	9.5	14.9	17.2	2.5	2.8
Psychology	10.3	7.3	8.5	12.9	16.3	14.0	16.8
Sociology	10.1	7.3	8.5	13.0	16.0	2.3	2.7
Linguistics	9.0	5.8	7.2	11.8	15.0	1.0	1.4
Physical sciences							
Physics	12.0	7.4	9.0	15.0	18.7	20.2	26.7
Chemistry	11.0	7.2	8.7	14.0	17.5	52.2	63.1
Mathematics	11.0	7.0	8.7	14.7	18.5	14.7	17.4
Biology	10.7	6.5	8.3	14.5	19.0	21.8	27.1
Meteorology	10.6	7.8	9.0	12.9	15.5	2.9	5.5
Earth sciences	10.3	7.1	8.5	13.0	17.0	13.6	17.9
Agriculture	9.2	6.2	7.5	11.4	14.0	8.5	9.5
Other surveyed fields	11.1	7.0	8.6	15.0	18.5	17.2	20.8
All surveyed fields	11.0	7.1	8.6	14.0	18.0	181.0	223.9

* Of the 223,854 respondents, 42,844 were excluded representing military, part-time employed, students, etc.

Source: National Register of Scientific and Technical Personnel, 1964.

salaries of those who were trained as economists or who were currently employed as economists or who regarded themselves professionally as economists.

The superior median salary of the economists was influenced considerably by the relatively large amounts earned at the higher end of their salary distribution. Thus, salaries of $16,000 or more were paid to one-quarter of the economists, whereas the upper quartile amount, even among the physicists and statisticians, was about $1,000 lower. The highest paid one-tenth of the economists had salaries which compared even more favorably with the salaries at the top deciles of the other scientific and technical groups. At the lower end of the salary distributions, economists also fared relatively well, but the contrast was much less striking than that for the better paid groups in each discipline. Salaries of $7,800 or less were earned by one-tenth of the economists and salaries of $9,300 or less were earned by one-quarter of them.

Those economists employed by educational institutions earned less than economists in other areas of employment but earned more than the academic salaries of most of the other social or physical scientists (Table 2). In the colleges and universities, the median economists' salary of $10,100 was

TABLE 2

MEDIAN BASIC ANNUAL SALARIES OF SCIENTIFIC AND TECHNICAL PERSONNEL IN EDUCATION, FEDERAL GOVERNMENT, AND INDUSTRY, BY FIELD SPECIALIZATION, 1964

Field of Specialization	Median Salaries in Thousands				Number of Respondents in Thousands			
	All Reported Employers	Educational Institutions	Federal Government	Industry and Business	All Reported Employers*	Educational Institutions	Federal Government	Industry and Business
Social sciences								
Economics.....	12.0	10.1	13.7	14.4	9.9	4.5	1.2	3.3
Statistics......	12.0	10.4	13.0	12.0	2.5	0.6	0.6	1.0
Psychology....	10.3	9.7	12.0	14.1	14.0	7.4	1.3	1.2
Sociology......	10.1	10.0	12.9	14.0	2.3	1.9	0.1	0.1
Linguistics.....	9.0	9.0	10.7	12.0	1.0	0.8	0.1	0.1
Physical sciences								
Physics.......	12.0	9.6	12.1	13.5	20.2	7.9	2.8	8.5
Chemistry.....	11.0	9.3	10.8	11.7	52.1	9.2	3.9	36.1
Mathematics...	11.0	8.7	12.1	13.0	14.7	5.9	1.1	6.6
Biology.......	10.7	10.0	11.0	12.5	21.8	13.4	2.8	2.4
Meteorology...	10.6	10.5	10.6	11.0	2.9	0.3	1.8	0.5
Earth sciences.	10.3	8.8	11.0	11.0	13.6	2.9	2.2	7.3
Agriculture....	9.2	10.2	9.3	9.0	8.5	2.4	3.2	1.3
Other surveyed fields..........	11.1	8.3	12.1	12.0	17.2	4.2	1.5	10.3
All surveyed fields	11.0	9.6	11.0	12.0	180.7	61.3	22.6	78.8

* Reporting both full-time salary and type of employer including the following types of employers, in addition to the three types specifically shown: state and local government, military and public health services, nonprofit organizations, self-employed, and others.

SOURCE: National Register of Scientific and Technical Personnel, 1964.

slightly below that of those in the fields of meteorology, statistics, and agriculture but above the medians for seven other disciplines reported. It should be noted that the salaries paid by educational institutions are those reported, whether on an academic- or calendar-year basis. The fact that educational institutions frequently pay basic salaries for an academic year of 9-10 months doubtless helps to account for the relatively lower median salaries from educational institutions of those in each of the fields of specialization.

Taking all types of employment together, economists' salaries not only ranked near the top in educational institutions but also were the highest reported in civilian government service and private business. The federal government alone employed nearly one-eighth of the economists reporting full-time salaries, and the $13,700 median salary of these government economists was above that of the other professional groups by amounts ranging from $700 to $4,400 per year. Private industry and business employed one-third of the reporting economists as compared with two-fifths of the statisticians and much smaller proportions of the other social science groups. In private industry, the economists' median salary of $14,400 was closely comparable to those for psychologists and sociologists but was from $900 to $5,400 above the median salaries of the other nine professional groups.

The highest educational degrees of those in the entire group of 12,143 respondents who recorded their greatest competence as being in economics were as follows:

Degree	Number
Ph.D.	5,091
Masters	4,204
Bachelors	2,613
Other	92
No report	143
Total	12,143

The major types of the current (1964) employers of the 12,143 economists, as here defined, were represented as follows:

Employer	Number	Employer	Number
Educational institutions	5,061	Self-employed	195
Industry and business	3,967	Military; public health	83
Federal government	1,274	Other employers	207
Nonprofit organizations	465	Employer types not reported	97
State and local government	284	Not employed	510
		Total	12,143

The primary work activities of the 12,143 economists were reported by them in broad groupings as follows:

Management or administration		3,534
Of research and development	1,288	
Other management or administration	2,246	
Teaching		3,469
Research and development:		1,927
Applied research	1,296	
Basic research	584	
Production and inspection		1,215
Other work activities		1,091
Work activity not reported		397
Not employed		510
Total		12,143

The count of 12,143 persons with greatest professional competence in economics resulted from a screening of the responses to questionnaires distributed, not only by the American Economic Association, but also by the other professional societies which cooperated in the 1964 National Register. The response from those questionnaires distributed by the A.E.A. was approximately 70 percent.

These limited observations on the salaries and characteristics of economists are reported here, in advance of any detailed analysis of the data to be made available, for two reasons: (1) To call timely attention to salary data available for the first time for economists and other social scientists as of 1964. (2) To invite suggestions as to the types of analysis of these data which members of the American Economic Association believe might be especially useful or revealing.

The appended list of selected items on the National Register questionnaire indicates the rich variety of possible types of cross-tabulation and analysis which might be undertaken. Of course, any attempt to deal critically with all of the possible combinations of these data would result in a report of unwieldy size and questionable utility.

A limited supply of an eight-page report by the National Science Foundation, *Reviews of Data on Science Resources,* "Salaries and Professional Characteristics of U.S. Scientists, 1964," *NSF 64-27*, Vol. I, No. 2, December, 1964, containing comparable and more detailed data for twelve disciplines may be obtained from the office of the Association or may be purchased from the Superintendent of Documents, U.S. Government Printing Office (5 cents a copy).

The undersigned have been appointed by the A.E.A. as a committee to work with the National Science Foundation in analyzing and presenting the data on economists from the 1964 National Register. Any suggestions and expressions of particular interests should be addressed to the Committee's chairman, N. Arnold Tolles (264 Ives Hall, N. Y. S. School of Industrial and Labor Relations, Cornell University, Ithaca, New York 14850).

Respectfully submitted,
N. ARNOLD TOLLES, *Chairman*
EWAN CLAGUE
ALICE HANSON JONES

List of Selected Items on the 1964 National Register Questionnaire

1. Date of birth
2. State or country of birth
3. State or country of secondary school graduation
4. Sex
5. Citizenship
6. Respondent's classification (15 categories, such as: economics, sociology, chemistry, etc.)

For Each Earned Collegiate Degree:

7. Level of degree
8. Name of college, university or other institution
9. Year degree granted
10. Major subject
11. Minor subject
12. Current employment status (9 categories, such as: full-time professionally employed; student, part-time employed; not employed; retired; etc.)

Principal Current Employment

13. Metropolitan area and state
14. Type of employer (14 categories, such as: private industry or business; self-employed; nonprofit organizations; etc.)
15. Employment specialty—under Economics:
    (*a*) *Major groups:* (1) General Economic Theory, (2) Economic History; History of Thought, (3) Economic Systems: Development and Planning, (4) Economic Statistics, (5) Monetary and Fiscal Theory and Institutions, (6) International Economics, (7) Business Finance and Administration; Marketing and Accounting, (8) Industrial Organizations; Government and Business; Industry Studies, (9) Land Economics, (10) Labor Economics, (11) Population; Welfare Programs; Standards of Living, (12) Economics, Other
    (*b*) *Detailed Specialties:* Under major groups 1 to 11 above, a total of 60 subareas, such as: economic fluctuations, macroeconomic theory, microeconomic theory, etc.
16. Primary and secondary professional work activity (10 categories, such as: management or administration of research and development; basic research, applied research, etc.—If "teaching," academic rank.)
17. Current work sponsored by U. S. government funds (If "yes," which of 10 programs, such as: agriculture, atomic energy, etc.)
18. Basic annual salary (If academically employed, 9-10 or 11-12 month periods distinguished.)
19. Estimated gross annual professional income
20. Years of professional experience
21. Specialized competence (1 to 4 specialties in decreasing order of competence, based on respondent's evaluation of his own "total educational and work experience." For list of specialties, see item 15, above.)
22. Specialized knowledge of foreign areas (name of country)
23. Professional society memberships (17 categories, such as: American Economic Association, American Farm Economic Association, etc.)

REPORT OF REPRESENTATIVE TO THE SOCIAL SCIENCE RESEARCH COUNCIL

The main new development of interest is a revision in Council policy affecting research training fellowships and faculty research grants. Both of these programs have been in operation for nearly forty years. Over the last decade many new nationwide programs in the same field have been developed with quite generous financial support, and these have been supplemented by a number of specialized programs as well, some of which have originated from within the Council itself. In result, the Board of Directors of the S.S.R.C. decided that the two programs should be reformulated with the intention of concentrating efforts upon grants to a smaller number of unusually talented persons who show real promise of breaking new research ground in the social sciences and who are unable to obtain support from other channels. This change of emphasis will permit the Council to make larger grants than it has in the past and to concentrate these awards upon individuals that show real promise of developing original ideas.

GEORGE H. HILDEBRAND

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Supplement (No. 2—May).—Sixty-third Annual Meeting:

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Supplement.—Supplement to 1948 Directory. Pp. 17.

Volume XLII, 1952

The American Economic Review, March, June, September, and December

Supplement (No. 2—May).—Sixty-fourth Annual Meeting:

Papers and Proceedings. Economic Theory and Public Policy; Issues in Methodology; Business Cycle Theory; Monetary Theory; Fiscal Theory; International Trade Theory; Value Theory; Recent Developments in United States Monetary Policy; Inflation Control in the United States; American Foreign Aid Programs; International Trade in the Postwar World; Wages, Manpower, and Rearmament; Governmental Policy on Business Practices; Economic Problems of Military Mobilization; Theoretical Analysis of Economic Growth; General Factors in Economic Growth in the United States; Growth in Underdeveloped Countries; Role of War in American Economic Development; Public Utilities, Transportation, and Spatial Organization; Collective Bargaining in the Regulated Industries. Pp. 768.

Supplement.—Supplement to 1948 Directory. Pp. 11.

Volume XLIII, 1953

The American Economic Review, March, June, September, and December

Supplement (No. 2—May).—Sixty-fifth Annual Meeting:

Papers and Proceedings: Monetary Policy; A Stock-taking of Bretton Woods Objectives; Underdeveloped Countries—The Theory and Practice of Technical Assistance; United States Demand for Imports; Interregional Analysis and Regional Development; Governmental Economic Activity; Development of Economic Thought; Technology; Economics in the Curricula of Agricultural Colleges in Canada and the United States; Sociology and Economics; Recent Developments in Mathematical Economics and Econometrics; Research; Long-run Effects of Full Employment on the Labor Market; Theory of Income Distribution; Distribution of Government Burdens and Benefits; Distribution and Utilization of Natural Gas. Pp. 612.

Supplement No. 2—Graduate Education in Economics. Pp. 223.

Supplement No. 3—Handbook. Pp. 187.

Volume XLIV, 1954

The American Economic Review, March, June, September, and December

Supplement (No. 2—May).—Sixty-sixth Annual Meeting:

Papers and Proceedings: Fundamental Characteristics of the American Economy; An Appraisal of Economic Change; Factor Markets Versus Product Markets; Farm Prices and Farm Incomes in American Agriculture; Industrial Pricing; Technological Progress and Economic Institutions; Growth Decisions in the American Economy; Diminishing Inequality in Personal Income Distribution; Wage Determination in the American Economy; Alternative Possibilities of Inflationary Pressures and Higher Cost Bottlenecks in an Economy of Large Bargaining Units and of Less Than Pure and Perfect Competition in the Marketing of Products; Regional Wage Differentials in an Economy of Large Bargaining Units and Less Than Pure and Perfect Competition in the Marketing of Products; Automaticity of Full Employment Under the Assumption of Diminished Defense Expenditures; Institutional Aspects of Savings and Investment; Role of Corporate Taxation in the American Economy; Theory of International Trade in a World of Trade Barriers and Controls and of Variegated National Economic Systems; Economic Doctrines Implied in the Reports of the U.N. and IBRD on Underdeveloped Countries; Corporate International Investment Policies and Programs; Economic Implications of an Aging Population; Report on Graduate Training in Economics; Economics in General Education; Economic and Regulatory Problems in the Broadcasting Industry; National Transportation Policy, Pp. 765.

Supplement.—Supplement to 1953 Handbook. Pp. 11.

Volume XLV, 1955

The American Economic Review, March, June, September, and December

Supplement (No. 2—May).—Sixty-seventh Annual Meeting:

Papers and Proceedings: International Flow of Economic Ideas; Development Policy in Underdeveloped Countries; Economic Development—Case Studies; Regional Economics; Urbanization and Industrialization of the Labor Force in a Developing Economy; Models of Economic Growth; Current Problems in Agricultural Economics; Long-term Trends in International Trade; Economic Research and Public Policy; Economic Stabilization, Forecasting, and the Political Process; Cyclical Experience in the Postwar Period; Debt Management and Monetary Policy; Taxation and Income Distribution; Concepts of Competition and Monopoly; Impact of Antitrust Laws; Research on the Business Firm; Price and Wage Flexibility; Pricing in Transportation and Public Utilities. Pp. 711.

Supplement.—Supplement to 1953 Handbook. Pp. 8.

Volume XLVI, 1956

The American Economic Review, March, June, September, and December

Supplement (No. 2—May).—Sixty-eighth Annual Meeting:

Papers and Proceedings: Statement of the Problem of Keeping the U.S. Economy Moving Forward, But Steadily; Production and Consumption Economics of Economic Growth; Income Distribution Aspects of Expanding Production and Consumption; Increase of Consumption Part of Economic Growth; Government Expenditures and Economic Growth; Monetary Role in Balanced Economic Growth; Shortening Work Week as a Component of Economic Growth; Highway Development and Financing; Unemployment as a Phase of Economic Growth; Urban Growth and Development; Population Specter—Rapidly Declining Death Rates in Densely Populated Countries; Current Economic Thought and Its Application and Methodology in Continental Europe; Economic Thought and Its Application and Methodology in the East; Economic Potentials of Latin America; Economic Potentials of Africa; Report of the Attorney General's Committee on Antitrust Policy; Changing Patterns of Competition in Transportation and Other Utility Lines; Economics in the Curricula of Schools of Business. Pp. 651.

Volume XLVII, 1957

The American Economic Review, March, June, September, and December

Supplement (No. 2—May).—Sixty-ninth Annual Meeting:

Papers and Proceedings: Institutional Economics; Economic Growth and

Development; Keynesian Economics after Twenty Years; Employment Act in the Economic Thinking of Our Times; Business Fluctuations and Inflation; Government Taxing and Spending; Developments in Banking and Monetary Policy; Price and Competitive Aspects of the Distributive Trades; Monopoly Problem as Seen by Social Scientists; Impact of Some New Developments in Economic Theory; Economics and Changing Technology; Power and Public Utility Problems; Transportation Problems; Social Security and Welfare; Income Distribution; Consumers in the American Economy; International Economics; Soviet Economic Developments; Economics in Schools. Pp. 754.

Supplement (No. 4—July).—Handbook. Pp. 522.†

† This issue is not included in the price of the volume.

Volume XLVIII, 1958

The American Economic Review, March, June, September, and December

Supplement (No. 2—May).—Seventieth Annual Meeting:

Papers and Proceedings: Veblen Centenary Round Table; Democracy and Trade-Unionism; Agenda for a National Monetary Commission; Is Another Major Business Contraction Likely; Monetary Analysis and the Flow of Funds; Economic Projections and a Statistical Contribution to Price Theory; Statistical Cost Functions; Trends in Capital Investment and Capacity; Income and Consumption; State and Local Public Finance; Agricultural Parity; Further Explorations in Monopolistic-Competitive Price Theory; Petroleum and Natural Gas and the Public Interest; Critical Evaluation of Public Regulation of Independent Commissions; Current Economic Questions Relating to Western Europe; Measuring Production in the U.S.S.R.; Selected Papers—American Economic Association Competition. Pp. 677.

Supplement.—Supplement to 1956 Handbook. Pp. 32.

Volume XLIX, 1959

The American Economic Review, March, June, **September, and December; each $1.50

Supplement (No. 2—May).—Seventy-first Annual Meeting:

Papers and Proceedings: Soviet Economic Trends and Prospects; Soviet Economic Planning; Non-Russian Communist Economies; Fundamentals of Economic Progress in Underdeveloped Countries; Special Problems Facing Underdeveloped Countries; Role and Character of Foreign Aid; International Trade and Payments in an Era of Coexistence; Maintaining Full Employment and Economic Stability; Balanced Economic Growth in History—A Critique; Economics of Government Expenditures; Power Blocs and the Operation of Economic Forces; Administered Prices Reconsidered; Studies in the Classical Economics; Selected Problems in Economic Theory; Open Competition; Organization and Financing of Economic Research; Market for Economists. Pp. 689. 3.00

Volume L, 1960

The American Economic Review, **March, June, **September, and December; each 1.50

Supplement (No. 2—May).—Seventy-second Annual Meeting:

Papers and Proceedings: Standards for the Performance of Our Economic System; Relations Between Economic Theory and Economic Policy; Incentives and Economic Growth: Changing Roles and Public Policies; Problems of Achieving and Maintaining: a High Rate of Economic Growth, Full Employment, and a Stable Price Level; Problem of Raising Incomes in Lagging Sectors of the Economy; Problem of International Harmony: Economic Policies for a Lasting Peace; Problem of Social Priorities; Investing in Education and Research; Facilitating Movements of Labor Out of Agriculture; Reducing Impediments to Foreign Trade; Reforming the Tax System; Improving the Efficiency of the Transportation and Utilities Systems; Research: on Theory of the Firm, on Income, Consumption, and Savings, and on Economic Development. Pp. 745. 3.00

Volume LI, 1961

The American Economic Review, **March, June, September, and **December; each 1.50

Supplement (No. 2—May).—Seventy-third Annual Meeting:
Papers and Proceedings: Monetary Theory—New and Old Looks; Mac economic Theories of Income Distribution; Capital Theory; Manage Economics—A New Frontier; Frontiers in Uncertainty Theory—The E dence of Futures Markets; Distribution Costs—Concepts and Measur Antitrust Problems; Economic Analysis of Urban Problems; Public Utili and Transportation; Wheat—A Permanent Need for a Farm Progra Problems of Economic Instability in Other Countries; Balance of Payme of the United States—Problems and Prospects; Economics and Natio Security; Economic Development in Mainland China; Influence of Mc and Social Responsibility on Economic Behavior; Economic Educatio Challenge to Our Profession. Pp. 675.

Volume LII, 1962

The American Economic Review, **March, June, September, and December; e
**Supplement (No. 2—May).—Seventy-fourth Annual Meeting:
Papers and Proceedings: New Developments in the Theory of the Fir Problems of Economic Development: Lagging U.S. Growth Rate; Int national Transmission of Business Cycles—Problems and Policies; Sov Economic Planning; Economics of Research and Development; Studies Business Behavior; Economic Behavior of Families; Reappraisal of Doctrine of Consumer Sovereignty; Report of the Commission on Mo and Credit; Tax Problems; Systems of Economic Accounts and Anal for Urban Regions; Role of Transportation in Economic Developme Transportation Problems in the American Economy; Economics of Wa Resource Use; Teaching of Economics; Market for Economists. Pp. 615.

Volume LIII, 1963
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Supplement (No. 2—May).—Seventy-fifth Annual Meeting:
Papers and Proceedings: Richard T. Ely Lecture; Public Policies w Respect to Private Business; International Commodity Stabilization; C ditions of International Monetary Equilibrium; Problems of Regio Integration; Problems of Methodology; Topics in Economic Theo Industrial Capacity; Tax Reform; Financial Institutions and Monet Policy—A Re-examination of Their Interrelationship; Defense and I armament; Pricing and Resource Allocation in Transportation and Pul Utilities; Postwar Growth in the United States in the Light of the Lo swing Hypothesis; Economic Trends and Prospects in the U.S.S.R. Eastern Europe; Japanese Economic Development; Economic Developm and the Population Problem; Economic Education. Pp. 753.
Supplement No. 2.—Economics in the Schools. Pp. 27.

Volume LIV, 1964

The American Economic Review, March, June, September, and December; e
Supplement (No. 1—January).—Handbook. Pp. 464.†
Supplement (No. 3—May).—Seventy-sixth Annual Meeting:
Papers and Proceedings: Richard T. Ely Lecture; Theory of Monopoli Competition after Thirty Years; Principles of Efficiency; Efficiency in A culture; Efficiency in the Labor Markets; Regulated Industries; Efficie in the Use of Natural Resources; Efficiency in the Government Sect Problems in Taxation; Financial Factors in Business Cycles; Reapprai in American Economic History; Comparative Costs and Economic De opment; Canadian-American Economic Relations—Efficiency and In pendence; Efficiency of the Soviet Economy; Invited Dissertations, I II; Efficiency in the Teaching of Economics. Pp. 698.
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Supplement No. 3—Surveys of Foreign Postwar Developments in Econo Thought. Pp. 101.

† This issue is not included in the price of the volume.

Volume LV, 1965

The American Economic Review, March, June, September, and December; each $2.00
Supplement (No. 2—May).—Seventy-seventh Annual Meeting:
Papers and Proceedings: Richard T. Ely Lecture; Economic Growth—International Comparisons; Comparative Economic Systems—Nationalized Industry; Economic History—Its Contribution to Economic Education, Research, and Policy; Contribution of the History of Economic Thought to the Understanding of Economic Theory, Economic History, and the History of Economic Policy; Evolving International Monetary Mechanism; Domestic Implications of the Evolving International Monetary Mechanism; Survey Research—Three Surveys, Findings and Implications for Theory and Policy; Recent Capital and Production Theory; Technological Change—Stimuli, Constraints, Returns; New National Econometric Model—Its Application; Invited Student Dissertations; Defense Economics—Applying Economic Criteria; Nonmarket Decision Making; Economic Theory and Nonprofit Enterprise; Economics of Poverty; Economic Education—Experiments in the Teaching of Economics. Pp. 652. 4.00

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